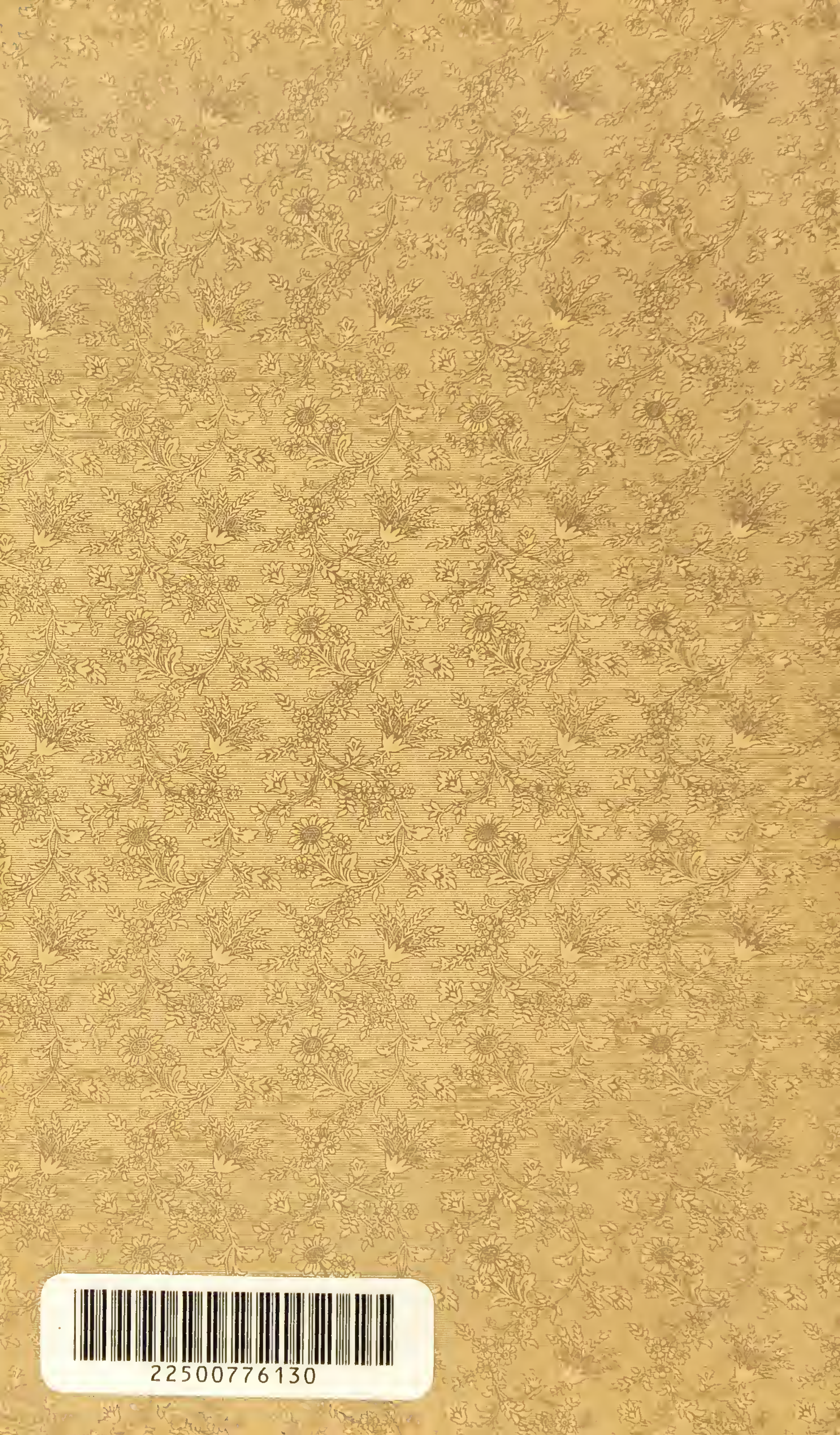
BRISBANE, QUEENSLAND.

SEPTEMBER, 1899.



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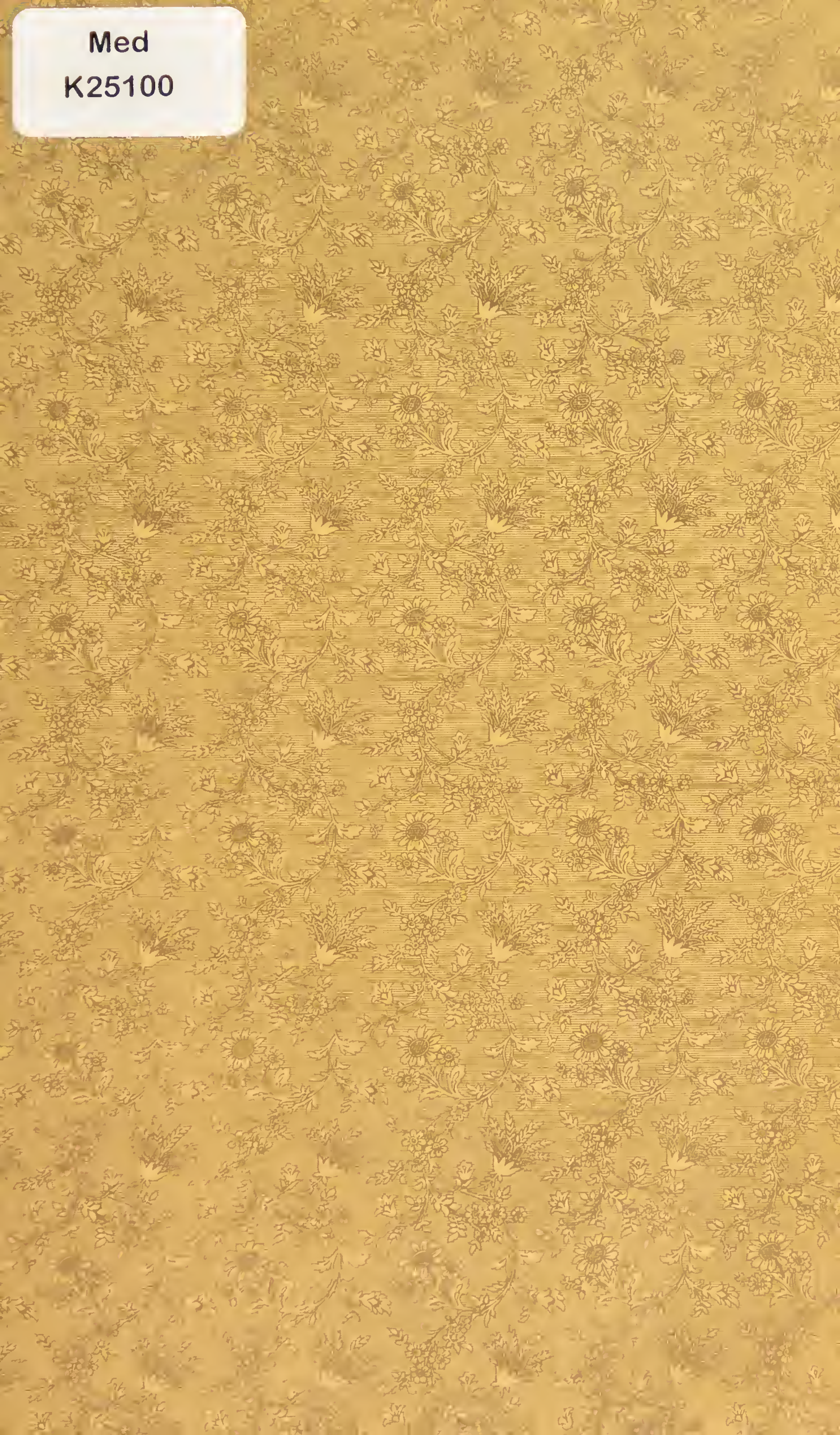

INTERCOLONIAL MEDICAL CONGRESS
OF AUSTRALASIA.

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TRANSACTIONS

OF THE

FIFTH SESSION

HELD IN

BRISBANE, QUEENSLAND,

SEPTEMBER, 1899.

Published under the direction of the Literary Committee.

BY WILTON LOVE, M.B.

Brisbane:
BY AUTHORITY: E. GREGORY, GOVERNMENT PRINTER WILLIAM STREET.
1901.

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PREFACE.

In issuing this volume of the Transactions of the Fifth Session of the Intercolonial Medical Congress of Australasia, the Literary Committee have to express their regret at the delay, which has been caused by circumstances outside their immediate control.

The Committee wish to place on record their indebtedness to the liberality of the Queensland Government, who have undertaken the whole expense of printing and binding the Transactions.

The delay referred to has been caused by an unusual press of work in the Government Printing Office—no less than five Royal Commissions and two Enquiries having monopolised the time and type of the office. In consequence of the repeated delays, it has been thought advisable by the Committee not to further retard the issue by sending proofs to authors; but it is hoped that the care which has been spent on the text will, to a large extent, have obviated the necessity of that course.

As a compensatory measure, each author has been provided with twenty-five extra copies of his paper or papers without charge.

The Committee have further to express their obligation to the Brisbane Press, particularly the *Brisbane Courier*, *Telegraph*, and *Observer*, for the reports of meetings, etc., which have been largely availed of in the compiling of the Transactions.

The various discussions were entrusted to special shorthand reporters, and, though the *ipsissima verba* of each speaker may not have been reproduced, it is hoped that the gist of the discussions has been given with a fair measure of accuracy.

JOHN THOMSON, M.B.,
President.

ADDENDA ET CORRIGENDA.

Page 58, line 20, *for* ("Newcastle") *read* ("Dunedin").

Page 62, line 48, *for* "Drs. Gregory and Byrne" *read* "Drs. Gregory Sprott and Byrne."

Page 214, line 8, *for* "post rectus" *read* "posterior rectal sheath."

Page 390, line 45, *for* "Dr. Florian King" *read* "Dr. Florian Krug."

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Vice-Presidents - T. K. Hamilton, M.D. (Dubl.), F.R.C.S.I.
A. L. Kenny, M.B., Ch.B. (Melb.)
A. J. Brady, L.R.C.S.I., L.K.Q.C.P.I.
W. Odillo Maher, M.D. (Q.U.I.)
F. Wallace Mackenzie, M.B., C.M. (Edin.)
C. E. Barnard, M.D. (Aberd.), M.R.C.S. (Eng.)
Joint Secretaries - J. Lockhart Gibson, M.D. (Edin.)
H. A. Francis, M.B. (Camb.)

Section 4.

MIDWIFERY AND GYNÆCOLOGY.

President - - M. U. O'Sullivan, L.R.C.P. and S.E.
Vice-Presidents A. A. Hamilton, M.B., Ch.B. (Dubl.)
J. A. G. Hamilton, M.B. (Dubl.), L.R.C.S. (Edin.)
G. Rothwell Adam, M.B., C.M. (Edin.)
E. T. Thring, F.R.C.S. (Eng.), L.R.C.P. (Lond.)
T. M. Hocken, M.R.C.S. (Eng.)
J. E. Wolfhagen, M.B., C.M. (Edin.)
Secretary - - W. S. O. Byrne, M.D. (Dubl.)

Section 5.

PUBLIC HEALTH.

INCLUDING STATE MEDICINE, PSYCHOLOGICAL MEDICINE, AND DEMOGRAPHY.

President - - J. Ashburton Thompson. M.D. (Brux.) M.R.C.S., L.R.C.S., D.P.H. (Camb.)
Vice-Presidents T. Borthwick, M.D., C.M. (Edin.)
J. V. McCreery, L.R.C.S.I.
Eric Sinclair, M.D. (Glasg.)
F. Ogston, M.D., Ch.M. (Aberd.)
T. Hope Lewis, M.R.C.S. (Eng.), L.S.A. (Lond.)
E. J. Crouch, M.R.C.S. (Eng.), L.S.A. (Lond.)
Joint Secretaries - J. B. Hogg, L.R.C.P. and S. (Edin.)
A. B. Brockway, M.R.C.S., L.R.C.P.

Section 6.

ANATOMY, PHYSIOLOGY, PATHOLOGY, AND PHARMACOLOGY.

Vice-Presidents R. H. Marten, M.B. (Camb.), M.R.C.S. (Eng.)
Professor C. J. Martin M.B., D.Sc. (Lond.)
Professor J. T. Wilson, M.B., C.M. (Edin.)
W. S. W. Roberts, M.R.C.S. (Eng.)
G. H. Hogg, M.B., C.M. (Edin.)
Secretary - - Jas. Atkin Wheeler, M.B. (Lond.)

List of Papers Contributed to the Congress.

Section 1.

MEDICINE.

1. Presidential Address.
   R. Scot Skirving, M.B., C.M. (Edin.)
2. Diphtheria.
   J. MacDonald Gill, M.D. (Lond.)
3. Complicating Infections.
   A. C. F. Halford, M.D. (Melb.)
4. Œdema in Infants not of Renal origin.
   W. Atkinson Wood, M. D. (Melb.)
5. Acute Yellow Atrophy.
   A. Jarvie Hood, M.B. (Glasg.)
6. Amœbic Abscess and Amœbic Dysentery in Australia.
   J. L. T. Isbister, M.B. (Adel.)
7. Brain Fever of the Drama.
   T. Hope Lewis, M.R.C.S., L.S.A.
8. Notes of a case of Primary Omental Carcinoma.
   J. R. Purdy, M.B. (Aberd.)
9. Excretion of Water and Urea in case of patient with one Kidney.
   J. B. Nash, M.B. (Edin.)
10. An Epidemic of Ptomaine Poisoning.
    S. J. Richard, M.B. (Syd.)
11. Clinical aspects of various Diseases found at Port Darwin.
    F. Goldsmith, M.B. (Adel.)
12. (1) Influence of Hæmorrhage on the course of Pulmonary Phthisis.
    E. Hirschfeld, M.D. (Strasb.)
13. Treatment of Diphtheria.
    A. J. Turner, M.D. (Lond.)
14. Origin of Sex.
    Arthur F. Davenport, M.B. (Lond.)
15. Bubonic Plague: One of the earliest epidemics, 1141 B.C.
    Frank Tidswell, M.D. (Syd.), D.P.H. (Camb.)
    J. Adam Dick, M.D. (Edin.)
16. Some Notes on Empyema.
    G. T. Howard, M.D., Ch.B. (Melb.)
17. The Clinical Value of the Serum Reaction in Typhoid, together with some notes on the use of Dead Cultures, and on Anti-Typhoid Inoculations.
    W. H. Goode, M.D.
    J. L. T. Isbister, M.B., Ch.B. (Adel.)
18. Discussion on the Treatment of Typhoid Fever—Introduced by
    F. W. E. Hare, M.D. (Durh.)

Section 2.

SURGERY.

1. Operative Treatment of Inguinal Hernia.
   Perineal and Suprapubic Cystotomy.
   H. V. Critchley Hinder, M.B. (Syd.)
2. The Treatment of Hydatids by a Modification of the Closed Method.
   J. Ramsay, M.B. (Melb.)
3. Cystitis.
   Partial Thyroidectomy for Graves' Disease.
   Alfred Lendon, M.D. (Lond.)
4. The Anatomy and Treatment of Talipes Equino-varus (Congenital).
   W. Kent Hughes, M.B. (Lond.), M.R.C.S. (Eng.)
5. Appendicitis.
   The Surgical Treatment of Perityphlitis.
   C. P. B. Clubbe, M.R.C.S. (Eng.)

GENERAL DISCUSSION ON APPENDICITIS.

6. Recurrent Carcinoma of the Breast treated with Thyroid Extract.
   Jas. P. Ryan, M.K.Q.C.P.I.
7. Some Observations on Renal Surgery.
   L. E. Barnett, M.B., (Edin.), F.R.C.S. (Eng.)
8. A case of extensive malignant disease with unusual prolongation of life.
   C. E. Todd, Adelaide, M.D. (Brux.), M.R.C.S. (Eng.)
9. Five Cases of Vasectomy.
   Wax Models of Bladder, Urethral and Kidney Diseases.
   P. C. Fenwick, M.B. (Lond.)
10. The Operative Treatment of Hydatids situated low in the Pelvis.
    The Treatment of Severely Crushed Limbs.
    W. Moore, M.B., Ch.M. (Melb.)
11. Three Years' Experience with the X Rays (Illustrated by Lantern Slides).
    F. J. Clendinnen, M.D. (Brux.)
12. Aseptic Bradawls for Bone.
    Pedicle Needle.
    Intrauterine Cotton Carrier.
    Modification of Jordan Lloyd's Scalpel—Shown by
    T. Hope Lewis, M.R.C.S., L.S.A.
13. The Etiology and Treatment of Inguinal Hernia in the Young.
    R. Hamilton Russell, F.R.C.S. (Eng.)
14. The Prevalence and Ætiology of Cancer of the Stomach.
    C. L. W. Hunt, M.B. (Syd.)
15. The Excretion of Urinary Water and Urea from the Human Body by Diminished Kidney Weight.
    J. B. Nash, M.D. (Edin.)
16. A Case of Allantoic Cyst.
    A successful Case of direct injection of Strychnine into the Left Ventricle of the Heart in a patient apparently moribund during Anæsthesia.
    R. D. Pinnock, M.D. (Glasg.)
17. Case of Traumatic Separation of the Epiphysis (with skiagrams).
    Review of Hydatid Disease.
    Robert Scott, M.B. (Glasg.)
18. Notes of a Case of Sarcoma of Kidney—Nephrectomy.
    Notes of a Case of Perforation of the Vermiform Appendix—General Peritonitis—Operation—Recovery.
    G. Herbert Hopkins, F.R.C.S. (Eng.)

Section 3.

EYE, EAR, NOSE, AND THROAT.

1. Eyesight in School Children under Colonial Conditions.
   H. Lindo Ferguson, M.D. (T.C.D.) F.R.C.S.I.
2. A Case of Retinitis Circinata.
   J. P. Ryan, L.K.Q.C.P., and L.R.C.S.I.
3. Muscular Insufficiency.
   F. Antill Pockley, M.D. (Edin.)
4. Retinoscopy, &c.
   Mark J. Symons, M.D. (Edin.)
5. Bifocal Lenses.
   W. Kent Hughes, M.B. (Lond.)
6. Discussion on Examination of Railway Servants and Mariners.
7. Ocular Symptoms and Signs of Locomotor Ataxia.
   H. W. Martindale Kendall, L.R.C.P. and S. (Edin.)
8. Case of Segmental Hemichromatopsia.
   F. Antill Pockley, M.D. (Edin.)
9. Strabismus.
   A. W. Orr, M.D. (Dubl.)
10. Two Cases of Subhyaloid Hæmorrhage.
    J. Lockhart Gibson, M.D. (Edin.)
11. Monocular Diplopia.
    J. Lockhart Gibson, M.D. (Edin.)
12. Cicatricial Contraction of Orbit.
    T. K. Hamilton, M.D. (Dubl.)
13. Secondary Cataract.
    T. K. Hamilton, M.D. (Dubl.)
14. The Advantages of the Prone Position in Operating for Adenoids.
    W. Kent Hughes, M.B. (Lond.)
15. Post Nasal Adenoid Growths as Factors in the Production of Morbid Changes and the Indications for their Removal.
    C. A. Parker, F.R.C.S. (Edin.)
16. McEwan's Burrs.
    A. J. Brady, L.R.C.S.I., &c.
17. Aural Vertigo.
    J. Lockhart Gibson, M.D. (Edin.)
18. Two Cases of Retro-nasopharyngeal Abscess.
    J. Lockhart Gibson, M.D. (Edin.)
19. Reflex Nasal Neuroses.
    G. H. Hogg, M.B., C.M. (Edin.)
20. Notes on Preventable Blindness.
    C. E. Barnard, M.D. (Aberd.)
21. Quinine Amaurosis.
    T. S. Kirkland, M.D. (Glasg.)
22. Conical Cornea.
    H. Lindo Ferguson, M.D. (T.C.D.)

Section 4.

MIDWIFERY AND GYNÆCOLOGY.

1. Presidential Address.
   M. U. O'Sullivan, L.R.C.P. and S. (Edin.)
3. Pseudo Diphtheritic Vaginitis and Endometritis, with notes of four cases.
   F. A. Nyulasy, M.B. (Melb.)

5. (1) Conservative Gynæcology.
   (2) Contribution to the simplification of Hysterectomy.
   J. Foreman, M.R.C.S., L.S.A.
6. Surgical Treatment of Uterine Fibro-myomata.
   E. T. Thring, F.R.C.S., Eng.
7. (1) The treatment of Uterine Myomata.
   (2) Notes of four cases in which pregnancy followed Ventrifixation of Uterus.
   Ralph Worrall, M.D. (Dubl.)
8. Serum-Therapy in fever after delivery.
   E. Fairfax Ross, M.R.C.S., Eng., M.D. (Brux.)
10. Two cases of Uterus Bicornis.
    R. D. Pinnock, M.D. (Glasg.)
11. Cancer of Cervix Uteri from a preventive point of view.
    Constance Stone, M.D. (Toronto).
12. Uterine deviations.
    J. A. G. Hamilton, M.B. (Dubl.)
13. Saving of blood in Gynæcological operations.
    Professor Watson, M.D. (Paris), F.R.C.S. (Edin.)
14. Record of Eight Years' Midwifery Practice in New Zealand.
    Digital Dilatation of Os Uteri in Labour.
    J. R. Purdy, M.B. (Aberd.)
15. Electricity in Gynæcology.
    C. H. Haines, M.D. (Dubl.)
16. Complete case of Hermaphroditism.
    A. F. Davenport, M.B. (Lond.)
17. Rupture in Early Ectopic Gestation.
    W. S. Byrne, M.D. (Dubl.)
18. Post-Partum Hæmorrhage.
    E. L. Luther, B.A., M.D., B. Ch. (Dubl.)
19. Tetanus in relation to Climatology and its occurrence in the Puerperium.

Section 5.

PUBLIC HEALTH.

1. Isolation of Infectious Diseases by Local Authorities.
   Gregory Sprott, M.D. (Glasg.)
   Discussion opened by Dr. Wilton Love.
2. The Diphtheria Mortality of the three principal Australian Colonies for the past fifteen years, with Special Reference to the Effect of Antitoxin on the Death Rate.
   A. Jefferis Turner, M.D. (Lond.)
3. The Fertility of the Unfit.
   W. A. Chapple, M.D. (N.Z.)
4. The Treatment of Phthisis in Western Queensland.
   Guy L'Estrange, L.R.C.S.I.
5. Some Effects of Environment upon Racial Development.
   R. S. Berry, M.R.C.S., L.R.C.P.
6. Rib Fracture in the Insane.
   J. F. Flashman, M.D. (Syd.)
7. The Use and Abuse of Hospitals in Queensland.
   A. C. Halford, M.D. (Melb.)
8. Reform in Quarantine.
   W. L'Estrange Eames, M.B. (Dubl.)

9. A Plea for Legislation to Empower Medical Practitioners to make Post Mortem Examinations in Cases of Doubtful Diagnosis.
R. D. Pinnock, M.D. (Glasg.)

10. Sewerage and Health.
Mailler Kendall, L.R.C.P. and S., Ed.
Discussion opened by J. Espie Dods, M.B. (Edin.)

11. The Sanatorium Treatment of Consumption.
G. Lane Mullins, M.D. (Dubl.)
Discussion opened by Dr. Horrocks.

12. Summer Colds, and Summer Dress in Australia.
Frank Tratman, M.D. (Lond.)

13. Preventable Diseases.
T. W. Garde, L.R.C.P. and S., Ed.

14. Remarks on the Australian Army Medical Services.
W. L'Estrange Eames, M.B. (Dubl.)

Section 6.

ANATOMY, PHYSIOLOGY, PATHOLOGY, AND PHARMACOLOGY.

In this Section a number of Photo-Micrographs, by Dr. F. Tidswell, were exhibited.

A demonstration of Pathological and Bacteriological subjects was given by C. J. Pound, F.R.M.S., daily in the Museum.

Church Services.

SUNDAY, 17TH SEPTEMBER, 1899.

FORENOON.

ST. JOHN'S CATHEDRAL.

The Right Rev. J. F. **Stretch**, D.D., Bishop-Coadjutor of Brisbane, preached to a very large congregation, including His Excellency the Governor and Lady Lamington, Mr. Le Hunte (Lieutenant-Governor of New Guinea), and the President and many members of the Intercolonial Medical Congress, a sermon entitled—

The Final Sovereignty of Law.

"We know in part and we prophesy in part. But when that which is perfect is come, then that which is in part shall be done away."—1 Cor. xiii. 9, 10.

This is certainly the age of congresses. Travelling has been made easy and rapid, and it is natural that men interested in a common work should desire to meet and take counsel. It would, of course, be easy to question the value of this, to contrast the results of the laboratory or study with those of the conference or platform to the disparagement of the latter. And the comparison, though specious, would probably be quite unfair. Each has its place. Knowledge is generous. It loves to impart itself. It is both natural and admirable that in so many branches of human activity, both sacred and secular, congresses have become the rule.

THE MEDICAL CONGRESS.

This week a very notable gathering of the members of the Medical Association takes place in our city. In England it has become the custom that a sermon should be preached in connection with the opening of the medical congress, at which the members are officially present. In doing so, I can hardly do better than take as my motto the words of St. Paul, which at once teach perseverance and modesty to us all—perseverance, for knowledge is possible; modesty, for the possible knowledge is, after all, partial; a lesson that is true both for theologian and scientist, and full of encouragement and warning for both. Side by side with it may I venture to place as a professional commentary the dictum of the great John Hunter, "Don't think—try. Be patient; be accurate."

A HEARTY WELCOME.

As Churchmen, we bid you welcome in the name of the Lord. No Christian can ever fail in reverence for the noble profession of medicine when he recalls the gracious presence and healing touch of the Son of God, as He moved through our sad world, and when he remembers that while to the wayward and erring the best-loved title of the Master is the Good Shepherd, to the maimed and suffering He is ever the Good Physician, Who "Himself took our infirmities and bore our diseases." We see in you an army, sworn to do battle against disease, banded together, not to destroy men's lives but to save—to lessen, not to increase, the sum of human misery. We echo and endorse the noble words of Lord Rosebery, spoken last month at Epsom College:—"The medical profession calls out all the qualities of manhood. Tenderness and sympathy are so essentially needed in the physician that, if he is not the patient's most welcome sight, he is by that fact a failure. He is always fighting a life-long contest with the Angel of Death; and he knows that, struggle as he may, the Angel of Death must win in the end. The profession of the physician is one long struggle in which, like the Englishman, he must never know when he is beaten until victory has gone against him."

Of this tenderness and sympathy and generous devotion to the cause of suffering humanity, every one of us has had many evidences.

That your profession is a school of the heroic we have had many proofs.

We have been told how on the battlefield the work of healing has been carried on as coolly as in the hospital theatre, while again and again the call of duty has hurried men, regardless of the personal risk, to plague spots of infection in the hope of discovering some remedy. And I think history will pronounce that one of the most heroic records of modern times is the pathetic narrative of those lonely deaths of physician and nurse, from infection by the bacillus of the plague, where the physician, self-isolated, calmly noted his own symptoms till it was no longer possible to observe and to record.

You have a very lofty ideal set before you in your profession, and on its roll are inscribed names that rank very high among the world's heroes, and speak trumpet-tongued to the world of the essential nobility of the life of duty.

REASONS FOR THANKFULNESS.

Surely a Congress such as this is a call to us to stop and reflect how much we owe to the skill and knowledge of a profession the thoughtless sometimes allow themselves to jest about. A recent writer in the "Nineteenth Century" says that medicine has made more advance in the last sixty years then in the previous sixty centuries. He says that the two great outstanding advances of that period are anæsthesia and antisepsis. How many of us realise to-day that little more that fifty years ago chloroform was unknown. James Young Simpson, practically the discoverer of chloroform, was in his student days so appalled by the hideous suffering caused by an operation that he nearly abandoned the thought of pursuing the study of medicine. Think of the gulf which divides the horror of necessary operations, performed without anything to deaden pain, from the present conditions, and then reflect on this one ground among many for gratitude to the profession of medicine.

The second great name which he quotes is that of Lister, and he declares him to be so epoch-making that surgery may be divided as "before Lister" and "after Lister." This is no place to quote statistics to show what we owe to the modern antiseptic treatment of wounds. Enough to say that even the layman can see it as a revolution; that operations previously considered impossible have become possible; while the death-rate after operation, in some cases formerly appalling, is constantly diminishing. Nor must we forget the great and unselfish efforts of the profession in the direction of the prevention of disease. Read Charles Kingsley's "Two Years Ago," and see what cholera was like in England. It has been unknown there for years. Only recently there has been a congress held in Berlin on the subject of tuberculosis, and the whole tone of the discussion was most hopeful and encouraging. Its ravages are being checked, and much success is looked for from the new preventive measures. Or take again the use of antitoxins, a quite recent development. One illustration only: What disease is more dreaded for its sudden and deadly assault than diphtheria? By the use of its antitoxin its fatality has in Paris been reduced nearly four-fifths. And further, investigation seems to be on right lines. All nations join hands in these discoveries. A Japanese scientist discovers the plague bacillus. The world is awake and expectant. We hardly know what fresh surprise may be in store for us. And for all this we thank God, the Source of all wisdom.

We must remember, too, the gallant struggle of the profession against insanitary conditions, in which there is so much to discourage. The medical expert has to carry his full share of the "white man's burden." So-called civilised nations can be savage enough where their pocket is concerned. And so it is needful—

By open speech and simple,
  An hundred times made plain:
To seek another's profit,
  And work another's gain.

So, too, you will have at times to see your wisest schemes brought to naught by outbursts of folly, such as the illogical revolt against vaccination, by which civilised and educated England is preparing for herself disaster. We have had several terrible illustrations lately of what I may, perhaps, call national atavism, as humiliating as unexpected.

And when your goal is nearest,
(The end for others sought,)
Watch sloth and heathen folly
Bring all your hope to naught.

While meeting in this city you will be able sadly to reflect that, with a population of 100,000 people, we are guilty of the "sloth and heathen folly" of dispensing with any real system of drainage, and contenting ourselves with a water supply repeatedly condemned as unwholesome. You fight against the vested interests of selfishness, backed up by the deplorable lethargy of ignorance.

WE CLAIM YOU AS FELLOW-WORKERS.

In all this we, as Churchmen, find a certain partnership. We also claim to be on the side of law. We believe that sin is lawlessness. We believe that the seat of law is the bosom of God; that health and holiness should run on parallel lines; that we ought to be fellow-servants of the living God. And yet it would be idle to claim that this is to-day the commonly accepted relationship of medicine to religion, of the physician to the parson. No doubt, such partnership still exists in many cases. The annual service of the Guild of St. Luke, in St. Paul's Cathedral in London, shows how powerful and how mutually helpful such an alliance can be. But very often the modern physician sits very light to his church, and in some cases adopts towards religion generally a somewhat aloof and superior attitude. Perhaps it is a symptom—a passing symptom, I hope—of the distrustful attitude that the modern man of culture assumes to religion. It seems to him too vague for scientific analysis, and he prefers an agnostic attitude. He may even accuse the Church, meaning thereby certain Churchmen, of an obscurantist attitude to science. This may be true. At times we may have forgotten my text and been unduly positive. But do scientific teachers never err in the same way? You cannot afford to stand aloof. In this, too, John Hunter's dictum is sometimes the truest attitude—"Don't think—try; be patient, be accurate." All knowledge, says one, begins with an experiment, and ends with an experience. Humility is a strictly scientific virtue. "Knowledge puffeth up; love buildeth up." It is all the difference between a balloon and a cathedral. Without religion we should be left in this strange contradiction: that whereas we expect each part of the universe to be scientific, and look with confidence to be able to co-ordinate the facts of science, we should have to allow that the universe as a whole was unscientific, that its facts would never be co-ordinated, but would remain a growing heap of fragments, growing, that is, till energy being dissipated, all was again swept into the chaos of nothingness. But is this reasonable? Has Lord Rosebery said the final word when he asserts that all your science is after all only a losing battle against death and extinction? Even if it were so, then we should still wish to cast our little fragment of knowledge on the heap. But it would be a cheerless prospect after all. Is it not exactly here that religion comes in, and at once satisfies an imperious instinct, and transfigures human effort with the radiance of Divine hopefulness? Bishop Lightfoot says, "Death had, till the resurrection of Christ, been the chief foe of humanity—the one paramount, intolerable, ultimate evil with which man must wrestle, though only with the absolute certainty of defeat. But in Christ, Death himself was conquered. Through darkness to light, through sorrow to joy, through suffering to bliss, through evil to good—this is the law of our Heavenly Father's Government, whereby He would educate His sons and daughters into the likeness of His own perfection."

Come, come, it's best believing, if we may,
You can't but own that.

And we may. Nay, if we would be truly scientific, we must. There is room in our life for personality, for effort, for knowledge. We do know in part. And our partial knowledge slowly grows into system. So above all, in the eternal order, there is room for personality, for mind, for will, for knowledge, for love.

On the earth the broken arcs; in the heaven, a perfect round.

The Eternal Father knows absolutely. God is the Absolute, the Universal Scientist; with us the partial, but the true; with Him the perfect, the Truth. The mystery of suffering, of pain, of death, of what Winwood Read calls the martyrdom of man—all these are rendered tolerable for us by the Cross of Christ. And that is not God's final word. God is, and He is on the side of law. He is making all things new. He makes for health, as he does for holiness. Gifts of healing are the gifts of His Spirit. It is your own loss if your profession is not your vocation, your calling from God. You are fellow-workers with God. Behind us and above us and around us, the source of all the scattered rays of fragmentary knowledge that stream upon us, is the full white light of the eternal, radiant truth. Our vision is not yet strong and practised enough to look directly at it. But *the perfect is*. It is no mere dream, no vanishing mirage which cheats our hopes, and sends us back to settle down into the pursuit of the partial, into the eternal gathering of fragments, for ever condemned to remain fragmentary. The perfect comes, and then the partial is—not lost, nor wasted, but—done away, because, having served its purpose, and trained and strengthened our vision, it fades in the radiance of the Greater Glory.

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EVENING.

ST. PAUL'S PRESBYTERIAN CHURCH.

The Rev. T. Nisbet preached a special sermon to a crowded congregation, amongst whom were His Excellency the Governor, Mr. Le Hunte (Lieutenant-Governor of New Guinea), and the President and many members of Congress. The subject of the sermon was—

A Doctor of the Old School,

one of the short stories in Ian Maclaren's popular work—"Beside the Bonnie Brier-Bush." The preacher took for his text—

> "Whatsoever good thing any man doeth, the same shall he receive of the Lord, whether he be bond or free."—Ephesians vi. 8.

MR. NISBET commenced by contrasting what he considered to be the realism of the best type, that seizes on points good in humanity rather than the realism that is so popular at the present time, and which seizes on what is evil in humanity and emphasises that. He went on to speak in general terms of the sketches in which Ian Maclaren depicts the Scottish country doctor of the old school, giving instances of his courage, devotion, and unselfishness, and then, turning from fiction to fact, he drew a parallel in the life of Sir J. Y. Simpson, the great Edinburgh surgeon. Speaking of the profession generally, he preferred, he said, to judge them by the best specimens. There were some men who pronounced judgment on a class from the most unfavourable specimens. He preferred to generalise from the best specimens; for all the literature on the subject pointed to the best being in the majority, the outstanding characteristics of whom were unselfishness and courage. We could say of doctors what we could speak of no other class in the community—that they spent an immense amount of time and energy in impressing upon families, municipalities, and Governments the necessity for the observance of sanitary laws and hygienic conditions, all of which, looking at the matter from a worldly standpoint, would tell against the emoluments of the profession. As an instance, there was the recent tuberculosis meeting at the Town Hall, which was worthy of honourable mention. The same thing was characteristic of the doctors all the world over. In connection with courage, he contrasted moral and physical courage, and, judging by the highest standard, he thought medical men were more entitled to such decorations as the Victoria Cross than the men who received it. In closing he referred to the death of Dr. MacLure, as given in Ian Maclaren's sketches, in which the writer made one of his characters say, "The doctor is a kind-hearted mon, though he has his faults like us all, and he does na trouble the kirk often." But church or no church, creed or no creed, theology or no theology, it was written because it was God's truth. "Whatsoever good thing any man doeth, the same shall he receive of the Lord, whether he be bond or free."

WHARF-STREET CONGREGATIONAL CHURCH.

At the Wharf-street Congregational Church in the evening, the pastor (Rev. Hugh Jones, M.A.) took for the subject of his discourse—

A Healthy and Holy Humanity.

Referring to the presence in Brisbane of so many medical visitors, he asked what was the inner reason of their presence, and said that they had come not to look for wealth, but to seek mutual help towards attaining their ideal of a healthy humanity. This ideal was the possession of all true doctors; hence the medical profession was one of constantly increasing value and interest both for such doctors and for the public generally. The preacher drew special attention to the newly formed society in Brisbane for the prevention of consumption, and exhorted his hearers to give the most earnest heed to the information that would be disseminated by the society, and to do all they could to help the doctors to fight down the ravages of consumption. True ministers of the Gospel held before them the ideal of a holy humanity. It was pointed out that as "healthy" and "holy" were cognate terms, so doctors and ministers were working for the health of humanity on the physical and spiritual sides, and that neither ideal would be realised alone. The sermon was based upon St. Mark ii. 9-11, where Jesus heals the palsied man's body and purifies his soul.

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ALBERT-STREET WESLEYAN CHURCH.

The Rev. C. E. James preached at the Albert-street Wesleyan Church in the evening, when he took for his text Isaiah xxxiii. 24—

"And the inhabitant shall not say, I am sick; the people that dwell therein shall be forgiven their iniquity."

Mr. James said he did not believe in the cast-iron theory which attributed sickness in the individual to his or her sin, as Jesus Christ himself had exploded that idea. Nevertheless, there was a recognisable bond between health and morals, and, further, the history of medicine, especially during the last century, had run parallel with the history of morals, and the medical profession stood, at the close of the nineteenth century, one of the most powerful of the moral forces of the world. The meeting of the Congress in Brisbane gave the minister an opportunity of pressing home this aspect of affairs. And this he did. Of the great advance made, he thought the greatest triumphs were in the region of surgery, due largely to the successful application of anæsthetics and antiseptics. Speaking of the medical man's moral force upon a community, his deliberate impression was that there was no profession or calling, not even excepting the Christian ministry, that ought to produce grander men and more useful citizens than that of medicine. And there was no calling so capable of blessing humanity on its moral as well as physical side. But while he gave them all credit, he commented upon the undue assumption of authority which was arising in some quarters. In that connection he had been informed by a clergyman that there was a growing tendency to exclude ministers of religion from the bedside of the dying on the ground that it was not good for the patient physically. From personal experience he could not prove or deny this; but he sounded a warning note, pointing out the necessity in such cases for the attendance of clergymen. Still, he paid a tribute to the majority of the medical gentlemen his sick visitations had brought him in contact with, and in conclusion drew attention to certain phases of the self-sacrificing work in which many of them engaged.

INTERCOLONIAL MEDICAL CONGRESS OF AUSTRALASIA.

FIFTH SESSION.

BRISBANE, QUEENSLAND.

FIRST DAY—MONDAY, 18TH SEPTEMBER, 1899.

At 10 a.m., members of Congress assembled in the Library, Technical College, Ann street, for the purpose of entering their names on the Congress roll.

At 11 a.m., a GENERAL MEETING of the members of the Congress was held in the Lecture Theatre of the Technical College. Dr. John Thomson, President of the Congress, in the chair, with the Hon. Dr. Taylor, Treasurer, on the left, and Dr. Love, the General Secretary, on the right.

PRESIDENTIAL WELCOME.

Dr. THOMSON, in opening the proceedings, said he had to thank the members of the Congress very heartily for the very distinguished honour they had conferred upon him in electing him their president. (Applause.) The position, which was the highest gift in their power, they had bestowed upon him, and he thanked them very sincerely. He could not express his feelings better than by quoting what that prince of medicine, Dr. Fitzgerald, of Melbourne, had said on a similar occasion. Dr. Fitzgerald said that he had reached the highest point of his ambition. He (Dr. Thomson) certainly thought he had reached the highest point in his own—(applause)—and he trusted Dr. Fitzgerald would excuse him for borrowing his words. He had now the very pleasant duty of giving them a very hearty welcome to Brisbane. Many of them had come from a long distance, and he could assure them that it was an infinite gratification to their brethren in Queensland to welcome them here. He trusted they would have a good time, and that they would bear away with them pleasant remembrances of Brisbane. He hoped that they would also do something to dispel the notion that we had a bad climate. (Applause.) He could assure them it was not a bad climate. He had experienced it for four-and-twenty years, and he did not think he looked very bad on it. (Laughter and applause.) The Premier would give them an official welcome this evening at the Centennial Hall. He might tell them that at the request of the Premier the Governments of the other Colonies had appointed representatives to attend the Congress. (Applause.) New South Wales was to send Dr. J. Ashburton Thompson (applause); Victoria, Dr. Watkins (applause); Tasmania, Dr. Sprott (applause); New Zealand, Dr. Cahill (applause); and South Australia, Dr. Bickle.

EXECUTIVE REPORT.

The SECRETARY (Dr. Love) read the report of the Executive Committee as follows:—"On the last day of the New Zealand Session of Congress, in February, 1896, it was decided that Brisbane should be the seat of the next Congress, and that the president should be John Thomson, M.B., of Brisbane. In pursuance of this mandate a general meeting of the profession was held in May, 1897, as a result of which, office-bearers and Executive Committee were appointed from the two medical societies in Brisbane, necessarily from residents of Brisbane. District representatives for five divisions of the Colony were also appointed. Of these latter, we regret that one, Dr. Scholes, of Goodna, did not live to see the completion of the work—his place has been ably filled by Dr. Hogg. In March, 1898, the real work of the Executive Committee began. The same gentlemen who acted as local secretaries in the various colonies have again assisted us, with the exception of Western Australia, where Dr. Athelstan Saw has taken Dr. Harvey's place. In Adelaide, Dr. Poulton; Melbourne, Dr. Syme; Sydney, Dr. Muskett; Hobart, Dr. Sprott; and Dunedin, Professor Scott, have rendered us valuable assistance, without which our work would have been interminable. A guarantee fund was established. Her Majesty's representatives in all of the Colonies and the Admiral on the Australian station cordially acceded to the request to become patrons of the Congress. The Queensland Government was approached, and substantial assistance was promised by the Premier; and concessions were obtained from railway authorities in the other colonies and from steam shipping companies. In June, 1898, Dr. Hardie resigned his position as joint general secretary, and early in this year Dr. Jackson seceded from the committee. The work of Congress has this time been divided into six Sections, the subject of Eye, Ear, Nose, and Throat being promoted to the dignity of a full Section. The response to papers has been liberal in all save the Anatomy Section, where we have not succeeded in obtaining either papers or president, doubtless due to the absence of a school of medicine in Brisbane. Three matters were referred by the Dunedin Congress to the Brisbane Session—(1) The question of a fully representative medical journal for Australasia; (2) the establishment of a dental section; (3) the question of the Contagious Diseases Act. With the first matter the Executive Committee have not interfered. As regards the establishment of a dental section, it was thought desirable to recommend to Congress that so long as the registration of the dental profession in the various Colonies was in its present imperfect condition the establishment of a dental section was hardly feasible. Regarding the C.D. Act, this was advised to be referred to the Public Health Section to deal with. Finally, the Government have consented to issue free railway passes to members and wives for a period of one month, available over all Queensland lines." (Applause.)

The Hon. Dr. TAYLOR (treasurer) read the balance-sheet of the previous Congress, showing total receipts amounting to £439 13s. 8d., and a credit balance after the payment of expenses of £54 3s. 11d. In welcoming the visiting members, he said he could assure them that they were all very glad to see them, and would do all in their power to make their stay a pleasant one. (Applause.)

On the motion of the PRESIDENT, seconded by Dr. LINDO FERGUSON, the report and balance-sheet were then adopted.

DENTAL SECTION.

The question of the proposed establishment of a dental section was then discussed, Dr. Kent Hughes (Victoria) reading the correspondence from Melbourne, applying for the creation of a dental section.

The PRESIDENT explained the action of the Executive Committee in regard to the matter.

Dr. KENT HUGHES moved,—"That a dental section be formed at the next Congress."

Dr. WOLFHAGEN (Hobart) seconded the motion, which was subsequently withdrawn, and the PRESIDENT ruled that the Congress had no power to include dentists, as they were not properly qualified medical men.

The matter accordingly dropped.

THE C.D. ACT.

The SECRETARY read a memorial from the Women's Christian Temperance Union of Australasia, asking for the co-operation of the Congress against the proposed extension of the C.D. Act in Queensland, and in favour of the repealing of similar Acts in Tasmania and New Zealand. This also, as the PRESIDENT put it, was a heirloom from the previous Congress, at which, after being referred to the Public Health Section, it was referred to the present Congress. The manner in which some portions of the memorial were received indicated that some of the members had some difficulty in regarding them quite seriously.

Dr. LINDO FERGUSON (Dunedin) moved that the matter should be referred to the Public Health Section. It was a matter which could be better "licked" into shape by experts in Public Health than by general discussion.

Dr. CROWTHER (Hobart), in seconding the motion, said he had taken an active part in Hobart in passing their Act through the Legislature, but if it had to come up again he would not help it at all. He believed that it placed most dangerous and arbitrary powers in the hands of ill-disposed women and very dangerous powers in the hands of the police. That was his impression. He was not at all satisfied with the working of the Act in his colony.

The motion was then agreed to.

QUESTION.

Dr. PINNOCK asked if there were no means of saving the time of the Congress by having printed papers which were usually read.

The PRESIDENT stated that it was too late to do anything as regards this Congress.

ADJOURNMENT.

The session adjourned till Saturday, the 23rd instant.

RECEPTION AT THE EXHIBITION BY THE MAYOR AND MAYORESS.

The delegates were formally received at the Exhibition during the afternoon by the Mayor and Mayoress (Alderman and Mrs. Seal). Altogether there were about 160 guests, among whom were notable three young lady medical students—Miss Ellinor Bourne, Miss Thomas, and Miss Jean Thomson, youngest daughter of the President. At intervals Mr. Sampson played selections on the grand organ—a feature which added in no small degree to the enjoyment of the afternoon.

The MAYOR, who was supported on the platform by Dr. John Thomson (president) and Dr. Love (secretary), expressed the pleasure it gave him to welcome them. He was given to understand that they were there to carry on the work begun years previously—the work which originated in Adelaide twelve years ago, and which each three years had been revived at Melbourne, Sydney, and Dunedin. He hoped that work would be carried on with good result here. The session promised to be as successful as any of its predecessors; the number of members enrolled was greater, while the papers promised had increased from 83 to 155. They had, during the short time they would be here, serious work to overtake. He believed the public recognised that theirs was no light labour, and that it was a serious hardship for medical men to leave their practices and attend the conferences. They that did so showed their interest in their profession. There were several functions arranged in their honour during the week. He trusted they would enjoy their stay in the youngest city of the group, and that on leaving they would carry back with them happy and lasting recollections of it. (Applause.)

Dr. JOHN THOMSON said: On behalf of the medical profession he had to thank the Mayor for having received them. The Mayor had made one statement that he highly appreciated, and that had reference to the serious work of the Congress. He had an impression that the public generally thought the conference a sort of big picnic for the doctors—a jollification and junketing. For the life of him he could not see why it should not be—(laughter)—but he did not say it was. (Renewed laughter.) As a matter of fact they tried to combine pleasure with business. And why should they not? There were no men in the wide world who had fewer holidays than the medical profession; there were no men longer in the collar than the practitioner. Another man might have sixteen or eighteen hours a day, but he could generally reckon on Saturday and Sunday. But, with a doctor, he heard the bell "tinkle-tinkle" at 2 in the morning, and had to turn out, although he was weary and tired; or perhaps the golf picnic, arranged weeks before, goes to the wall because his best patient has been suddenly taken ill. There was to be some very serious work—work that required that the Congress should be divided into six sections or it could not be overcome. They all hoped that the present session would enable a valuable addition to be made to the tomes of literature which had accumulated as the result of the long and varied experiences of delegates who had attended the conferences. He again thanked the Mayor.

Afternoon tea and light refreshments were then served in a marquee erected in the grounds, and the remainder of the afternoon spent in social intercourse.

INAUGURAL CEREMONY.

The official opening of the Intercolonial Medical Congress took place in the Centennial Hall at 8·30 p.m. There was a very numerous attendance, a large proportion being ladies. The President of the Congress (Dr. John Thomson) occupied the chair, there being also on the platform His Excellency Lord Lamington (the Governor of Queensland) and Lady Lamington (attended by Captain Pelham, A.D.C., and Captain Fielden, A.D.C.), His Excellency the Hon. G. R. Le Hunte, Lieutenant-Governor of British New Guinea (attended by Captain Barton, A.D.C.), His Honour Sir Samuel Griffith (Chief Justice) and Lady Griffith, Right Hon. Sir Hugh Nelson (President Legislative Council) and Lady Nelson, Hon. J. R. Dickson (Premier of Queensland), Mrs. John Thomson, the Mayor and Mayoress of Brisbane (Alderman and Mrs. Seal), Hon. Dr. W. F. and Mrs. Taylor, Hon. E. B. Forrest, M.L.A., the Bishop of Brisbane, Rev. Thomas Nisbet, Dr. Bright (Hobart), Dr. and Mrs. Wilton Love, Major-General Gunter, and others.

The President invited Lord Lamington to do honour to the medical profession by opening the fifth session of the Medical Congress.

The Governor said that he felt first of all that he must apologise for the Queensland weather. It was most unusual and most unwarranted of it to rain at any time, but particularly on an occasion when it was not wanted. He welcomed them all most heartily, particularly those who came from the other colonies in Australasia. He trusted not only that they would find their sojourn here productive of fruitful work, but also that they would carry away with them pleasing reminiscences of Brisbane and its vicinity. No doubt the Congress would be found giving instruction in everything that pertained to the medical science, but he thought too—and he was glad to see that there was a class of lectures devoted to the subject—that the Congress would deal not only with medical science in its character of curing diseases, but also in regard to public health. The Congress might thus be enabled to induce people to take precautions against those conditions that rendered disease liable. (Hear, hear.) He believed that in Brisbane there might be found opportunity for great improvement in the sanitary arrangements, and he fancied the same observation might be made about various other towns in the colony. Recently they had had a meeting for the promotion of giving instruction in regard to the causes of and the prevention of the spread of tuberculosis. He trusted that the society then formed might do good work in the future, and that it might show people that by taking proper and decent care of themselves they might render disease less usual; and in fact that, instead of having, as in some cases like that of leprosy, an almost insane dread of the disease, people might estimate rightly diseases in their true proportions, and take such precautions as would render the community more immune from the chances of infection. In respect to the Congress, he was aware that many distinguished gentlemen had come up to visit us. He believed that numbered amongst the company were Dr. McCormick, Professor Anderson Stuart, and others whom it would be almost invidious to particularise. He regretted that that very eminent surgeon, Sir Thomas Fitzgerald, was unable to be present, owing to other important engagements. In mentioning those eminent in the medical profession the name of one would occur, though he did not live in Australasia—the name of Lister. (Loud applause.) He had been ennobled by the direction of Her Majesty the Queen, and had acquired that distinction because it might frankly be stated that he had revolutionised the art of surgery. (Applause.) If they inquired as to how that scientist had accomplished his great work, it would be seen that it was chiefly by reasoning and logical deductions following on patient observation. These were the great qualities, though not the only ones, which contributed to the success of those engaged in the medical profession. They should investigate most carefully the symptoms of the disease, and it was by these means that the greatest reforms were effected. The whole system of medicine was perhaps the most broad and humanising education. It required considerable knowledge of botany, of natural history, of chemistry, of electricity, of the use of the microscope, and so on. Perhaps more particularly in the use of the microscope had the most wonderful resources been found. Who had not heard of Metschnikoff and his discoveries in respect to the phagocyte? (Hear, hear.) When he had read of them, it seemed almost like a romantic passage in medical science. Perhaps the most interesting part of the question was that which touched the relations between the mind and the body. He was not one of those who indulged in fantastic notions in respect of mental influence, but the relation between the two was every day becoming more apparent. In Queensland they had the aboriginal population, who could not believe in disease or death except through the agency of some other individual at some, perhaps, great distance, who must be operating to produce these effects upon their bodies. They, the aborigines, failed to trace to physical causes or to their own mental condition any ailments or sufferings that might befall them. But medical science was evolving that mind was supreme in respect of the body. Of course it was a commonplace not only in medical science, but also in our social life. They saw it in regard to their own ideas of suffering. They all knew that mental suffering was much greater than bodily suffering, notwithstanding the terrors of some horrible bodily diseases that might afflict one. Take the ancient play of "Hamlet," written by Shakespeare. Who could imagine that those tragic events could have been related because Hamlet suffered from some bodily disorder? Naturally the question of the mind and the body was the most interesting which belonged to the medical profession. In every department of the work of those who were engaged in medicine they were dealing with the one great factor of life. Now, he believed it was assumed that the great generalisation was that living matter was of the same essential nature whether found in the animal or vegetable kingdom. That might be so, but it was also a fact that the human animal remained the truest type of all species. When dealing with the human body, one was dealing with that which had existed in its present form for countless generations. Whilst the mammoth and other animals had disappeared in the same glacial period, man alone, by his greater powers of brain, was enabled to survive. Therefore, whatever might be found in Java, or elsewhere, it was yet possible, or rather it was yet a fact, that evolution was only an accepted theory because it might be still impossible to trace that man was derived from one common source, owing to the impossibility of reconciling geological records with man's present nature. (Applause.) These were only theories he ventured to put forth. It was evident that the medical profession dealt with and treated a subject which gave them the greatest responsibility. A doctor in his relations with his patients accepted a most onerous position, and it required a man of the highest moral fibre, of the utmost self-devotion, and of the keenest sense of honour to undertake to enter into the medical profession. He saw around him there—and he thought it most appropriate—representatives of the law and of the Church—(applause)—and it seemed to him that a physician occupied something of a middle position between those two callings. Those who were engaged in the law dealt with man in his relations with man. The physician dealt with man in his relations with himself, and the clergyman dealt with man in his relations with his Creator and with the great hereafter. And, indeed, at the last closing scenes of a man's life these three were generally assembled. On the previous day many of those present heard sermons preached. He heard an eloquent discourse by Bishop Stretch, and another by Mr. Nisbet, dealing with that Medical Congress. Those gentlemen had described what acts of self-devotion had been performed by medical men; how they had been on the field of battle or in some pain-stricken town, where they had volunteered at the risk of their lives. Mr. Nisbet particularly brought forward an instance in that well-known story of "Dr. Maclure." He was glad to think—and Maclaren had mentioned it in his book—that the lifework of Dr. Maclure was not a solitary or isolated instance. It was a work performed by many in Scotland, England, Australasia, and in other parts of the world. He thought another main fact about medical men was not only in respect to their relations between man and man, but another relation was their relation with the world-wide community. They were perhaps not working drilled together, but as an army scattered about throughout the world. Working for one common purpose, embracing almost every country of the world, including Japan, they were seeking methods to remedy human pain, and, if possible, to prolong life. Recently the world had been exercised by the meeting of a great peace conference, but he considered that the medical profession was an ever-existing and continuously-sitting peace conference, because it was almost impossible, when they were distributed throughout every country of the world seeking to relieve pain, that there should not be a corresponding feeling induced into the minds of everybody of a dislike to the horrors and cruelties that war must necessarily inflict. Therefore he trusted this week's proceedings, heralded by that evening's meeting, would lead to the mitigation of human suffering; that those who had just started upon their career of healing would derive encouragement and enthusiasm by coming into contact with those who had already acquired distinction in their life work; and that in the history of the advancement of medical science and the amendment of public health, an epoch might be created in the minds of all by the holding of the fifth Australasian session of the Intercolonial Medical Congress of Australasia, which he had the honour to declare to be open. (Loud and continued applause.)

The PRESIDENT invited the Premier to welcome the visitors.

The Hon. J. R. DICKSON said it was his pleasant duty to extend to their distinguished visitors a most cordial welcome to Queensland. He was very glad indeed that Brisbane had been selected as the seat of the meeting of this Congress, the triennial sittings of which had resulted so much to the advancement and development of medical science. He sincerely hoped that the present session would still further increase the knowledge and the development of the healing art. He regretted, however, that Western Australia had been unable to send a representative; but he was sure that that did not arise from any lack of medical and surgical ability in that colony, but from circumstances which had rendered it inconvenient to send a representative. He welcomed the many medical experts and men of science who had come here, perhaps at some inconvenience, and he hoped that they would find compensation in the warmth of the welcome extended to them, and in the results of their labours. (Applause.)

PRESIDENTIAL ADDRESS.

The President (John Thomson, M.B.) then rose to deliver his address. When the rounds of applause which greeted him had subsided, Dr. Thomson said—

Your Excellency, Ladies, and Gentlemen,—When I received the intimation that, at a general meeting of members of the Intercolonial Medical Congress of Australasia, held in the city of Dunedin on the 8th day of February, 1896, I had been unanimously elected president of the next session of Congress to be held in Brisbane in 1899, I replied to Professor Scott, the general secretary, "In accepting this high position, which I do with mingled feelings of pride and diffidence, may I request you to place on record my hearty thanks for, and my appreciation of, the very distinguished honour which my brethren of the medical profession have seen fit to confer upon me? I very humbly pray that my efforts in connection with the Brisbane session will warrant the confidence reposed in me."

And now that the years have been bridged over, and what looked then so very distant is now so very real, my pride and my diffidence are only more accentuated. Familiarity with it has but increased my appreciation of the very high compliment which has been paid me, while the constant association with Congress workers and work impresses me with my own deficiencies.

The presidency of the Congress is the blue ribbon of our profession here. It is the very highest distinction which the Republic of Medicine can bestow on one of its subjects; and nothing but my own unworthiness can detract from the fulness of the honour which my brethren so unanimously and so generously conferred upon me. What adds to the lustre of the prize and swells my pride is to have my name bracketed with those of Verco, Fitzgerald, Sydney-Jones, and Batchelor—household names in Australasia; makers of medical history; giants at the work; and my predecessors in the Congress chair.

Had it not been for Death's untimely interference, another should have had the honour. We had with us in this fair city until some five years ago, when he answered to the summons, one who had made a name for himself far beyond Australasian seas; one who was an original thinker, an industrious contributor to scientific knowledge, a successful practitioner, and one who would have been the universally accepted of the profession. I mean the late Dr. Joseph Bancroft.

And since last Congress the Mower has not been idle; over 150 obituary notices, other than mere death advertisements, have appeared in the *Gazette;* and in the lists are the two knights, Sir Anthony Brownless and Sir Alfred Roberts, with those of Thomas Chambers, of Sydney; William Gardiner and Richard Youl, of Melbourne; Allan Campbell, of Adelaide; and Richard Scholes, of Brisbane.

And the world is all the poorer that it has lost such men as Bowman, Charteris, Erichsen, Ernest Hart, Harley, Braxton Hicks, Humphrey, Jenner, George Johnson, Benjamin Richardson, Rutherford, Greig-Smith, Lawson Tait, and Spencer Wells.

I have again to thank the profession; I have to thank those members of it who have journeyed to Brisbane to make our Congress a success; I have to thank you, Sir, and our friends who have honoured us by being here to-night; I have to thank my committee for their constant attention and kind assistance; and I have especially to thank our general secretary, Dr. Love, for the zeal with which he took up the work, and the energy and application with which he carried it through; to him we are all deeply bounden.

I had chosen as the text of my address this evening—

"OUR CRAFT IN THE QUEEN'S REIGN,"

but the subject is so extensive that the time at my disposal would barely suffice to catalogue the work that has been done with the names of the many who have been doing it. I have, therefore, confined myself to a few topics which may be of special interest to you; and I have occasionally used these as pegs upon which to hang my own opinions.

Ages and epochs have frequently been known by special designations, and already the quidnuncs are suggesting titles for the quickly fleeting nineteenth century—the century of invention, the era of electricity; to us it surely is the age of scientific medicine—not the century, for the rolling on of progress has been watched by one woman, our Sovereign Lady; it has all happened in her reign; and two of her subjects will be remembered while time lasts as having contributed most substantially to the advancement of our craft. Surely the names of Simpson and of Lister will never die! In mentioning them we must not forget the many and other serious workers in our own and foreign lands who have given of their strength and mental wealth to push along our Art.

And yet it is a far cry from the birth of medicine, through all the changes to the present day. To search for the beginnings of the healing art would be to hunt for the cradle of the human race, and find the hand that rocked it; for surely the paleolithic savage had some acquaintance with simple surgery, and his squalid spouse some knowledge of domestic medicine. Have we not received most important contributions to our materia medica from savage and semi-savage tribes; and in the case of our blacks are there not physiological and surgical procedures, so remarkable, which point rather to the shadowy traditions of a departed civilisation, than to the dawnings of a new life?

We are exceedingly ignorant of medicine as known and practised among the extinct nations of the East. Historians differ as to the position it held in Egypt; but if the Ebers Papyrus, written about the 18th dynasty, or 1500 B.C., was the medical text-book, and it is accepted as the most ancient work on the subject, its story is a strange blending of practical receipts and magical formulæ. One would naturally expect that a nation whose faith seemed to point to a material resurrection, by the costly and elaborate preparation made in expectation of it, through the process of mummifying by evisceration, would become acquainted with normal and pathological conditions sufficient to establish some theory of disease and its cause; but it seems the eviscerators were of the lowest and most ignorant caste, and were hooted away immediately their work was done.

The medical history of the Jews, as gleaned from the Old Testament, is limited, and important only to those who strive to attach a modern name to some words suggesting vague symptoms. In this manner thirty-eight diseases are said to be described. But health was a necessity among the tribes wandering in the desert, and the leaders emphasised the importance of sanitary laws by attaching to many of them a religious significance and enforcing their compulsory observance. This knowledge of cause and effect must surely have been spoils from the Egyptians.

The Greeks, with their lively mythology, asserted medicine to be controlled by a special heavenly bureau; and Æsculapius, a mortal and a successful practitioner, was eventually credited as the son of Apollo, the god of the craft and the permanent head of the department. He was worshipped as a divinity, and his temples partook of the nature of infirmaries and colleges. He gave two sons to the Greek army before Troy, and they acquired fame both as combatants and as members of the medical staff. Machaon was a successful surgeon, and Podalirius practised phlebotomy. The offspring of Æsculapius were known as the Asclepiads, and were supposed to practise, but guard with the greatest care the medical secrets possessed by the family, handed down from one generation to another. The term was used also to designate the followers of Æsculapius and the priests who officiated in his temples; and they were all bound by the famous oath which still exists not to divulge the secrets of the profession. Hellenic medicine was, unfortunately, the plaything of the priests and the toy of the philosophers; but the popular faith was that disease depended upon the temper or the vengeance of the gods, and the priests encouraged this delusion for it brought wealth to their temples. Of the Asclepiads, in seventeenth or nineteenth descent from the founder, sprang Hippocrates, a native of Cos. He has been called "The Father of Medicine," and he well deserves the title. He gathered from the schools all the medical knowledge of his day, and with his wonderful power of close observation, and his equal facility for accurate description, he has given us a text-book which is a marvellous monument of the man and his time. A perusal of Adam's transaction will astonish the reader at the minute accuracy of the symptoms and records of disease of an age 2,300 years gone by. The influence of Hippocrates was quickly felt over the polite world; he made the medical school at Cos the centre of professional learning, and his writings became the standards of the Greek and Roman physicians.

However interesting the study of the origin and growth of the healing art may be, we must take a long, long leap; for an authority not to be contradicted has recently stated, "When the Queen came to the throne in 1837, it is hardly too much to say that the average medical practitioner knew little more about the diseases of the heart, lungs, stomach, liver, and kidneys than was known to Hippocrates." At any rate it may be admitted that the science and art of medicine was still in the expectant statein which it had remained since the dark ages, notwithstanding the efforts of men like Harvey, Sydenham, Locke, Cullen, Gregory, Lettsom, the Hunters, Charles Bell, and many others. The physician was looked upon as a more important individual than the surgeon; the business of the former was supposed to be more obscure, and only to be elucidated by some subtle process of dialectic reasoning—his was a mental effort; the surgeon, on the other hand, was but a craftsman.

Descriptive anatomy was thorough, but physiology was in a very elementary condition, and the chief facts known were Harvey's discovery of the circulation, certainly "the greatest of the discoveries in physiology," and Sir Charles Bell's demonstrations concerning the sensory and motor functions of nerves. Diseases had been most skilfully and carefully reported, their every symptom and peculiarity watched and noted. They were grouped in classes according to their clinical significance, and nothing seemed awanting, so perfect and so replete were the observations and the experiences; yet pathology was an unknown quantity, and anything like a scientific foundation of medicine had not then been laid. Bacon's famous aphorism, "They be the best chirurgeons which being learned incline to the traditions of experience, or being empirics incline to the methods of learning," was as true or as false in the author's day as it was when 250 years later it was used by Erichsen as the motto of his book; for the whole science and art of healing was purely empirical. It was based on the results of mere experience and personal observation. These were the watch-words of sixty years ago. The only aid to the latter was the stethoscope, introduced by Laennec in 1816; but so slowly accepted that forty-five years later a well-known clinical teacher wrote of it as "an instrument useful to remove the head of the practitioner a respectable distance from the bodies of persons not distinguished for cleanliness, and as the most delicate method of auscultating the chest anteriorly in women." The thermometer, which now is in everyone's mouth, although graded by Fahrenheit in 1721, was seldom used clinically until late in our sixties or early in our seventies, although Boerhaave (1611—1738) is said to have employed it in the axilla when examining his cases; and the compound microscope—the instrument of all others to which medicine owes most, for have not its marvellous explorations revealed the new worlds of physiology and pathology—although invented by Jannsen about 1590,

In good Queen Bess' glorious days,

did not become the monarch of the laboratory and the intimate of man until our own great Queen reigned over us. And all the instruments—and they are many and various—for objective examination and the discoveries made by them, can be credited to our time.

ANÆSTHETICS.

The year 1847 is and always will be memorable, for in it Simpson, of Edinburgh, introduced chloroform as an anæsthetic. The history of anæsthetics runs far back into the distant ages. Opium was used in Homer's time; the Chinese have employed a drug named mayo, a preparation of hemp, for tens of centuries; the Greeks and Romans valued the mandrake, and intoxicants have occasionally been in favour; but something was needed speedy in its behaviour and evanescent in its effects. Morton, a dentist of Boston, in 1846 administered sulphuric ether to his patients. Simpson tried it, but was dissatisfied with it, and after much experimenting upon himself and his friends, selected chloroform, a chemical novelty discovered by Guthrie, of New York, in 1831. He publicly employed it on 15th November, 1847, in the Edinburgh Infirmary; and subsequently introduced it to his obstetric practice; and then a howl went up from the Scottish "unco-guids" that its use was in contravention of the Divine ordinance, for it annulled the primal curse. But among other and weightier replies the laugh went with him when he showed that the first operation mentioned in history—the removal of the rib for the genesis of Eve—was conducted under anæsthetic conditions: "The Lord God caused a deep sleep to fall upon Adam, and he slept; and He took out one of his ribs." When this fatuous opposition ceased, fear of the consequences or effects of the drug still lingered, and it was not until the Queen, in 1853, at the birth of Prince Leopold, took chloroform, that the prejudices against it disappeared.

Chloroform opened out new possibilities for surgery, and permitted the performance of many operative procedures which from their length and severity could not be undertaken during consciousness.

The production of local anæsthesia by the use of cocaine was notified in 1884 by Karl Koller, a medical student at Heidelberg, and to him is due the credit of the valuable discovery.

ANTISEPTIC SURGERY.

If we have to divide the anæsthetic honours with our American cousins, we can safely assert that the greatest discovery of any or all time affecting surgery was made by the Englishman, Joseph Lister; the honours are all his; there is no one in his own or other country who dare lay claim to any share in this great work, a work which has revolutionised surgery, relieved humanity, and reduced mortality. He knew of the war waged between the abio- and bio-genetic forces. He recognised the victorious labours of Tyndall, Pasteur, and others. He accepted Schwann's dictum, "That putrefaction was due to something suspended in the air which heat was able to destroy." He speculated that suppuration resulted from a similar cause—the entrance to a clean or surgically produced wound of a something from without which induced inflammation and other evils, and, acting on his belief, he introduced antisepticism—surgical or scientific cleanliness—and met with a great deal of opposition in so doing. I once heard a well-known Scottish professor, a brilliant surgeon, say in a dismal fashion, as was his wont, "Gentlemen, I'm going to operate. antiseptically. Mr. So-and-so," turning to one of his assistants, "shut the door and keep out the germs." Lister's earlier methods were cumbersome, and in some of their details have been proved unnecessary. But the truth of his system is now most firmly established, not only by bacteriological experiment, but by surgical result. Wherever surgery is practised, by and among all the civilised peoples of the earth, there Lister's name and fame are known and revered. He is the father of all modern surgery; he has made the craft what it is; and he lives, full of years, honoured, rich with the wealth of a world's admiration, and blest in that he has been spared to witness the triumphs of his labour.

With antiseptics, or the more recent aseptics, or a judicious blending of tho two, associated and in active partnership with anæsthetics, surgery has long since sallied from the limits which confined it to things external. It now opens every cavity and attacks every organ. The peritoneum, the dread of the old operator, is no longer feared, it is scarcely respected; two kidneys are not essential, one may go; the spleen can disappear; portions of the liver and pancreas may be excised; the gall bladder is opened and its stony contents removed; the pelvic viscera are freely dealt with; sections of the intestine are cut out; and lastly, and lately, the whole stomach has been extirpated. Abdominal surgery before Lister's time was in the hands of the very, very few, chief of whom were Keith and Spencer Wells; since Lister's time the work has been perfected, and everyone operates. Thoracic surgery is not so encouraging; wounds of the lung may be treated on surgical principles, and so have been some injuries to the heart, but the operator has a very different kind of material to deal with than is found to his hand in the abdomen, and his work at present is limited. The brain seems more amenable to surgical discipline; abscesses have been opened, tumours removed, and portions of the organ excised; the subsequent benefits resulting from some of these proceedings are not very apparent, but the fact is established that the brain can be successfully treated by surgical methods and precautions. Thanks to surgical cleanliness, inflammatory processes are held in check, pyaemia is curbed, and hospital gangrene destroyed, limbs that were formerly sacrificed are now conserved, deformities are speedily corrected by operative measures instead of by the long and weary pressure of mechanical appliances, while transplantations and skin graftings on the extensive plan of Thiersch are undertaken with the certainty that they will "take." Helmholtz's invention of the ophthalmoscope, in 1857, placed ophthalmology on a scientific basis, and made it a distinct department of surgery; while the rhinoscope and the laryngoscope (1855) have lit up the dark respiratory tunnels which reach from the nostrils to the commencement of the bronchi, and have given employment to specialists on the nose and throat.

In fact the progress made in surgery can be described by only the old dominie's exclamation "Prodigious." It is one of the marvels, one of the glories, of the Victorian age.

BACTERIOLOGY.

If bacteriology be one of the youngest of the sciences, and it can hardly be said to have reached its majority, some of the organisms which give to it its name were seen so long ago as 1675, when Anton Leeuwenhoeck, a Dutch philosopher, not only made his microscope—a simple one—but used it to such good purpose that he discovered and described organisms which are now known as bacteria, baccilli, spirilla, and micrococci. He classified these as animalculæ, but he formulated no theory as to the part they played in the vast orchestra of nature. Observers succeeded him; Muller, a Dane, named between 300 and 400 different varieties found in the waters—salt and fresh—of Copenhagen; Needham, an Englishman, experimented, and believed in abiogenesis. Spallanzani endeavoured, and successfully, to disprove the theory of spontaneous generation. In 1837, the year of Her Majesty's accession, Schwann (the author of the cell theory) asserted that fermentative processes were dependent upon the proliferation of certain yeast plants, and that putrefaction was due "to something suspended in the air, which heat was able to destroy." But 1863 was the year when Davaine proved that anthrax was due to the presence in the blood of the sufferer of a specific infective organism, the now well-known bacillus anthracis, and that may be taken as the year when bacteriology, as a special study, had its birth. Its infancy was both puny and capricious, and it was not until 1880 that the foundations of an absolutely sound constitution were laid; "the work done earlier than that was more likely to be erroneous than correct." And it has had many students in many countries; its literature rings with the names of them, and they are unanimous that their chief is Louis Pasteur, the chemist, and to him undoubtedly belongs the honour of having been the most successful experimenter, the most careful observer, and the most original thinker among the workers in the new field. Second only to him stands Robert Koch, whose name will be for ever identified with the discovery of the micro-organic cause of tubercle. Koch,—the tubercle bacillus,—and 1882,—are inseparable associates; they form a trigon for recording the whom,—the what,—and the when,—of later observations and discoveries. Koch's thoroughness is well exemplified in his famous postulates, which must be proven before a micro-organism can be accepted as the author of some special disease:—

1. The micro-organism must be found in the blood, lymph, or diseased tissue of the man or animal suffering from, or dead of, the disease.

2. The micro-organism must be isolated from these media, and artificially cultivated outside the animal body, and these cultivations must be carried on through successive generations, and the purity of them maintained.

3. The micro-organism thus obtained, after generations of pure culture, must, when introduced into the body of a healthy animal, capable of taking the disease, produce the disease in question.

4. The micro-organism must be again found in the inoculated animal, and in greater number, showing it has proliferated.

Pasteur, the Frenchman,—Koch, the German,—with the brilliant Englishman, Joseph Lister, will be for ever identified with the study.

The general belief seems to be that bacteria are necessarily pathogenic, and that microbe and disease are almost convertible terms. It is hardly understood that bacteria play an important, even if decomposing, part in many of the arts. Some of the fermentative processes are due to their action, so is "retting" in the linen and jute trade, the preparation of indigo, the flavouring of tobacco, the curing of opium, the ripening of cream and cheese, while the agriculturist, in the nitrification of his soil, the sprouting of his seeds, and the success of his silo is dependent upon the behaviour of friendly bacteria.

Some five years ago and Queensland had no bacteriological laboratory, when the Government Stock Department, recognising the urgent necessity, started work in a small way, and that small way has so proliferated, like the tiny organisms it was set to watch, that its paths spread all over the colony. The work done has been thorough, and largely original—such as the discovery in Australia of the bacteria of chicken cholera, fowl enteritis, and swine fever; the preparation of pleuro-pneumonia virus, guaranteed free from tubercular taint; of immune-blood-serum for the preventive inoculation of tick fever; of locust fungus for the destruction of locusts, grasshoppers, &c.; of the bacillus typhi murium, to destroy mice in grain fields and cornstacks; and of specially standardised tuberculin in commercial quantity. Brisbane is the only city of the south where this bacteriological product is manufactured, and its quality is pronounced equal to, if not better than, the best coming from Europe. Enough has already been issued to test over 2,500 cattle. All this has been freely accepted by both theorist and capitalist, and it is recognised throughout Australasia as eminently successful. If the Government has done so much for cattle, why not for the higher animals? Surely the workers in this land, toiling with brain or moiling with hand, are of more value than many cattle and deserve more consideration; and that consideration should be a fully equipped laboratory. The problems to be solved there are innumerable; but two of the simplest and yet perhaps the most frequent would be the bacteriological recognition of diphtheria and the serum diagnosis of typhoid. These are denied to the general practitioner; it is impossible for him to conduct the investigations; so the Government, whose duty it is, has, within the last fortnight, signified its intention to establish such an institution. It will be heartly welcomed by the profession here.

HYGIENE.

Hygeia, the daughter of Æsculapius, the goddess of physical health and the giver and protectress of mental health, has bestowed her name upon a science which, known and observed by at least one community, has received academic recognition only within recent years, although the first British Board of Health was created in 1848. Hygiene concerns itself less with the individual—not that he is forgotten—than with the community; less with disease—although it is actively combated—than with prevention; hence the synonyms—public health, State medicine, preventive medicine. The first board was finally superseded in 1871 by the Local Government Board, which is practically a committee of the Privy Council, dealing, among other things, with sanitary improvements, vaccination, and the prevention of disease. This department is emphatically a creation of the Queen's reign, and its enormous achievements are a lasting monument to those who initiated it and to those who perfected it; its published reports are in themselves a comprehensive reference library, exhausting all that is at present known of the science; while the investigations, "the patient search and vigil long" of the medical scouts, the struggles, the labours, the checks, the losses, yet the victories of this advancing scientific army, fighting insidious, treacherous, and deadly foes—fighting and winning—are surely as worthy of record and of honour on the tablets of fame as are the successful campaigns of an army on the colours of its regiments. Would not Diphtheria, Tubercle, Tetanus, and Typhoid read as well as Sebastopol, Lucknow, Tel-el-Kebir, and Burma?

Lord Wolseley thinks slightingly of the army sanitary officer, for he calls him "the creation of recent years, and, as a rule, a very useless functionary," and speaks of his duty as "a fad." Perhaps it is a fad with our lay army to secure for us a healthful peace in our time, but it is a fad which appeals very strongly and persistently to us, for it certainly gives promise of at least partial realisation.

With the Great Council of War which recently assembled in London under the presidency of the Prince of Wales, and with many similar gatherings, not the least of which was the one lately held here, when the Governor kindly took the chair, all to devise a campaign against, and, let us hope, for the eventual destruction of tuberculosis, there was initiated a new scheme of medical combat; for professional and lay forces have agreed to co-operate, and happy results may be expected from the union. The issue is not likely to be known in our day; but who dare say that, when another Congress meets at the close of another century, phthisis may not be referred to as a forgotten, because an extinct, disease?

Surely the time has arrived when concerted action should reign in Queensland! With federation accomplished, the Premier will cease to be Minister of War, the Treasurer will be relieved of his Customs, while the Postmaster-General will find his occupation gone. Plenty of time and opportunity for a new Minister—a Minister of Health. Under him would be the administration of many Acts, and the management of many institutions—the Health Act, Dairy, Adulteration of Food, Leprosy, C.D., Insanity, Asylums, Hospitals, Benevolent Institutions, Government Medical and Health Officers, Medical and Pharmacy Boards, and others in existence and prospective; for legislation of the most advanced health kind might now be framed which would be more readily accepted and adopted in sparse communities and in youthful towns, than when forced upon old country cities with their teeming multitudes wrangling over vested interests and squabbling over ancient rights.

The Minister need not, like him who presides over the Department of Justice, be a specialist—the Minister of Education does not always possess a university degree—but his under secretary, or at least his confidential adviser, should be an expert; and on his staff should be scientific investigators of recognised ability. Apart from its contributions to contemporaneous practical philosophy, this scheme would have an economic value and would yield in many ways an excellent return for the outlay, besides advertising Queensland as a State recognising the importance of protecting her family and guests by the most modern methods.

MEDICINES.

If the practitioner's needs are many, so are the medicaments within his reach and at his command. During the last half-century the additions to the pharmacopœia have been numerous and most valuable; and yet with so much to choose from, the complicated prescriptions of the past are rapidly disappearing in favour of simplicity and unity. The art of elegant prescription writing, in which stress was laid upon the necessity for a judicious use of a base, adjuvant, corrective and excipient, will soon be numbered amongst those that are forgotten; and the day of the polypharmacist is waning, although even now there is a well-recognised nostrum whose original formula required some couple of hundred ingredients. By the use of standardised preparations, concentrations, compressed drugs of recognised purity, hypodermic tabloids, and cachets, medicines may be administered guaranteed in their behaviour and deprived of their unpleasantness. The use of the needle for such remedies—about two dozen—as require to be immediate in their action, has been recognised for thirty years. But the most wonderful addition to our method of combating disease—introduced during the last decade—is that known as serotherapy. Jenner's great discovery in 1796 of vaccination is suggestive of the new treatment, but not identical nor to be confounded with it. Jenner inoculated the individual with an eruptive disease of a mild character, which afforded protection against another eruptive disease of a somewhat similar nature but of an exceedingly malignant and fatal type. In serotherapy, the blood serum of an immunised animal is used by hypodermic injection to protect the individual. These blood serums are known as anti-toxins; some fifteen or eighteen have been suggested and experimented with, and so far anti-diphtheritic (diphtheria), anti-streptococcic (puerperal fever, erysipelas, scarlatina, &c.), anti-tetanic (lockjaw) are provided. These all differ from the tuberculin of Koch, which is purely a bacteriological product, and was introduced before its scientific investigation was complete. The disappointment then created caused reactionary criticism and jeopardised the discovery, but constant efforts to improve the tuberculin have attained a measure of success; the results are both satisfactory and hopeful, and the belief is reviving that "tuberculin is still the best remedy available against tuberculosis."

In organotherapy the attempt is made by using extracts and preparations derived from animal tissue to treat disease, either organic or functional, attacking similar tissue in man. Bone marrow has been given for blood affections, heart extract as a cardiac tonic, brain and spinal cord extracts in nervous disturbances. Practically every organ in the body has been put under tribute to yield its healing virtue, but the results are not encouraging. In the case of thyroid preparations made from healthy sheep's thyroid gland, success has been claimed in cases of myxoedema and goitre, but some doubts exist, and the demand for these preparations is not increasing

SPECIALISTS.

If Herodotus may be believed, the land of the Pharaohs was the home of the specialists—" the art of medicine is thus divided among them; each physician applies himself to one disease only, and not more. All places abound in physicians; some are for the eyes, others for the head, others for the teeth, others for the parts about the belly, others for internal disorders." And there have been specialists more or less ever since, some from inclination, others from compulsion, as in the case of surgery, which, in the middle of the twelfth century, was divorced from medicine owing to the action of the universities in teaching *ecclesia abhorret a sanguine* (the church abhors the shedding of blood). It was therefore unprofessional for the learned physician to operate. He indicated the necessity for, and authorised the surgical interference, but left the actual performance to the menial chirurgeon—the hand worker.

With the extraordinary expansion of our craft in the Queen's reign specialism has kept pace, and specialists abound, so that the statement of the old Greek historian made some 2,350 years ago would apply to-day.

The profession owes much to the specialist, and some specialists are in debt to the profession. Where a man is learned in all or nearly all branches of his art, and when he accepts one of these branches as a subject of special study, remembering always that it neither is nor ever can be a separate entity, but is only an offshoot of the great tree of medical knowledge, drawing its nourishment and sap from the parent stem, and dependent on the laws and forces which govern the whole, then he will make headway; working with a scientific love he will store up experience, add to the sum of knowledge, and still further help to expand that very expansion which called him into being. There are many of these skilled specialists throughout Australasia who have deservedly won the confidence of the public and the esteem of their brethren, and to whom the profession owes much. Unfortunately there is another class of specialist, who have to thank the profession for the leniency shown them. Their specialties are results neither of love nor of learning, but are conducted on filthy lucre lines; and, to put it vulgarly, " are run for all they are worth." So surely as the patient can be quoted at a given valuation, so surely is an operation necessary or a lengthy line of special treatment insisted upon. This type is at present rare under the Southern Cross, and let us trust that it may remain so. Elsewhere it is unfortunately better known, and one section at least has gained the unsavoury soubriquet of " ovary hunter."

NURSES.

From the day when Florence Nightingale broke the windows of the hospital huts in the Crimea, admitting sunshine and fresh air, the darkness of the old nurses was exposed and the necessity for a new order of things made apparent. Sarah Gamp and Betsy Prig were found out, and had to go. The going was slow. In Edinburgh twenty years after they had not quite disappeared; and they lingered somewhat longer here. The change from the old to the new has wrought marvels in the practice of us all. With every confidence, we leave our cases to the vigilant attention of ladies whose training and experience have been gained during long years of hospital apprenticeship. They have been taught to observe and to obey, and that discipline insures in many —ay, most—instances the success of the treatment. It is needless to refer to the hard work which is undertaken by the nurse during her tuition period; but unnecessary work is to be condemned. Why any girl—any probationer—who should be taught to keep her hands aseptic and her touch deft and gentle, is compelled to scrub and clean, and generally to do charwoman's work, passes comprehension; its folly has been recognised, and the usage is fast disappearing. But she might with advantage be taught the essentials of invalid cooking. If the hale man rejoiceth himself with the pleasures of the table, the sick one has to be coaxed and humoured to take his nourishment; and, as unfortunately there seems to be a wearying monotony in the usually prescribed sick-room dietary, the nurse should know how varyingly to prepare it and how temptingly to present it. Her mission takes her to many a home where this culinary knowledge would be gratefully appreciated.

The present system of examining and issuing certificates to nurses is most unsatisfactory. The certificate has, certainly, to be worked and waited for; but it is practically valueless, for it represents no standard either of training or of testing; these vary, and are as numerous as the hospitals attended. To correct this, the universities might recognise such institutions as have a minimum number of beds, sufficient to provide clinical teaching; might prescribe the length and character of study; might indicate what practical work had to be passed before the candidate presented herself for the final test, which might be conducted on the lines of the well-known junior and senior examinations. Certificates would then have a recognised value, would be eagerly sought after, and be prized when obtained.

MEDICAL EDUCATION.

With the general progress, the education of the medical student has kept pace, and the methods of teaching and the facilities for learning insure a very substantial foundation on which his after life's work can be reared. The hidden mysteries of life, in health and in disease, are being slowly unravelled, thanks to the workers in every department of science; hypothetical problems of the past are fast disappearing before the material investigations of to-day; every branch of physics has been pressganged to supply the instruments and the methods of accurate observation and research; and, accordingly, the *fin-de-siècle* student of medicine enjoys a practical and a technical education which his father and his father's teachers never dreamed possible. Owing to the expansion of medical knowledge a five years' course is now demanded of the student, and even this seems all too short; for, if some three or four decades ago four years was the time allowance to digest the teachings of that date, the instruction of to-day can hardly be assimilated during the authorised quinquennium. The schools seem to vie with each other in the equipment of their laboratories and the thoroughness of their practical teaching, and this healthy competition is having a beneficial effect on all concerned, and is securing a high standard of work. The medical departments of the Australasian universities, thanks to the lofty ideals of those responsible for their foundation, have been established, and are maintained, on lines parallel with those of the best home teaching schools. The opportunities for practical training are most efficient; the curriculum is of full length; in fact, Sydney was the pioneer of the five-years' course; the examinations are thorough, and the graduates are freely accepted and registered by the Imperial Medical Council. We are justly proud of these men, and they have cause to feel pride in their *alma maters*, which, if not haloed round with a glamour of the past, shine bright in the present, and give promise of a grand historic future.

POSITION OF THE MEDICAL MAN.

The so-called learned professions were Divinity, Law, and Medicine; but these different faculties, with their varieties of learning, attracted distinct classes of students. While the church and the bar held out inducements to aristocrats and to representatives of wealth and vested interests, it was left to recruit the medical profession from another stratum of society; for a very large majority of our practitioners hail from a bourgeois origin, or a class from which wealth and influence have temporarily diminished if not disappeared. And why? Is it that those ambitious for honours and power fail to find a fitting field with us;—or that the distinctions when secured carry with them no substantial emoluments;—or does the hard grind of the ordinary practitioner, with the weary hours and the unholidayed years, deter those who expect that some portion of each day shall be their own to do as they please with—an expectation not to be realised by the busy medico? Or do the old traditions of retail trade and humble handicraft still linger, when the apothecary was identified with the grocer, and the surgeon with the barber? But this is a story of long ago, for the physician apothecaries, under charter bestowing upon them the special term Society finally separated from the City Company of Grocers in 1617; and in 1745 the surgeons were incorporated by the name of Masters, Governors, and Commonalty of the Art and Science of Surgery, breaking up the old Chartered Company of Barber-surgeons, which included Periwig makers.

Nor has the medical profession attracted those classes which supply the naval, military, and diplomatic services. And can it be wondered at? Where is the Lambeth for the most successful of us? Where a vacant woolsack that can ever be filled by the most brilliant of our craft? Are honours similar to those so liberally lavished upon victorious warriors on sea or land, ever likely to be conferred on those victors in our service who have conquered disease and death? Or is a country's thanks made substantial with a country's gift, or a city's welcome in its golden casket likely to reach any of the enormous army of between 35,000 and 40,000 in whose ranks we are?

Distinctions are being bestowed. We have quite recently (1897) had representation in the House of Peers, a unique thing in the annals of British medicine. In the roll of nearly 600, there is no one who is more richly entitled to the honour, for no one there has done more to deserve it, than he who signs himself Lister. Then there are thirteen medical baronets, and about seventy knights, many of whom obtained the distinction for military or diplomatic services; but two are members of our Congress—the veteran Sir James Wilson Agnew, who graduated in medicine sixty years ago; and our Victorian friend, Sir Thomas Fitzgerald.

In Australasia the medical profession has been highly esteemed, and its position cannot be assailed; the members are absolutely unfettered, and the individual is eligible for any position, political or social; and he is exactly what he makes himself, free to choose his place and free to occupy it. But he may discover his professional path beset with some obstacles, the more important of which are benefit societies, benevolent institutions, and official indifference; and these difficulties are to be encountered on all British courses.

FRIENDLY SOCIETIES.

The inception of the friendly society is of ancient date, for the craft, guild, and the village club were essentially benefit societies. Defoe, a couple of hundred years ago, advocated the promotion of "societies formed by mutual assurance for the relief of members in seasons of distress." The oldest known lodge is a Scotch one—the Free Gardeners of Dunfermline—whose charter dates from 1715, while the earliest notice of the Foresters is in the parish of Leeds in 1790. In 1793 an Act was passed in England to protect and encourage these societies; but it was not until the Queen's reign that they began to flourish, becoming benefit rather than benevolent, financial rather than convivial. Their growth has been assisted by careful and timely legislation, and it has been said of them "that there are no associations which are managed with greater rectitude and few with equal success." This may be all very true, but if the success is due to an enforced slavery of one or many of the officers, then the encomium is unwarranted. In the abstract Defoe's advocacy is incontestable, and it seems flawless; but in these latter days the working out of the problem has allowed abuse to creep in and spread and grow until the excessive growth has choked nearly all of the original; and the abuse now reigns and governs. One of the many functions of the society is to provide efficient medical attendance in the time of sickness, and such efficiency as can be obtained from a much overworked yet conscientious practitioner is at the disposal of the members. The club doctor, perhaps more than any of his fellows, has a searchlight constantly shining on him; he has not to deal with the individual, as his brother in general practice does; he has to stand the "sharpened, sly inspection" of his employers in lodge assembled *re* his attention and his skill; and woe betide him if the former is lacking or the latter doubted. With his neck ever in the collar, he is the representative of an important department in the medical army known for its energy, capability, and self-reliance; and also for the utter inadequacy of its remuneration or reward. It is needless to repeat the extraordinary tales of wretchedness and tyranny, of the enactment of everything base in humanity, of cruelty and oppression won by the *Lancet* in the "Battle of the Clubs," fought in nearly every large centre in England; or similarly told by Bruck in the "Sweating of the Medical Profession by the Friendly Societies of Australasia." We know it all too well, but to lay readers the story of the white slaves of England and of Australia will never be complete until it tells of the serfdom of the club doctor. Of course there are many exceptions; there are men, numbers of men, who are not in the slave class, but who work for and are worked by the lodges on rational and even courteous terms, and who manage to make the wheels of life run smoothly.

Then there is another phase of the subject, another form of agreement whereby associated societies or institutions engage men to devote themselves to club duty only; this arrangement while entailing more constant work at higher pressure, carries with it obligations which the ordinary club doctor considers debasing, for the contracting medico lends himself to a scheme by which the club is enriched at his expense; in other words, the salary he receives does not by a long way represent the fees collected to retain him, even when all expenses have been met. The institute doctor, as he may be called, has brought himself under the ban of the profession, and is boycotted by its members. Should this be? His behaviour is or has been undignified; he has voluntarily submitted to a somewhat humiliating arrangement, but, to use the language of the Medical Act, has his conduct been infamous in a professional degree? Putting aside the incompetent, the intemperate, and the indifferent, there are men beginning life who find no realisation in their day dreams of practice; there are unlucky ones who have not yet reached the noon of their work, anxious to find a home and a shelter for those depending upon them; and there are the elders, who through no fault of their own, have got out of the rush and hurry of the stream and been left by the edge of the water stranded on the bank. And there always will be such men; and if nothing better can be offered them they will of necessity accept unpleasantly subordinate positions. Various schemes have been suggested and promulgated to counteract the evils of the clubs and of the institutes, but nothing successful has yet been achieved, and will not be until the medical profession throughout the British Empire is as thoroughly organised as are the workers. The British societies at the end of 1896 had a membership of 8,078,000, and a capital of £25,408,000, or about three guineas a member; while the Australasian societies at the same date had 258,000 members, and a capital amounting to over £3,000,000, or nearly £12 a member. This is a big army to encounter, and it is well provided with the munitions of war. Would the fight be successful?

The scheme which might solve the difficulty would be the complete management of the club medical service by the Government. This may be apparently socialistic, but it is eminently feasible. A civil medical service could be created on the lines of the naval and military medical departments.

Men would enter it by examination, be trained to it, receive pay and promotion by seniority, be rewarded or decorated for distinguished service, and eventually retire on pension. It would be a large medical army, thoroughly organised, with its specialists travelling about doing their peculiar work. This service could easily be made attractive, and it would be an honourable one. The work might be hard, very hard; but it could never become tyrannically oppressive. Those worked for would get the best attention, and would quickly realise the fact; while the Government would terminate the evils and abuses which disgrace the present system.

HOSPITALS.

It has been claimed that institutions for the care of the sick and the injured poor are distinct outcomes of Christianity, and that the older and the other faiths of the world failed in what is accepted as a duty by the western religion, while the oldest London hospital (St. Bartholomew's) dates only from 1546, the famous Hotel Dieu, in Paris, was founded in the seventh century, and is believed to be the most ancient of its kind in Europe. About the middle of our century public attention was directed to the hospital question, and great changes were effected in construction and management, for these were found to be in most instances extremely defective, so the British hospital is distinctly Victorian. The idea of the institution is: To provide for the immediate, and, if necessary, prolonged care and comfort of all those who, requiring medical or surgical assistance, are unable to pay for it, and to render "first aid" or to undertake the emergent treatment only of urgent cases among all classes.

The hospital is essentially a benevolent institution; with us it is rated, and it receives State aid as such; privately, its funds are raised on the plea of charity, and the subscriber, whether called upon from the pulpit, waylaid at the street corner, or visited by the collector, is appealed to "of your charity"; donations and benefactions, not forgetting the Scottish mortifications, are charitable bequests. How different when the benefits are being dispensed; the idea of an alms is then thoroughly ignored; the fact that a special class only is to be provided for receives no attention, and the people generally look upon the infirmary as a huge medical hostelry to which they can demand admission for payment; and it is astonishing, as most of us know, how successful they often are. To those deserving of the charity it were a sin to remind them that they were recipients of it; to those abusing it—and their number is great—a fine and a black list were the mildest of penalties. But, of course, there must be occasional exceptions—cases of infectious fever or of violent injury; and experience has taught us that these few are usually the most thankful and the most generous. Their urgency or their agony gain them entrance. Their gratitude or their munificence condone the irregularity. Yet there may be some excuse. A hospital is, or should be, the centre of medical thought in its district; capable men are on its staff, its nursing is highly trained, its equipment is complete, and its facilities for treatment ample. Is it astonishing, then, if people flock where the attendance is excellent and the cost trifling? The wonder is that the profession generally tolerates it, for the profession, limitedly, is admittedly to blame; and the limits include more than the erring brethren of the paid and of the honorary staff. The private practitioner is occasionally involved. He having a case he fears to cope with, sends it to hospital lest it should fall into his rival's hands. But who shall say what interests or passions sway their conduct? Were they truer to themselves and their profession, some at least of the abuses would disappear, and with official supervision of the State bounty others would be destroyed. It has been suggested that members of the honorary staff of a hospital should receive for their services part of the fees, and some very big fees too, occasionally paid to the institution by so-called paying patients. Certainly not! This would but accentuate the evil, and open the points for nower and still baser mischief.

The surgeon on the staff of a city hospital is amply rewarded for his honorary work when bestowed upon those deserving it. If in doubt of this truth, let him resign and make way for another, rather than barter his services for a monetary consideration. But his staff appointment should not compel him to render gratuitous services to those well able to pay him. Keep these people out of the benevolent institution, or else provide separate accommodation for them, and admit them under regulations which recognise the attention they receive.

WANT OF OFFICIAL RECOGNITION AND PROTECTION.

From many risks and dangers the citizen is safeguarded, but it is more usually his chattels than himself that the law protects. He may not pawn his goods but to an accredited, license-paying, and protected individual; he may not sell them by auction but through a respectable, taxed, and again protected man of business; he cannot transfer his property but by the aid of a licensed and of course protected conveyancer; and he cannot settle his differences with his neighbour except with State provided machinery, set in motion and kept going by one of the closest and most protected corporations known. Himself he is supposed to look after, although he required protection with his affairs. Any humbug, charlatan, or rogue may work upon him mentally and take charge of him physically, loading his stomach and unloading his purse, and no one can interfere.

The individual is unprotected and the practitioner undefended. The latter, with a capital representing long years of preparatory work crowned by satisfactory evidences of competency and a financial outlay covering nearly a quarter of a century—for the earning era of a practitioner cannot commence until he is four or five and twenty—has to enter into the open field and contend against advertising frauds, whose only stock in trade is unblushing, criminal effrontery. Some of these folk may not do much or any harm with their nostrums; they are too ignorant and too timid to deal with active remedies; but the incalculable evil they are guilty of, the personal and domestic injury they inflict by their intimidating assertions and forecasts—for both their diagnosis and prognosis are intended to terrorise and secure trade—are known to us all. If most of their nostrums are innocuous some are not, and many of the patent medicines which are so lavishly advertised, and which find such ready sale in our continent, contain potent ingredients. It is but fair to assume that they are compounded from the prescriptions—probably illegally obtained—of some practitioners who, in selected cases, succeeded with their treatment; but these would never claim, as the patentees and venders do, a catholicon—a universal remedy.

Surely in these days, when every water has to be analysed, every food proclaimed fit for man's use, and every drug certified free from adulteration, the sale of unknown remedies should be prohibited. If proprietary and patented medicines are necessary for Treasury purposes, or to humour the vagaries of a credulous public, the formulæ of these should be given upon oath and submitted, along with the trade mark, and registered before any protection order could be issued. By publishing his formulæ and exposing his methods, the mystery which surrounds the quack would be unshrouded, the public would then discover that he has no rare drugs, no wonderful specifics, no secret remedies, and that the contempt which the medical fraternity has for him is not begotten of professional jealousy, but is deserved, as the common rogue and cheat deserves the contempt of the well-conducted community.

A great deal has been done at home to protect the medical man from outside foes and from others within his own profession; recent (1886) legislation, the care of the General Medical Council, and the vigilance and activity of defence associations are guaranteeing a better state of things, one whose beneficial effects are already being felt.

Here in Australia the various Medical Boards have little else to do than inspect his credentials and register the new arrival; and when subsequenty he appeals for protection against a next-door quack, he finds to his dismay that the machinery to assist him is too complicated to act, or the difficulties are too great to be overcome. The constant cry of the Colonial Boards is for fresh enactments empowering them to deal with registered and unregistered transgressors. Their appeal is a just one, it deserves practical response.

THE PAST AND THE FUTURE.

It has been said—and the statement is used somewhat as a motto by one of the great American drug houses—that the medical art has made more progress in the last twenty years than during tho previous twenty centuries. This may be true, possibly is, and when the great discoveries of the last two decades are remembered, it seems true, but if true then the years have to thank the centuries.

During these latter the workers in the many medical fields were never idle; the land was reclaimed and the soil prepared; crops sometimes good and useful were cut and stored; crops bad and dangerous were destroyed; along the line of centuries numerous storehouses were erected and packed with the fruits both of knowledge and experience, and there is an unbroken record of leaders and foremen great in their generation as the greatest of to-day who shed upon the work lights which shine now, and which the more dazzling glare of our arc lamps cannot obscure.

Then there were scientific artificers in the workshop of optics and acoustics and other sciences, inventing and fashioning the tools, the microscope, the spectroscope, the opthalmoscope; seekers in chemical laboratories striving after the denied—the philosopher's stone and the elixir of life—but in the search unravelling mysteries and gaining knowledge, important to medicine; botanists, naturalists, and meteorologists; electrical experimenters and engineers, with apparatus for testing, illuminating, and exploring. What wonder then with this wealth of perfected tool and appliance, but recently brought to the fields of medicine to assist in the work there, that the progress during the past twenty years has been lightning speeded and the results marvellous. And why should this cease? The work of the past will continue in the future, and new medical discoveries will astonish other generations. The public knew the insecure and empiric foundations on which our science stood, they will be quick to realise the truths which now support it and give it life and strength.

Bacteriology teaches that its germs in their proliferation "break down complex organic substances into simpler products"; may not the growth and the development of the new science break down the complex fallacies and myths of medicine, disorganise the obscuring cobwebs of empiricism, simplify the study of health and of disease, and produce a natural and a concrete medical philosophy appealing as only material truths can appeal to a distrusting public?

Can anyone doubt that the infectious and contagious diseases, the germ diseases as we at present know them—and their name is pretty well legion—will in the process of time be swept from the earth; that all parasitic affections will disappear; that cancer will be conquered, and that gout and rheumatism will be subdued? It may be that other affections will replace them, disturbances of a neurotic kind; but if the sum total be perennial comfort, surcease of sickness and suffering, improved physique and increased longevity, surely tho hygienic workers will not have worked in vain.

The wonders of the Queen's reign are but an earnest of greater wonders yet to come. Who, sixty years ago,

> That dipt into the future, far as human eye could see,

dared prophesy of the accomplished marvels of to-day? And we, standing on the brink of a new century, dare we hint that science stagnates or that knowledge lingers?

The address was frequently heartily cheered; and when the loud and prolonged applause which was bestowed upon the President when he had resumed his seat had ceased,

Sir S. W. Griffith rose, and, in moving a vote of thanks to Dr. Thomson for the extremely instructive and interesting address that he had delivered, said he would like to refer to one or two points which had been touched upon. He was sure they must all have been struck by the very interesting survey the President had given of the progress of medical and surgical science during the Queen's reign. There were few present who could not remember very great advances made within their own time. It was only the other day that consumption was supposed to be a hereditary disease, for which the physician could do nothing. Now, at any rate, we knew its cause, and, with Dr. Thomson, he hoped that it would not be the end of the century before they knew the cure, and that ere long the disease might be practically extirpated from the face of the earth. (Applause.) It was within his own experience when he was a Minister of the Crown that the medical advisers of the Government did not know what was the best course to adopt when a ship arrived with passengers suffering from typhoid. Now, at any rate, medical science had a definite idea of the nature of the disease. He had been very interested in Dr. Thomson's remarks with respect to nurses and hospitals, and with respect to quack medicines. He would suggest for the consideration of Mr. Dickson, his colleagues, and the Parliament, the laws of some of the European countries, which did not allow the proprietors of quack medicines to prey upon the public. (Applause.) It was a great gratification to him to see so many visitors from the southern colonies. As he had said in many parts of Australia, the more we knew of one another in Australia the better for Australia, and every part of it. (Applause.) For his own part, he thought he expressed the sentiment of everybody in Queensland when he said, "The more our neighbours knew us the better they liked us, and our country." (Hear, hear.) Although the "terrible climate" which his southern friends sometimes referred to might deter them, and although the experience the present visitors had had of the climate was not all that could be desired, still they would have the opportunity of seeing the people, and judging whether they looked as if they lived in an unhealthy and dangerous climate. (Applause.)

The President briefly responded.

The Rev. T. Nisbet moved a vote of thanks to His Excellency the Governor, and in doing so expressed the hope that when he returned to the colony he would be able to lay the foundation-stone of the Queensland University. (Applause.)

His Excellency, in returning thanks, referred to the fact of the establishment of the Pathological Institute by the Government, and spoke eulogistically of Mr. Pound's work. (Applause.) Sir S. W. Griffith and Mr. Nisbet had made demands on his Prime Minister for future service, and he had a request to make himself. Speaking entirely from his own observation, and not from anything he had heard, he wished to say that he thought the responsibilities of the Board of Health should be increased so as to give them more efficiency in their work. (Applause.) He trusted also that the two medical societies in Queensland—the Queensland Medical Society and the Queensland branch of the British Medical Society—would soon be amalgamated, and work together as one body. (Applause.) He thought, at any rate, that in not large communities it was desirable for people to unite their forces so as to secure greater effect. (Applause.)

The proceedings then terminated.

SECOND DAY—TUESDAY, 19TH SEPTEMBER, 1899.

At 10 a.m., in the Lecture Theatre of the Technical College, Dr. R. SCOT SKIRVING, of Sydney, President of the Section of Medicine, delivered his presidential address, 125 members of Congress being present.

At the conclusion of the address, which was frequently applauded, Dr. Skirving was tendered a hearty vote of thanks, on the motion of Dr. THOMSON, President of the Congress.

The various sections then assembled in the class-rooms assigned to them, and until 1 p.m. o'clock the following papers were read and discussed :—

SECTION OF MEDICINE.

Notes of a case of Primary Carcinoma.
Dr. J. R. PURDY (Wellington, New Zealand).

Excretion of Water and Urea in case of Patient with one Kidney.
Dr. J. B. NASH (Wallsend, N.S.W.)

Complicating Infections.
Dr. A. C. F. HALFORD (Clermont).

SECTION OF SURGERY.

The Treatment of Hydatids by a Modification of the Closed Method.
Dr. WOLFHAGEN, for Dr. RAMSEY (Launceston).

The Operative Treatment of Hydatids situated low in the Pelvis.
Dr. W. MOORE (Melbourne).

A Review of Hydatid Disease.
Dr. R. SCOTT (Ballarat).

An interesting discussion followed, in which Drs. HAMILTON RUSSELL, G. A. SYME, L. E. BARNETT, CLUBBE, and the President, Dr. MCCORMICK, took part.

The Treatment of Severely Crushed Limbs.
Dr. W. MOORE (Melbourne).

SECTION OF EYE, EAR, NOSE, AND THROAT.

Eyesight in School Children under Colonial Conditions.
The President, Dr. LINDO FERGUSON (Dunedin).

Discussion ensued, and it was decided to appoint a committee to consider the main points of the discussion and formulate resolutions to be submitted to Congress, with a view to approach the educational and other authorities on the matter.

On Bifocal Lenses.
Dr. W. KENT HUGHES (Melbourne).

A Case of Segmental Hemichromatopsia.
Dr. F. A. POCKLEY (Sydney).

SECTION OF PUBLIC HEALTH.

Isolation of Infectious Diseases by Local Authorities.

Dr. GREGORY SPROTT (Hobart).

After some discussion, in which Drs. LOVE, TILLEY, BRIGHT, DAVENPORT, ESPIE DODS, BROCKWAY, and DOYLE took part, it was resolved, on the motion of Drs. SPROTT and W. CORRY—

"That in the opinion of this Section the compulsory notification and isolation of infectious diseases by proper hospital accommodation is of such importance to the health of the community, as well as to the best interests of trade and commerce, that this Section respectfully submits to the attention of the colonial Governments the desirability of providing such accommodation in the various centres of population."

QUEENSLAND ART SOCIETY.

In the afternoon the members of Congress were guests of the Queensland Art Society, whose annual exhibition was being held in the Masonic Hall.

GARDEN PARTY.

In the evening the President of the Medical Society of Queensland (Dr. Hardie) and Mrs. Hardie entertained the visitors to a moonlight party in the Botanic Gardens. The preparations for this were extensive, elaborate, and artistic, and had it not been for the inclemency of the weather a most successful and unique function would have been chronicled.

THIRD DAY—WEDNESDAY, 20TH SEPTEMBER, 1899.

At 10 a.m., in the Lecture Theatre of the Technical College, Dr. A. McCORMICK, of Sydney, President of the Section of Surgery, delivered his presidential address. His Excellency the Governor and Lady Lamington were present, and occupied seats on the platform. The members present numbered about 120.

At the conclusion of the address, Dr. THOMSON, President of the Congress, proposed a vote of thanks to Dr. McCormick, and it was carried by acclamation.

A paper was then read by Dr. F. J. CLENDINNEN (Melbourne) upon "Three Years' Experience with the X Rays," which was listened to with great interest. The various experiments made by Dr. Clendinnen were illustrated by a series of remarkably fine lantern slides.

At the conclusion of this demonstration His Excellency and Lady Lamington retired.

During the forenoon the following papers were read:—

SECTION OF MEDICINE.

"So-called" Brain Fever of the Drama.

Dr. T. HOPE LEWIS (Auckland).

Diphtheria.

Drs. J. MACDONALD GILL, C. P. B. CLUBBE, and A. JEFFERIS TURNER.

SECTION OF SURGERY.

The Surgical Treatment of Appendicitis.

Dr. C. P. B. **Clubbe** (Sydney).

Discussion followed, Drs. **Syme**, **Hope Lewis**, **Hopkins**, **Falkner**, and **Scott** taking part.

SECTION OF EYE, EAR, NOSE, AND THROAT.

Some discussion took place in this Section on the question of the eyesight of railway and marine employees. At the previous Congress resolutions were passed for the formulation of tests of the eyesight of men employed in shipping and on railways; and they were forwarded to the authorities interested. It was stated that the precautions still taken to guard against the employment of men who were colour-blind or whose vision was otherwise defective were carried out in a very lax manner in some places, and instances were given of men who, though condemned after scientific examination, had been employed afterwards on a practical test. Dissatisfaction was expressed with the regulations in Victoria and New South Wales and elsewhere, and it was urged that it was desirable, in the interests of public safety, that matters should be placed on a better footing. The whole matter was referred to a committee, with instructions to draw up resolutions for submission to the Congress.

SECTION OF MIDWIFERY AND GYNÆCOLOGY.

Contributions to the Simplification of Hysterectomy.

Dr. J. **Foreman** (Sydney).

The treatment of Uterine Myomata.

Dr. **Ralph Worrall** (Sydney).

Surgical treatment of Fibro-myomata.

Dr. E. T. **Thring** (Sydney).

SECTION OF PUBLIC HEALTH.

Reform in Quarantine.

Dr. **Eames** (Newcastle).

The paper dealt with the quarantine regulations affecting the port of Newcastle, the contention being that the various restrictions which had been placed upon shipping had impaired the reputation of the port and diverted tonnage which would otherwise have come to Newcastle. His suggestion was that they should perfect their sanitary measures, waterworks, and disposal of town refuse, and adopt compulsory vaccination; then substitute medical inspection, with the application of the Public Health Act.

Dr. **Doyle** (Newcastle) spoke in support of what had been done by the health authorities in Newcastle, and in disagreement with the view taken by Dr. Eames. At his suggestion, further discussion was deferred until the arrival of Dr. Ashburton Thompson, the president of the section.

SPECIAL MEETING OF CONGRESS.

In the afternoon there was a special meeting of Congress to permit the members to discuss the question of "The Relations of the Medical Profession with Friendly Societies." There was a fair attendance (about fifty-five); and the President, Dr. Thomson, occupied the chair.

Dr. **Syme** (Melbourne) opened the discussion by explaining what had been done in Victoria to improve the lot of the lodge doctor, and to remove the grievances from which he suffered. There, as in some other colonies, medical defence societies had been formed, and the matter having been taken up by the Victorian society, a conference had been held in Melbourne. He then explained the resolutions which were arrived at on that occasion, and which practically referred the matter to the Congress.

Dr. CROWTHER (Tasmania) said that to a certain extent he took the opposite side. He had been associated with friendly societies for over twenty-four years, and he had no trouble with them. He had a number on his list who paid from £2 2s. to £5 5s., and he had received as much as ten guineas for a special operation. It was very difficult to say what was best to be done to prevent people who could afford to pay ordinary fees taking advantage of lodges. A wages limit of £200 a year had been mentioned, and he would ask if there was any member present who had found himself with £200 a year and typhoid in his house? If any had, he doubtless discovered that the whole of the £200 was soon gone. A wage limit of £200 was quite out of the question. In Hobart the doctors once combined to charge higher fees, and the result was immediate competition from outside. He thought it was really a matter for courteous and friendly discussion with the friendly societies rather than hard-and-fast resolution. It would not do to take up a stand-and-deliver attitude. They had to recognise that the friendly societies were a very important factor in the community, and if it were not for them there would be a poor law throughout Australia. He (Dr. Crowther) had been treated so fairly and courteously that he could not support anything like a stand-and-deliver policy. (Applause.)

Dr. WORRALL (Sydney) said that for three or four years he had one of the largest lodge practices in Sydney, and it seemed to him that the previous speaker's experience was altogether exceptional. (Applause.) He was not now connected with any lodges in a professional sense; but he could say, from his own experience, that the grievances of the lodge doctors were very real indeed. One of the three great grievances they had to fight was proprietary medical provident societies. Those societies were supported by canvassing the patients of other doctors. A society of that kind was started in Sydney, and it boomed for a time. Medical men, however, met together, and decided that it was adverse to the interests of the profession, and must be met by the relentless professional ostracism of every practitioner who accepted office with such a society or company. As a result, many medical men retired, and now only the riffraff of the profession could be obtained to take their place. Those who gave up their position then asked what was to be done for them, and the result was that a Provident Society was formed by the whole of the medical profession, and employment was found for them there. This society had evils of its own, but it was thought the only way of combating the other system, which threatened to embrace a large portion of the population of Sydney and suburbs. The means he suggested for combating that sort of society was by determined ostracism, social and professional, of practitioners who stooped to ally themselves with the societies to which he had referred. The next evil was what were called institutes, formed by the amalgamation of benefit societies. Each of these institutes engaged a medical man at a fixed salary, regardless of the number of his patients. Several of these were established in Sydney, and they had become so powerful that they could not be abolished. It was possible to prevent new institutes being formed, but they could not abolish the existing ones. Then they came to the ordinary and legitimate lodge combination of working men. Here the difficulty which occurred was the wage limit. He himself had attended Ministers of the Crown, who received £1,500 a year. [Dr. CROWTHER: "They don't stop long on my list."] He found that they could not prevent cases of that kind taking advantage of membership of a lodge. All they could do was to endeavour to promote a better feeling between the lodges and the doctors, and induce the lodges not to take men on the list who were well able to pay.

Dr. CLUBBE (Sydney) moved,—"That this meeting is of opinion that the present relations of medical men and friendly societies is unsatisfactory." It seemed to him the only way to meet the difficulty was by combination. The difficulty all arose from the way in which doctors competed with one another. He did not blame the lodges. If medical practitioners would treat men at ridiculous prices, it was their own fault if people took advantage of them.

Dr. SCOTT (Tasmania) said no doubt the crux of the matter was the want of union between the medical men. He did not think anything could be done unless they combined together to bring about a better state of affairs.

The motion was then put and carried.

Dr. SAMUELSON (New South Wales) thought the position of the profession was like that of Æsop's wagoner who had driven his wagon into the quagmire and could not get it out again. They had partly driven their wagon there, and it was for them to consider how they could get it out. It seemed to him that the friendly societies from the vantage ground which they had won from general practitioners were invading the provinces of the consultant and the specialist. They had, therefore, a right to ask those of light and leading to help them to extricate the profession from the difficulties in which it found itself in connection with the matter. His suggestion was that they should form an association to supervise all contracts between medical men and the friendly societies, and to improve the conditions under which such practitioners laboured. He moved that they should recommend the formation of such an association, because he believed its disciplinary powers would be of benefit, and its advisory powers equally great. (Applause.)

Dr. EAMES (Newcastle) seconded the motion. He thought it would be a very good thing to have such a society formed, because friendly societies would then be able to apply to it for medical practitioners, and it would be able to supervise the relations between practitioners and those societies.

Dr. NASH (New South Wales) objected to medical men having to sign contracts with friendly societies, because those agreements were binding on one side and not on the other. The only object of the societies was to get the medical man and then worry him. Nothing would be done to improve their relations until the doctors united and formed a society which could intervene between the lodges and the doctors. Let the medical profession be organised, and establish a fund for the assistance of the poorer members of the profession, so that they would not be tempted to humiliate themselves by entering into a contract with these societies.

Dr. SAMUELSON moved as an amendment,—"That this meeting recommends the formation of an Australian Medical Practitioners' Association, for the purpose of supervising and controlling the practice with regard to benefit societies and similar institutions."

Dr. CROWTHER suggested that they make additions that some limit should be placed on the number who might enter the profession. If the supply exceeded the demand, then things were awkward, and that unfortunately was the position of the medical profession. What they required to do was not only to fix proper rates, but to take steps to reduce the number admitted to the profession. He felt sure they would have to do that sooner or later.

Dr. GLYNN CONNOLLY (Brisbane) said he was glad that he had no lodges now. Friendly societies were very good things, but they required to be controlled. (Applause.) The question was, How were they to go about the treatment of the case? The only way he could see was by organisation. What they had to do was to combine. They could not, of course, avoid signing contracts, but they ought to have a voice in fixing the conditions. They could only do that by combination. They must organise, and prepare to fight, and those who had not lodges should help those who had. To do that they must subscribe funds, not merely for the sake of the lodge doctors, but for the general good of the profession.

Dr. Chapple (N.Z.) said the difficulty they had to face had arisen chiefly from the agreements having been made by one side, without consultation with the other. He thought if they were to prepare a model agreement they would be able to secure the improvement of existing conditions. He did not approve of the motion, because he believed they had sufficient organisation in their medical societies. He proposed, as an amendment, that a committee should be appointed to draw up a model standard agreement, which should form the basis of all contracts with friendly societies.

Dr. Syme (Melbourne) considered that the proper way to deal with the matter was by means of the Medical Defence societies. He did not think that their medical societies, which were largely of a scientific character, could properly take up questions of this kind. He believed the friendly societies recognised there were grievances. What was wanted was a representative body, which could negotiate with them for the improvement of conditions.

Dr. Barnett (N.Z.) thought the subject would be more thoroughly dealt with if referred back to the Medical Defence associations, rather than to a committee. He thought some wage limit, if not £200, say £300 a year, should be introduced, so as to prevent the notoriously wealthy from obtaining the benefit of friendly societies. He moved a further amendment, remitting the matter to the Defence Association, with the request that they should draw up a model agreement for the use of lodge surgeons, and recommending them to bring under disciplinary control to the utmost of their powers those medical practitioners who offended against the spirit of the agreement.

Dr. Worrall (Sydney) moved, as a more suitable amendment,—"That this Congress suggests to the defence associations that they should take up the control of the relations between friendly societies and medical men, and urges upon the colonies which do not already possess defence associations the formation of such an association; and that the following gentlemen be hereby appointed in the various Colonies, with power to add to their number, to press forward the desire of the Congress in this important matter:—Drs. Sprott, Syme, Morgan Martin, J. Thomson, Love, and Hayward."

Dr. Samuelson withdrew his motion, and seconded Dr. Worrall's proposal.

Dr. Barnett also withdrew his amendment, and Dr. Chapple his.

Dr. Worrall's proposal accordingly became the motion, and as such it was carried unanimously.

The proceedings then terminated.

BALL GIVEN BY THE PRESIDENT AND MRS. THOMSON.

In the evening the President of the Congress (Dr. Thomson) and Mrs. Thomson gave a ball in the Centennial Hall, entertaining between 400 and 500 guests, among whom were the Governor and Lady Lamington, the Governor of New Guinea, the Chief Justice and Lady Griffith, the President of the Legislative Council and Lady Nelson, the Premier and Miss Dickson, the Bishop of Brisbane, the Commandant, the Congress visitors, all of whom had been invited, and many others.

This dance is reported to have been one of the very best entertainments of its kind, public or private, that has ever been held in Brisbane.

FOURTH DAY—THURSDAY, 21st SEPTEMBER, 1899.

At 10 a.m., in the Lecture Theatre of the Technical College, Dr. J. Ashburton Thompson (Sydney), President of the Section of Public Health, delivered his presidential address to a large audience, the subject being "Consumption and Its Mitigation."

At the conclusion of the address, which was frequently applauded, the President of the Congress (Dr. Thomson) moved a hearty vote of thanks to the lecturer which was carried by acclamation. Dr. Ashburton Thompson having replied, the Public Health Section was formally constituted.

The following papers were then read and discussed in the various Sections :—

SECTION OF MEDICINE.

Amœbic Abscess and Amœbic Dysentery in Australia.
Dr. Isbister (Sydney).

Discussion on Typhoid Fever.
Drs. Scot-Skirving, Hare, Hirschfeld, Isbister, Pain, Sprott, Nihill, and others.

SECTION OF SURGERY.

The Discussion on Appendicitis was continued and concluded.

The Anatomy and Treatment of Talipes Equino-varus (Congenital).
Dr. Kent Hughes (Melbourne).

Recurrent Carcinoma of the Breast treated with Thyroid Extract.
Dr. Jas. P. Ryan (Melbourne).

Some Observations in Renal Surgery.
Dr. L. E. Barnett (Dunedin).

SECTION OF EYE, EAR, NOSE, AND THROAT.

Advantages of the Prone Position in Operating for Adenoids.
Dr. Kent Hughes (Melbourne).

Exhibition of Mastoid Burrs.
Dr. A. J. Brady (Sydney).

Reflex Nasal Neuroses.
Dr. G. H. Hogg (Launceston).

Two Cases of Retro-naso-pharyngeal Abscess.
Dr. J. Lockhart Gibson (Brisbane).

SECTION OF MIDWIFERY AND GYNÆCOLOGY.

Saving of Blood in Gynæcological Operations.
Professor Watson (Adelaide).

Puerperal Eclampsia and its Treatment.
Dr. E. A. Falkner (Toowoomba).

Professor Watson exhibited a remarkable specimen of a hermaphrodite child, which was born in Melbourne, and died when 5 years old.

SECTION OF PUBLIC HEALTH.

Fertility of the Unfit.

Dr. Chapple (Wellington, New Zealand).

Rib Fracture in the Insane.

Dr. J. F. Flashman (Callan Park).

RECEPTION BY HIS EXCELLENCY THE GOVERNOR AND LADY LAMINGTON.

During the afternoon His Excellency the Governor and Lady Lamington gave a garden party in honour of the visitors to the Intercolonial Medical Congress. The guests, of whom there were over 1,000, were received on the lawn, while refreshments were served in various large marquees. It was a very brilliant function.

LECTURE BY PROFESSOR ANDERSON STUART.

During the evening Professor T. P. Anderson Stuart, M.D., Dean of the Faculty of Medicine, Sydney University, delivered a lecture in the Protestant Hall on "Flies and their Allies in the Spread of Disease." There was a large attendance. Dr. John Thomson, President of the Medical Congress, occupied the chair, and briefly introduced the distinguished lecturer.

Professor Anderson Stuart's lecture was illustrated by means of lantern slides. The first fly alluded to was the ordinary house fly. This, said the lecturer, was the most widely diffused of all flies in nature. The common fly could not bite, but sucked its sustenance. The insect was covered with minute hairs; so that wherever the fly alighted the substance with which it came in contact became entangled in the hairs. Consequently, where the fly next rested footprints were left. It could easily be seen, therefore, that bacilli could be conveyed by the fly, and thus spread disease. Organisms which caused typhoid fever, consumption, cholera, diphtheria, and others, were illustrated, and it was shown how the fly could carry the germs of consumption into butter or jam. The person eating these of course swallowed the germs. The mosquito was next mentioned. It was explained that a large number of mosquitoes were found in England, where they were known as midges or gnats. It was erroneous to suppose that the recent "alleged plague of mosquitoes" in England came from West Australia in jarrahwood. It was impossible—therefore untrue. For a long time it was supposed that female mosquitoes, after maturing, fed, laid her eggs, and then died. To disprove that had been left to a Brisbane man—Dr. Thomas Bancroft. (Applause.) He had found by experiment that the mosquito, if fed, would live at least as long as five months. The mosquito had an important connection with the carrying of disease. It was shown that elephantiasis was due to the presence in the blood of an embryo worm, *Filaria sanguinis hominis*, sometimes called *Filaria Bancrofti*. (Applause.) The parent of the worm was first found by the late Dr. Joseph Bancroft, whose photograph was thrown upon the screen, being greeted with loud applause. The lecturer said that Brisbane people did not realise what a great man the late doctor was. He had an enormous amount of work to do, and he was so busy with his work that he forgot to write about it. But he was a really great man of science. Dr. Thomas Bancroft had proved definitely the connection of the mosquito with *filaria*. One of the greatest authorities in England, Dr. Manson, held that the worms got into the chest muscles of the mosquito, and, when the mosquito died, he thought the creatures still lived, getting out of the mosquito's body into the water, where they continued to exist. Dr. Manson's theory was that man in drinking the water swallowed the worms. Dr. Thomas Bancroft, however, had proved that when the mosquito died in the water the *filaria* died too, as it could be found dead in the tissues of the mosquito. Therefore, in whatever way the little worm came from the mosquito to man, it was probably not through the contamination of drinking water. When the embryo worm got into a human being, it found its way to the lymphatic vessels, where, like a sensible *filaria*, it settled down to a happy married life. The result was numerous little embryo worms, and in a single drop of blood as many as 200 might be recognised. The habits of the three species of *Filaria sanguinis hominis—nocturna*, *diurna*, and *perstans*—were described in a most interesting way. The tsetse fly of South Africa was mentioned, the professor stating that the wild animals of the country were immune from the pest, while imported animals, such as horses and cattle, nearly always died from its effects. The lecturer showed that fleas were the means of communicating the bubonic plague to rats, which in turn conveyed the scourge to mankind. A brief allusion was made to the kissing bug of America. With regard to the cattle tick, so well known in Queensland, Professor Stuart said that the tick was not the cause of the fever, but the carrier of the little organism, which it conveyed to the blood of the beast. The subject of malarial fever was dwelt upon, the agency of flies and mosquitoes in the carrying of germs of disease, and spreading it over large areas, being forcibly shown. The lecture was one possessing extreme interest, the professor succeeding in his endeavour to choose his language so that on the one hand he would make himself clear to the laymen, and on the other hand so that he would not unduly bore the professional men.

The Chairman proposed a vote of thanks to the lecturer. In reference to the natural history of the mosquito he mentioned the labours of Mr Colledge, of Brisbane, who probably knew more about that insect than any man living. His name deserved to be coupled with that of the Bancrofts. (Applause.)

The vote was carried by acclamation, and Professor Anderson Stuart briefly responded.

FIFTH DAY—FRIDAY, 22ND SEPTEMBER, 1899.

At 10 a.m., in the Lecture Theatre of the Technical College, Dr. W. U. O'SULLIVAN (Melbourne), President of the Section of Midwifery and Gynæcology, delivered his presidential address. On the motion of the President of the Congress, a hearty vote of thanks was tendered to Dr. O'Sullivan.

The following papers were then read and discussed in the various Sections:—

SECTION OF MEDICINE.

Dr. Scot-Skirving, president of the Section, having left Brisbane, Dr. Power, of Sydney, occupied the chair.

Acute Yellow Atrophy.
Dr. A. JARVIE HOOD (Sydney).

Origin of Sex.
Dr. A. F. DAVENPORT (St. Kilda).

Influence of Hæmorrhage on the Course of Pulmonary Phthisis.
Dr. HIRSCHFELD (Brisbane).

Notes on Empyema.
Dr. G. T. HOWARD (Melbourne).

Dr. TAYLOR showed a patient suffering from a nervous affection.

SECTION OF SURGERY.

Adjourned discussion on Talipes Equino-varus (Congenital).

The Etiology and Treatment of Inguinal Hernia in the Young.
Dr. R. HAMILTON RUSSELL (Melbourne).

Operative Treatment of Inguinal Hernia.
Dr. H. V. CRITCHLEY HINDER (Ashfield, Sydney), read by Dr. PAIN (Sydney).

At the close of the discussion Dr. MCCORMICK, who presided, referred in terms of praise to the work done by the Secretary, and to the kindness and hospitality with which the members of Congress had been received. A vote of thanks was accorded to the Secretary.

SECTION OF EYE, EAR, NOSE, AND THROAT.

Quinine Amaurosis.
Dr. KIRKLAND (Sydney).

Congenital Sarcoma of Eyelid.
Dr. KENT HUGHES (Melbourne).

Excision of Larynx.
Dr. W. F. TAYLOR (Brisbane).

Operation for Conical Cornea.
Dr. FERGUSON (Newcastle).

Aural Vertigo.
Dr. LOCKHART GIBSON (Brisbane).

SECTION OF MIDWIFERY AND GYNÆCOLOGY.

Two cases of Uterus Bicornis.
Dr. PINNOCK (read by Dr. W. F. Taylor).

Notes of two cases in which Pregnancy followed Ventrifixation of the Uterus.
Dr. RALPH WORRALL (Sydney).

Record of Eight Years' Midwifery Practice in New Zealand.
Dr. J. R. PURDY (Wellington, N.Z.).

Early Rupture in Ectopic Gestation.
Dr. W. S. BYRNE (Brisbane).

SECTION OF PUBLIC HEALTH.

Discussion on the Contagious Diseases Act.
Discussion on the Venice Convention.
Diphtheria Mortality and Effect of Antitoxin on the Death Rate.
Dr. A. JEFFERIS TURNER (Brisbane).

The Use and Abuse of Hospitals in Queensland.
Dr. A. C. HALFORD (Clermont).

Sewerage and Health.
Dr. T. MAILLER KENDALL (Sydney).

THE MEDICAL CONGRESS ENTERTAINED BY THE PREMIER.

The Premier (the Hon. J. R. Dickson) gave a river picnic during the afternoon on the steamer "Lucinda," in honour of the Intercolonial Medical Congress. The weather conditions were all that could be desired, and the function was as enjoyable as it was successful. The attendance was worthy of the occasion. The Premier's hospitality was enjoyed by between 300 and 400 ladies and gentlemen. All the officers and members of the Congress, and the ladies who had accompanied them, were present, and the local ranks of the profession were influentially represented. The Premier received his guests on the deck of the steamer, and among them were:—Hon. A. Rutledge (Attorney-General), Hon. R. Philp (Treasurer), Hon. W. H. Wilson (Postmaster-General), Hon. J. F. G. Foxton (Home Secretary), His Honour Sir S. W. Griffith, C.J., His Honour Mr. Justice Real, the Right Hon. Sir H. M. Nelson, Hon. A. Morgan (Speaker), Hon. G. W. Gray, Hon. T. Macdonald-Paterson, Dr. John Thomson (President of the Congress), and Lieutenant-Colonel Ricardo. The proceedings were entirely of a social character. The steamer left the Queen's Wharf shortly before 3 o'clock, and proceeded down the river as far as Lytton. The return journey was then commenced, and town was reached at about 6 o'clock. The outing was of a very pleasant character. The only matter for regret was that the afternoon passed so quickly. As the ladies and gentlemen disembarked, they gave three hearty cheers for the Premier. The proceedings during the afternoon were pleasantly varied by the musical selections rendered by the Headquarters Band.

SCOTTISH CONVERSAZIONE.

Mrs. Nisbet and the Rev. T. Nisbet, of St. Paul's Presbyterian Church, Leichhardt street, during the evening tendered a conversazione to Scottish students attending the Medical Congress, and to friends. The schoolroom, in which Mrs. and Mr. Nisbet received their guests, was very tastefully decorated. A very large number accepted the invitations, included amongst the guests being very many of the medical men, Dr. Thomson (President of the Congress), Professor Anderson Stuart, and others. There were also present: The Hon. R. Philp (Treasurer), Hon. J. Murray (Minister for Railways), Mr. Forsyth, M.L.A., Mr. J. F. Thallon (Deputy Railway Commissioner), Mr. Stanley (Engineer in Chief for Railways), Colonel Ricardo, Mr. James Munro, Mr. James Chapman, Mr. J. Macdonald, Mr. John Evans, and others. Musical items were given by Messrs. Mason, Pasker, Ewart, Good, Dr. Maguire, and Dr. Byrne, and recitations by Dr. Nash (New South Wales) and Dr. Purdy (New Zealand). The intervals were enlivened by selections by Truda's band, and refreshments were also supplied. A very pleasant evening was spent.

SIXTH DAY—SATURDAY, 23RD SEPTEMBER, 1899.

The various Sections met at 10 a.m.

SECTION OF MEDICINE.

Dr. TURNER (Brisbane) exhibited several cases of lead poisoning.

SECTION OF EYE, EAR, NOSE, AND THROAT.

Recommendations, on the motion of the President, Dr. LINDO FERGUSON, were adopted for transmission to Congress.

SECTION OF PUBLIC HEALTH

Various recommendations were adopted for transmission to Congress, and during the consideration of a letter from the W.C.T.U. of Australia in reference to the repeal of the C. D. Acts, on the motion of the President, Dr. ASHBURTON THOMPSON, that the Section could not agree with the W.C.T.U., a short discussion took place.

Dr. **Carruthers** (New South Wales) said that the Act was very necessary in seaport towns. Having had to deal with the unfortunate creatures in Sydney, where there was no Act in force, he could say that great ravages had been committed. He considered that the legislation was needed.

Dr. **Chapple** (New Zealand) seconded the motion. He, however, was not satisfied with the administration of the Acts. He knew a surgeon who had been to India, having gone there prejudiced—as most medical men—in favour of the C.D. Acts, but, having observed its operations among the troops, had returned to England an opponent of the Act. It was necessary that they should endeavour to educate people and promote legislation for the prevention of that particular disease, as they did for the prevention of tuberculosis or any other ailment. The fact that a moral element came in should not deter them from that course. The Acts, however, had not been a success in those parts of the world in which they had been adopted. The surgeon he had referred to had declared that it was absolutely impossible to prevent the spread of the disease, and said that the police were in collusion with the class that was undesirable. He (the speaker) was not satisfied with the Act as it had been administered, and he did not wish the Section to commit itself by making an assertion that the Acts were giving entire satisfaction.

The **Chairman** said that the W.C.T.U. had asked them to express a wish that all C.D. Acts should be repealed. He was glad to see from the applause that the sense of the meeting seemed to be with the motion.

The resolution was carried.

ADJOURNED GENERAL MEETING.

At 11 a.m. the adjourned general meeting of Congress was held in the Lecture Theatre of the Technical College.

The chair was occupied by the President (Dr. John Thomson), who, in taking his place, received quite an ovation. The General Secretary (Dr. Wilton Love) was greeted in a similar manner. The first business consisted in receiving the reports of the various Sections.

SECTION OF PUBLIC HEALTH.

Dr. **Ashburton Thompson** (President of this Section) submitted the following recommendations as having been adopted by that Section :—

CONTAGIOUS DISEASES ACTS.

" The Fifth Intercolonial Medical Congress, having considered a letter from the Women's Christian Temperance Union of Australia in reference to the Contagious Diseases Acts, is quite unable to agree to the repeal of the measures which science and experience have devised and the Legislatures have adopted for lessening the ravages of a disease which is transmitted by its victims to innocent mothers and children, causing in them many serious and fatal affections. The Intercolonial Medical Congress begs to remind the Women's Christian Temperance Union of Australia that these measures are enforced not in the interests of the vicious and depraved, but for the protection of mothers and children. The Intercolonial Medical Congress, while unable to agree with the Women's Christian Temperance Union as to the medical methods of preventing this disease, begs to assure the Women's Christian Temperance Union that the medical profession throughout Australia will hail with pleasure every auxiliary effort made by the Women's Christian Temperance Union of Australia for the diffusion of a correct moral tone among the community."

ARMY MEDICAL CORPS.

"That this Section recommends Congress to appoint a board consisting of the principal medical officers, and another, a member of the Army Medical Corps, of each colony, with power to add to their number; such board to act in an advisory capacity with regard to the peace and war requirements of the Army Medical Corps. This board to report on this matter to each succeeding Congress."

ABUSES OF THE CHARITABLE INSTITUTIONS.

"That this Section requests Congress to forward a memorial to the Governments of the respective colonies, setting forth the abundant evidence available of abuses of the charitable institutions, and that, in the interests of the State, radical changes in the constitution, the management, and the maintenance of such is urgently indicated."

ISOLATION HOSPITALS.

"That isolation hospitals should be opened as separate establishments, apart from and not adjacent to general hospitals, because of the extreme danger of infectious disease being communicated to the patients in the wards."

COMPULSORY NOTIFICATION AND ISOLATION OF INFECTIOUS DISEASES.

"That in the opinion of this Section, compulsory notification and isolation of infectious diseases by proper hospital accommodation are of such importance to the health of the community as well as to the best interests of trade and commerce, that this Congress respectfully submits to the attention of the colonial Governments the desirability of enforcing notification and of providing proper accommodation in the various centres of population."

RESOLUTIONS AGREED TO.

The resolutions were agreed to unanimously, with the exception of that in favour of the establishment of separate isolation hospitals. To that Dr. Ashburton Thompson said he could not give his support, because he did not consider it practicable under the present circumstances of the colonies. It seemed to him that to insist upon having infectious hospitals entirely separate from general hospitals would be to prevent the establishment of infectious hospitals for many years until the population increased.

VENICE CONVENTION.

The report of the sub-committee of the Section of Public Health upon "Whether the Congress should advise the Hon. the Premier that the Executive Government of Queensland should signify its adherence to the Venice Convention of 1897 or to any part thereof," was then submitted. The sub-committee consisted of Dr. Ashburton Thompson, representing the Government of New South Wales; Dr. T. Cahill, the Government of New Zealand; Dr. L. W. Bickle, the Government of South Australia; Dr. G. Sprott, the Government of Tasmania; Dr. W. L. Watkins, the Government of Victoria; and Dr. Wilton Love, the Government of Queensland The unanimous report was as follows:—"(*a*) That as the Convention orders that persons who arrive on 'suspected' ships shall be dismissed to their destinations, there to be kept under 'surveillance' for a term of ten days, it would be inexpedient for the Government of Queensland to adhere to the Convention in its entirety, that method not being sufficiently stringent in the actual conditions met with in Queensland. (*b*) That the Government of Queensland might and should signify its adherence to the first part of Chapter I. and to Chapter II., Titres I. and II., of the Convention, which relate to notification of any outbreak of plague within its territories and of the subsequent progress of the disease to the foreign Governments which are parties to the Convention and to neighbouring countries."

SECTION OF EYE, EAR, NOSE, AND THROAT.

Dr. Lindo Ferguson, President of the Section, reported the following recommendations from the section :—

1. That in the opinion of this Section it is highly desirable, both in the interests of the children themselves and of the future of the race, that the incidence of educational work should be much lightened for girls between the ages of 12 and 15 years by reduction of the school hours and home lessons, and that their physical development should be aided by appropriate outdoor physical exercises as part of their school course.
2. That it is desirable that school teachers in training schools receive such instructions as may lead to their recognition of symptoms of their pupils indicating defective vision, hearing, or other physical condition calling for medical attention.
3. That the attention of the colonial Governments be called again to the resolutions passed by the Intercolonial Medical Congress held in Dunedin in 1896 on the subject of the standard of vision in sailors and railway men, and that the urgency of their adoption should be again emphasised, with the recommendation that a uniform standard for all the colonies be fixed, not lower than the highest standard at present demanded in any of the colonies for both form and colour.

The recommendations were adopted unanimously.

QUARANTINE.

Dr. Eames moved,—"That in view of the early federation of the colonies, and the enormous commercial interests at stake, this Congress recommends to the colonial Governments the advisableness of holding a conference at an early date, at which commercial representatives from every colony should be present, to point out to the various medical experts the dislocation in trade and disabilities to shipping caused by the imposition of quarantine ; and that the Premier of Queensland be requested to communicate with the other Premiers, and arrange the time and place of such conference."

Dr. Purdy seconded the motion.

Some objection was taken to the motion being received, as it had not come as a recommendation from any Section, and could not be debated. Ultimately the President decided to put the motion; and the sense of the Congress being taken, the motion was negatived by 33 votes to 24.

NEXT SESSION OF CONGRESS.

Dr. Bright (Hobart), on behalf of the medical profession of Tasmania, extended a cordial invitation to hold the next Congress in 1902 in Hobart. He moved a motion to that effect, and advised the Congress to sit in March.

Dr. Wolfhagen (Hobart) seconded the motion, which was carried amid applause.

The President stated that an invitation had been received from South Australia, in the event of Tasmania's invitation being declined.

It was agreed to thank the South Australian Association for its invitation.

ELECTION OF PRESIDENT.

On the motion of Drs. Gregory and Byrne, Dr. Bright, of Hobart, was unanimously elected President of the next Congress, amid loud cheers.

Dr. Bright returned thanks, and was congratulated by the President.

VOTES OF THANKS.

Professor WATSON proposed,—"That a vote of thanks for the kindness, courtesy, and boundless hospitality they have displayed—actions appreciated to the full by all—be given in writing to His Excellency the Governor, the Hon. the Premier, the Minister for Railways, the Bishop of Brisbane, Bishop Stretch, Rev. T. and Mrs. Nisbet, the Mayor and Mrs. Seal, the President of the Medical Society of Queensland, President and Members of the Brisbane Branch of the British Medical Association, Queensland Club, Johnsonian Club, Golf Club, Tennis Club, the Art Society, Mrs. Pinnock, and the Press."

Dr. JARVIE HOOD seconded the motion, which was carried with cheers.

On the motion of Drs. HARDIE and DAVENPORT, a vote of thanks was passed to the sectional officers. A similar compliment was also, at the instance of Drs. WORRALL and GREGORY SPROTT, accorded to the members of the Executive Council, to which the Hon. Dr. TAYLOR responded.

Dr. SYME proposed a hearty vote of thanks to the General Secretary, Dr. Love, whom he eulogised for his excellent work. He had been to all the congresses, with the exception of the first, and he could say that on no previous occasion had the minutiæ of details been attended to as thoroughly as during the present Congress. (Cheers.)

Dr. BRADY seconded the motion, which was carried enthusiastically.

Dr. LOVE acknowledged the compliment.

Dr. LINDO FERGUSON proposed a cordial vote of thanks to the President (Dr. Thomson), whom he praised for his splendid work during the session, and for his kindness to all the members. He coupled with the motion the name of Mrs. Thomson.

The motion was seconded by the whole Congress, and was carried with vociferous cheers.

The PRESIDENT said that he felt more than he could express. He had written to the general secretary of the New Zealand Congress: "I very humbly pray that my efforts in connection with the Brisbane session will warrant the confidence reposed in me." They had found their verdict, and he accepted it. (Prolonged cheering.)

The whole of the members stood up, and sang "For He's a Jolly Good Fellow," after which cheers were given for Dr., Mrs., and Miss Thomson.

The PRESIDENT declared the Congress closed.

BACTERIOLOGICAL DEMONSTRATIONS.

During the sitting of the Medical Congress daily demonstrations have been given by Mr. C. J. Pound, the Government Bacteriologist, in the room set apart for use at the museum. Needless to say, a very keen interest was manifested in what Mr. Pound had to say and to exhibit. He had on view specimens illustrative of the various forms of tuberculosis in the lower animals —the ox, the hare, the sheep, the pig, the goat, the cat, the rabbit, the guinea-pig, fowls, geese, turkeys, and the emu. Mr. Pound also gave demonstrations of the preparation and the use of tuberculin, and the cultivation and use of economic micro-organisms, such as the bacillus of mouse typhoid, used for the destruction of mice; chicken cholera, used for the destruction of rabbits; and locust fungus, employed against locusts and grasshoppers. Among the tubes exhibited were some containing silk threads impregnated with the spores of the anthrax bacillus. These had retained their fertility after being desiccated for thirteen years. Tetanus organisms in wood splinters, which remained virulent five years after desiccation, and the bacillus of blackleg, which continued active four years after that treatment, were also on view, together with a large collection of the cultivations of various forms of pathogenic and non-pathogenic bacteria, and specimens of ticks and tick-infested hides. The members of the Congress were greatly interested in Mr. Pound's demonstrations, and his Section was well attended throughout.

MEDICAL PICNIC.

A very enjoyable picnic up the river was given to the members of the Intercolonial Medical Congress and their friends on Saturday afternoon by the Queensland Branch of the British Medical Society. Five steamers were chartered for the occasion, and they were all well filled, carrying about 800 guests. The excursionists were taken as far as the Fifteen-mile Rocks, and there they disembarked. The river scenery, looking as it did almost at its best, was greatly admired by those who saw it, as many did, for the first time. The home journey in the cool of the evening was exceedingly pleasant. The wharf was reached about 7·30 o'clock.

JOHNSONIAN CLUB.

In the evening about fifty of the visitors, in response to a general invitation, were entertained by the medical members of the Johnsonian Club to supper.

MONDAY, 25TH SEPTEMBER, 1899.

The Prime Minister (Hon. J. R. Dickson) placed the steamer "Miner" at the disposal of the executive committee of the Congress. The steamer left the wharf at 8·45 a.m. with twenty visitors, and Dr. Love acted as host. During the trip down the river the Eagle Farm Meatworks were visited, and an inspection of the methods of work made. After crossing the Bay, the lazarette on Stradbroke Island was visited, and the unfortunate patients were examined, and this was done with much interest. Leaving the leper station, Dr. Smith, of Dunwich, conducted the visitors over the asylum. The steamer returned to town at about 8 o'clock, all on board delighted with the trip.

[Finis Coronat Opus.]

SECTION OF MEDICINE.

AN ADDRESS DELIVERED TO THE SECTION OF MEDICINE.

By R. Scot Skirving, M.B.,

PRESIDENT OF THE SECTION,

Lecturer in Clinical Medicine in the University of Sydney, and Honorary Physician to the Prince Alfred Hospital, Sydney.

Gentlemen,—

My first duty is to thank you for the honour you have done me in placing me in the presidential chair of the Section of Medicine. I feel, on the one hand, that I have received a great honour, and on the other, one which is very difficult to live up to. Much is rightly expected of anyone who is placed by his fellows in this position. I therefore ask you to look leniently on my shortcomings, and to accept the will to do well when the deed falls short.

I must now welcome the members of this Section of Medicine to what I trust may prove a useful and instructive session.

One of the duties of a president, which time and custom have crystallised into a necessity, is the delivery of an address, either purely technical, or at least bearing on our common art. The custom has its merits, but it has added another terror to the acceptance of official position, at least, to men of my temperament. I have had many inward questionings as to the subject on which I should speak to you. One thing only was I sure upon—that the address should be short.

I put purely technical matters aside, for although many lie to one's hand, they are best treated in the ordinary work of the Section. Again, I thought of certain matters of general scientific interest, especially the advance of human knowledge in certain special lines. But most of the subjects of this class have been dealt with so well, and so much better than I could hope to handle them, that I decided to take something more at home to all of us. I therefore propose to offer a few remarks on a subject which every day of professional duty brings to our notice—"The Influence of Modern Methods on the Practice of Medicine."

I think we must all agree that to be a good physician, "to practise a sublime art not frivolously," one must be grounded, not only in the sure foundation of anatomy, physiology, and pathology, but likewise habits of close clinical observation must be cultivated, patient ordering of facts noted, combined with a shrewd sifting of chaff from grain before finally arriving at a diagnosis or a method of treatment. Especially is it necessary to arrive first at a working diagnosis. I am far from sharing the view of an old Scotch lady who wanted a change of medical adviser because her own doctor "wasna able to pit a name to her tribble." Still I would urge that a strong effort be made to reach some definiteness of diagnosis in every case. The frame of mind of the man who is content to drift, to tell the patient's friends that so and so "is suffering from an irritation in the neighbourhood of the stomach," as an excellent old practitioner in Sydney used to do, is not one to be imitated. His soul must surely be steeled against many tragic surprises, or else he is of all men most miserable. Having said so much in praise of exactitude, I return to the means to gain that safe ground of sureness. I would urge and re-urge my hearers, especially those who still have the happiness to be young, to make themselves masters, and by habits of constant observation to retain that best possession of our fathers in medicine—CLOSE CLINICAL OBSERVATION OF SIGNS AND SYMPTOMS—and not fall in with a tendency of this progressive age of flying after new things to the forgetting or disregarding of the old and tried. I must here beg my listeners to acquit me of being a reactionary and of failing to hail with joy the rapid advances in medical science; but I wish to take advantage of my present position to sound a note of warning that we are somewhat inclined to run riot in our new learning, and to forget "the long result of time" which has led up to our present vantage ground. Let us take an example: Widal's test is one of the most striking examples of recent advance, yet, reliable as it is, it is not absolutely so. Certain reservations concerning it are familiar to all of us who try to follow the advance in bacteriological work. And even if, in every case, the information so gained was absolutely conclusive, I would still say that Widal's test is, so to speak, the coping-stone of the diagnosis, and not the edifice itself. I hold it is the duty of all of us, as in times past, to carefully hear the history, to note pulse, temperature, respiration, tongue, skin, &c., just as our fathers did, and then, after a judicial weighing of the differential diagnosis, to arrive at a conclusion based on the old simple lines of clinical observation. Then, if doubt remains, we can call in the modern aids, and, as I well know, in most instances we can rely upon them. Instead of the plan I have just sketched being pursued, I now come across instances where no faithful study of the case has been made; but the practitioner, opinionless, because no effort to find the premises on which to found an opinion has been made, calls in Widal's test, or some similar modern aid, to make up his mind for him. If this condition of things were carried too far clinical observation might verily become a lost art.

Röntgen's discovery has opened up a new world of possibilities. Its use is manifest already in a number of conditions, but it need never absolve us from doing our best to diagnose an injury to the elbow on the lines which Astley Cooper or Hamilton would have taught us. Even the witness of a skiagram often only gives information which is "seen through a glass darkly," and needs clinical confirmation. Nor must the imperative call to seek for the organism of Weichselbaum by lumbar puncture, or to scan the blood cells for the *Plasmodium malariæ* make us neglect the ordinary clinical features of cerebro-spinal fever or pàludism.

Again let us turn to Tuberculosis. I regard the discovery of the tubercle bacillus as one of the greatest in the history of medicine, and its discoverer as one of the greatest of men. Nevertheless, the examination of the sputum for the tubercle bacillus and other micro-organisms to establish the diagnosis of tuberculosis, or of a mixed infection, is not quite the be all and end all of the inquiry into a case of phthisis. I once was asked by a bacteriological enthusiast how I diagnosed a case of consumption? I therefore suggest to the rising generation that inspection, palpation, percussion, and auscultation, with likewise an attentive ear to the personal and family history, may be found useful aids in arriving at a conclusion in the absence of a microscope and suitable stains. These admirable adjuncts find a fitting use when the other and older routine has been carried out. As yet I see no justification for the refusal of a very up-to-date practitioner to prescribe for a slight case of bronchitis in a child till he had examined the sputum, "to exclude tuberculosis," as he told the anxious mother.

Next let me refer to the use of Tuberculin. Far be it from me to shout with the crowd and decry its use on insufficient grounds. The use of Tuberculin is a landmark in Therapy, almost as epoch-making as Jenner's immortal discovery, but its merit seems just now to lie more in its suggestiveness of great things to come, of an ever-widening arc of therapeutic possibility, rather than at the moment of a successful method of cure.

In this connection, as well as towards new remedies generally, a wholesome attitude of waiting for light, if not carried too far, is, to my mind, the best for the profession, and, I may add, for the public as well. We ought not to be like the people of Athens—seeking after a new thing. A too easy and credulous optimism about new methods arouses hopes in the lay breast doomed to early disappointment. Take the new Tuberculin. In some quarters it was practically stated in almost so many words that, within six months of the publication of Koch's paper, our efforts to provide open air treatment for consumptives in suitable climates would be largely unnecessary; and it is a good while now since this occurred.

Synthetic pharmacy has flooded us with a hundred substances with three-decked names. A few, a very few, are likely to be of permanent value. The greater number, in company with countless vegetable preparations with American testimonials, are equally lauded and equally useless. While on the subject of new remedies, there is another development in the matter of pharmacy which has its bad points as well as its good. It is this: that the trend of modern drug-giving is to hand over a part of our business to the manufacturing chemist. I am very ready to agree that the poly-pharmacy of the past was often unscientific and harmful, a filling of the patient's stomach with an array of nauseous drugs, which either had no action at all or were not seldom harmful. I never had any sympathy with an old practitioner in Sydney, now long dead, who put as many as nineteen ingredients in one prescription. On being remonstrated with, he said, in defence, "that one of them would hit." I also utterly disapprove of the man who has a prescription handy for every ill that flesh is heir to; the type of man the late Professor Badham once defined "as a fellow who prescribes drugs—not a doctor." On the other hand, are we not letting some of our knowledge of pharmacy—of rational pharmacy, of the suitable combination of drugs—slip from us. I now occasionally see prescriptions such as the following:—

℞ Pil 4291 (A)
℞ Mist X Y (my formula)

and the like. This kind of prescribing, no doubt, saves time, but there is something mysterious, something not to be understood, except by the prescriber and his special chemist, which grates on my sensibilities, and which seems contrary to the best traditions of our profession. We all learn at times from the practice of each other, even from a casual prescription, and I must be honest and confess to a reasonable curiosity as to what the last doctor ordered.

One of the glories of medicine is the information the microscope reveals to us in pathological anatomy; but I must here again insist on the value of the clinical aspect as a right balance to the microscopical examination. Take the diagnosis of sarcomata by the microscope. We are not yet on the safe land of certainty, and it is wise to very fully blend the clinical aspects of the case with the pathological report before acting radically on either. I well remember my revered master, the late Professor Spence, telling me to get some very evil-looking tissue examined. I did so, and reported to him that it was pathologically a sarcoma with a very long name. "Dear me," said Mr. Spence, with a dry chuckle, "I thought it was granulation tissue. Suppose we rub it down with bluestone." And granulation tissue the sequel showed it to be.

Mr. Mayo Robson has lately published an admirable address on "The Dangers of Delay," with which I am in entire sympathy, but I am equally sure that a very good homily might be preached on "The Dangers of Precipitancy." In no former period have medicine and surgery in one sense been so closely interwoven. Surgery, as we all know, now invades territories long sacred to the pure physician, and this overlapping is well; but with it, and with the astonishing success which asepticism and anæsthesia have brought, there is a risk of our forgetting that, after all, an operation is a great and sorrowful evil. I am sure that, in some cases, operations are embarked upon in too light a spirit and without the weighty justification which such measures ought to demand. Just as in matters purely medical there is a seeking after a short and sometimes not "a more excellent way," so in those cases where medicine and surgery are alike concerned, there is, at times, a precipitancy, a lack of effort to get at the bottom of affairs by clinical examination, a too easy settlement of doubt by an incision. Palpation and percussion might some day become obsolete knowledge, and Dr. Cope's forecast of the "clinician of the future sitting, out of employment, alone with the stethoscope he had fondled for years draped in black" has some justification.

The Germans, as a rule, do things slowly and solidly. We are told they even bring jokes out in parts, but they seem to be going more rapidly of late. As an aid to memory, a short cut to knowledge, a Dr. Friedrich Fuchs has lately published a system of medical mnemonics I give you a sample of how he wishes us to remember the symptomatology of locomotor ataxia:—

Lanpapuron priguranbypolei
Janglag opoc bladas kritro appei.

This form of nourishment is a little too concentrated for me, and I still prefer the lengthier pabulum presented, say, by *Ziemssen's* Cyclopœdia.

But this haste to know is found in many other branches of learning. Take navigation. There the seaman is swamped with all sorts of tables and mechanical short-cuts to expedite calculation—no doubt some of them very useful but quite inimical to his gaining the rudiments of trigonometry—the science on which his art is founded. Take the instance of so great and striking a figure as Nansen. An explorer is expected, like Bacon, "to have made all knowledge his province." And what apparently befel Nansen for want of a due amount of trigonometrical knowledge? When his timekeepers stopped, during his marvellous journey over the ice, he was unable to find even an approximately correct Greenwich date and so deduce his longitude.

In the hands of a skilled few a justified precipitancy may do no harm, but in the hands of the many, nearly as great evils may arise from precipitancy as from delay. Therefore, I hold that we should exercise a wise conservatism. Let us by all means operate as early as possible in suitable cases, but let us at least try our best to make out what is a suitable case before doing so. When we have failed to satisfy ourselves on that point by ordinary means of examination, let us then make an exploratory incision, but not before.

There is at present a tendency to hanker too much after the dramatic and larger events of medicine and surgery. So it is well to remember that the bulk of the profession will be occupied more often with the humbler details of practice. Ordering an enema, giving a comforting cough mixture, or opening an abscess must occur more frequently than diagnosing a cerebral tumour or excising a larynx. We do not live only for ourselves, but for our patients. As Louis Stevenson finely says, our mission is "to bring air and light into the sick room, and often enough, but not as often as we wish, to bring healing." It is right, then, that what we do often we should try to do well. Little minutiæ of practice are all important in making a sound and useful practitioner. There is nothing mean in such knowledge and such things. I like to recall good George Herbert's pious lines, in which he says that a right spirit

"Makes drudgery divine
Who sweeps a room, as for Thy laws
Makes that and th' action fine."

I am beginning to doubt if the diffusion of information on every conceivable subject to the masses is an unmixed blessing. We are getting too many half-educated people in every walk in life, and above all concerning ours, where ever ignorant know-all in the street thinks he is a capable critic of things medical. One must either weary oneself by trying to tell such persons the whole truth—if they will take it in—or else leave them to their own wrong opinions to worry their doctors. As it is just now, many neurotic women, some of the clergy, and ladylike men generally, all usually persons with the little knowledge which is dangerous, swell the ranks of anti-vaccination and anti-vivisection propagandism. The people who really understand these matters make no sign. And let me add to their credit, that the very ignorant usually accept our guidance in matters absolutely our own. But it is that half-educated class which give medical men trouble—not only in great questions such as vaccination, which affect a nation's welfare, but also in the parochial details of seeing patients. Cheap science and amateur medicine-mongering give some of the laity the idea that they know a great deal; and the result is that medical men are forced not seldom to treat the patient's fussy friends as much as the sick man! Here is an example of the kind of jumbled scientific knowledge possessed by the kind of people I refer to. Some time ago, I saw professionally a young man who was anxious to relieve himself of a very long and trying journey with disagreeable business at the end of it. He therefore, out of the fulness of his knowledge, concocted the following, which I was requested to sign, to obviate his going I was quite satisfied that the expedition would do him physical injury, but I was certainly not prepared to endorse the following verbiage :—

> That the patient is suffering from sub-acute cystitis and prostactic inflammation caused by an abnormal alkaline condition of the blood, such being continuous and accompanied by a deterioration and decomposition of organic cells forming alkaline and irritant deposits, and in my opinion this is primarily attributable to an exhausted state of the nerve-centres.

All the above, gentlemen, is a roundabout and incorrect way of saying that he had a specific inflammation of the bladder and prostate. Such is an example of the diffusion of scientific knowledge of a kind.

Time does not permit me to speak of good remedies passing into disuse. It is a theme already well handled in many addresses. It is enough for me to refer to the extinction of some of them as one of the many-sided effects of modern methods on our practice. But, not always do the fittest survive, nor is the early disappearance of the good an unknown event.

I must now bring these remarks to a close. I hope I have used this opportunity, not as a reactionary, thinking things old are good simply because of their antiquity, or resenting new methods because he objects to learning them. I wished simply to draw attention to the risks which go with successful advance. Let us hail all solid new knowledge with acclammation, but let us still hold fast to that which is proven and good—

"Lest we forget, lest we forget."

SOME POINTS IN THE TREATMENT OF DIPHTHERIA.

By J. Macdonald Gill, M.D. (Lond.).
Assistant Physician to the Children's Hospital, Sydney.

In *The British Medical Journal* for 23rd October, 1897, Dr. Clubbe published the results of the first 300 cases of Diphtheria, treated with Antitoxin at the Diphtheria Branch Children's Hospital, Sydney, and showed that the total death rate had been reduced from 52 per cent. to 20 per cent. I wish now to publish the further results from that date (12th November, 1896) up to the present time.

We have had under treatment 324 cases, of which 59 died, or rather (excluding one death from general tuberculosis) 58—*i.e.*, a mortality of 17·9 per cent.

> *Total cases*, 323; died, 58; tracheotomies, 100, of which died, 49; total laryngeal cases, 190.
>
> *Mortality.*—Total, 17·9 per cent; tracheotomy, 49 per cent.; mortality of other cases, 4·0.

In the previous list the mortality was 20 per cent., and the tracheotomy mortality 37·9 per cent., so that there has been a small improvement in the general mortality, and a considerable increase in the mortality after tracheotomy. This latter is due to the fact that a smaller proportion of cases have been tracheotomised, 30·96 per cent., instead of 43 per cent; increasing experience with the use of antitoxin showing that a larger proportion of the laryngeal cases can escape tracheotomy, and that only the severer cases require that operation. It will be noticed that the number of deaths after tracheotomy is exactly the same in each list—*i.e.*, 49.

I have no intention of trying to prove that antitoxin can cure diphtheria—that has already been fully proved; but what I wish to discuss is in what quantity, and how it is best given, and what other means we should adopt for treating the disease. First of all, it is necessary to consider the causes of death: I have tabulated these in our 58 fatal cases, and find that 26 died of asphyxia—*i.e.*, from membrane involving the trachea and bronchi—11 of broncho-pneumonia, and 8 of membrane in the tubes and broncho-pneumonia combined, 2 of post-diphtheric heart failure, 4 were moribund on admission, 3 were so ill-nourished that they very soon succumbed, 2 died while tracheotomy was being performed, and 1 died of scarlet fever. In 26 were *post-mortem* examinations made. Thirteen of these died of membrane in the tubes, without broncho-pneumonia; 9 of broncho-pneumonia, and 4 of broncho-pneumonia and membrane in the tubes combined. In the other cases the cause of death was inferred from the symptoms. I have not been able to satisfy myself that any died of Toxic Anuria (to which a fairly large proportion of deaths has been ascribed by Drs. Sidney Martin and Hunt—*B. M. J.*, 1898, September 3rd), the difficulty in collecting urine from small children dying of broncho-pneumonia or asphyxia being so great that I have thought such anuria to be secondary to the general condition. It will be seen, therefore, that about four-fifths of the deaths are due to extension of membrane and broncho-pneumonia, either singly or combined, and about half to extension of membrane only, without broncho-pneumonia. Now, it is very remarkable how membrane may involve the bronchial tubes as far down as one can trace them, and yet there may not be a single patch of broncho-pneumonia; such a condition as that, one frequently sees. Generally, those cases are relieved only partially by tracheotomy; the dyspnœa gets worse and worse, and the child dies after thirty-six or forty-eight hours, or may struggle on a little longer. The local application of trypsin, with the idea of dissolving the membrane, was formerly used, and occasionally resulted in bits of membrane being got up, but was generally of little use. It seemed to me, therefore, that the only way was to give larger doses of antitoxin, especially as it was certain that the doses (1,500 to 3,000 units) formerly given was not always sufficient to arrest the progress of the disease. The most obvious effect of antitoxin is to loosen membrane, and the more is given, the sooner membrane is loosened, and as antitoxin generally shows marked effects in twenty-four hours, the majority of these cases ought to be saved if enough serum is used. I therefore adopted the plan of giving immediately 15,000 units in all laryngeal cases requiring tracheotomy; and if dyspnœa persisted, giving 6,000 to 9,000 units more in six or eight hours, repeating the dose at twelve-hourly intervals if necessary. In this way two cases each received 36,000 units. (I may say that I have never seen any harm caused by the injection of such large doses.) In the great majority of tracheotomy cases one injection is sufficient.

In purely pharyngeal cases 1,500 to 4,500 or 6,000 units are given, according to the severity of the case and the extent of surface involved. In laryngeal cases 6,000 to 9,000 units, according as the case is mild or moderately severe. The injection of such large doses is rendered much easier by the increase in the strength of the antitoxin now supplied; the last lot sent out by Burroughs and Wellcome containing 375 units per cent., and that by the British Institute of Preventive Medicine 500 units per c.c., so that 15,000 units are contained in 40 or 30 c.c., according to the serum used. I look upon the extent of surface involved as being the best guide to the amount of serum required; and I think it wise to give such large quantities in every tracheotomy case, as one can never be sure at the time of operation as to whether, and to what extent, the bronchial tubes are involved. Previously to October, 1898, Behring's antitoxin was generally used, 1,500 units being usually given at once, and repeated in 12 hours if necessary; occasionally another 1,500 units was injected. Since then, we have used Burroughs and Wellcome's (or that of the B.I.P.M.) antitoxin in the larger doses above mentioned.

—	Total Cases.	Total Mortality.	Tracheotomy.	Mortality after Tracheotomy.	—
November, 1896, to October, 1898	249	55	81	47	Total Mortality, 22 % Trach. Mortality, 53 %
* October, 1898, to September, 1899	74	3	19	2	Total Mortality, 4 % Trach. Mortality, 10 %

* I have excluded the case that died of general tuberculosis.

It is a pity that the second series includes so few cases; but still I think that 74 are enough to base sound conclusions on. Of the three fatal cases, one was admitted moribund, and died in three hours of extensive broncho-pneumonia; the second died within fourteen hours of admission of similar trouble; and the third died of asphyxia at the end of 44 hours. In this last case 36,000 units of antitoxin were given, and oxygen inhalations were used freely. At the autopsy, almost every bronchial tube contained plugs of membrane, which, in most places, were loose and could be easily pulled out; some of the membrane in the trachea was still fairly firmly adherent. No membrane had been got up till just before death. It seemed that if the child could have been kept alive a few hours longer more membrane could have been got up, and she would have recovered. (There was not a single patch of broncho-pneumonia) We were encouraged in treating this girl by the case of a child shortly before, who had remained in a state of semi-asphyxia for three days, and then at last large pieces of membrane were got up, and she recovered completely.

It seems as if antitoxin required about two days to loosen the membrane in such cases as these; and, I think, it is apt to be very firmly adherent in the bronchi. This second child also had 36,000 units.

Having discussed the dosage of antitoxin, we now come to consider other means of treatment. Trypsin, I have already mentioned. I don't think that it has much real value, but still it may be used in a desperate case. But there is one aid to treatment that has lately occurred to me, and though its use is obvious enough, I have never seen it recommended for diphtheria—I refer to oxygen. It seemed to me that oxygen inhalations were likely to be of the greatest value in cases of persistent membrane in the tubes. In the two cases referred to above it was used freely, and gave great relief. It at once restored the child to a good colour, and enabled it to rest, and even to sleep. In the case of one it certainly prolonged life for some hours, and in the other it helped greatly in its recovery. The gas is allowed to escape by a gentle stream from a tube held over the tracheotomy tube. Perhaps it would be better to use some arrangement like the face piece of a nitrous oxide apparatus tied on the neck. However, the use of the tube answers the purpose very well. The gas is stored in a tank, containing water, with a capacity of 12 cubic feet; and when the supply runs short, fresh gas can be made in the iron retort provided. It is necessary to have an abundant supply of gas, and to have the means of making it at hand when required. The gas is also supplied compressed in iron cylinders, but then it becomes too expensive for general use. It is purified by passing it through solutions of Na_2CO_3, and $NaHCO_3$ to free it from chlorine. We have only used it in three cases: two referred to above and the third a hopeless case of broncho-pneumonia. It seems to me that it is likely to help the patient in those desperate cases of asphyxia due to membrane in the tubes; and that in many cases it will keep the patient alive till the antitoxin can loosen the membrane. After the initial expenditure of putting up the apparatus the only expense is getting fresh $KClO_3$ and MnO_2; to make 12 cubic feet of oxygen costs about 2s.

One advantage I forgot to mention in the use of large doses of serum is that the tracheotomy tube can be left out much earlier; hence there is less risk of trouble in finally getting the child to do without the tube, and also much less risk of subsequent broncho-pneumonia.

As to local treatment, I think it is of very little importance, and children do just as well without as with, and that, therefore, in troublesome cases it can be dispensed with; but I must except the cases in which the throat is very offensive or in a very septic condition, though even then the local application is of secondary importance to antitoxin. We generally apply 1-1000 $HgCl_2$ by swabs every four hours.

My object in writing this paper is to show that very much larger does of antitoxin should be given in severe cases, and that they are absolutely necessary to secure the best results. It seemed to me also that a mortality of 17 to 20 per cent. (which is about the average in most hospitals) is very high in a disease for which we have a specific remedy. We have been able to give these large doses by the use of Burroughs and Wellcome's antitoxin, which we have found satisfactory in every way. When the supply of this antitoxin runs short we have used that supplied by the British Institute of Preventive Medicine, and have also found that very satisfactory.

In the statistics furnished I have omitted any reference to the incidence of post-diphtheritic paralysis, as most of the cases were not kept in more than four weeks, and paralysis often develops after that period, rendering any such statistics of no value.

No case has been admitted to the list in which the Klebs-Löffler bacillus has not been found.

I add a tabulated list of the first 249 cases, according to hundreds, which shows that both the general and the tracheotomy mortality remained at about the same level during the two years:—

—	Cases.	Total Deaths.	No. of Tracheotomies.	Deaths after Tracheotomy.	Tracheotomy Mortality.
I.	100	22	31	17	55 per cent.
II.	100	23	34	20	58·8 ,,
III.	49	10	16	10	63·0 ,,

COMPLICATING INFECTIONS.

By A. C. F. Halford, M.D., Etc.,
Clermont Hospital, Queensland.

Mr. President and Gentlemen,—

I am afraid that my ability to do this subject even common justice will be severely taxed, and that my treatment of it will be of a very superficial and imperfect character.

Nevertheless, perhaps I will be able to indicate the direction in which this subject might have been pursued had it been in more capable hands.

I intend to treat a complicating infection as one or more secondary contagions superimposed or accompanying a specific condition in which the signs of specificity may be masked or obscured. Or further, complicating infections may occur during the course of diseases independent of micro-organisms. The probabilities of any ordinary infectious condition becoming a mixed one are considerable if we keep in mind the many modes of entrance for contagia.

Therefore when we are confronted with a case of infectious origin we must be careful that we do not regard the patient as a sterilised test-tube inoculated with some pure virus setting up some pure disease. Nor can we infallibly provide against the entrance of other pathogenic or non-pathogenic microbes. It is probable that many conditions which we regard as the result of an uncomplicated infection may depend for their want of general uniformity upon two or more infections unequal in their dosage numerically and potentially. A good example of this possible source of fallacy may be found, I think, among the so-called puerperal septicæmiæ.

Hitherto our treatment, as far as it goes, has been appropriate whether the condition has been due to a single or to a multiplicity of cause. When, however, we come to serum-therapy, as a curative measure it has a much more limited or specific action. In the face of the facts of many reported cases it is necessary to insist that the indications for the use of anti-streptococcic serum should be determined on more exact grounds. Something more is wanted than mere biological relation between the disease and the antidote.

Under no circumstances should we trust to the serum alone, should there be any suspicion that the condition is due to a mixed infection.

There are no more likely conditions than the puerperal infections to become mixed in character. One would not object seriously to the use of anti-streptococcic serum in such cases, but the results should not be taken for or against the efficacy of the particular serum employed. Such results are not edifying, nor is the principle of the treatment to be recommended.

Though missing the mark in this way is almost worse than useless, yet even as serious may be the effect of failing to recognise that certain important diseases of a specific nature may be dangerously complicated by a secondary infection, and that the treatment should be modified or extended accordingly.

The first and most important example to engage our attention is

PHTHISIS.

The evidence that pulmonary tuberculosis is generally associated with mixed infections depends upon clinical observations, biological examinations, and results of treatment. *Post-mortem* results go to confirm this.

Clinically there is almost everything in common between phthisis and other chronic suppurating or septic diseases. In fact, it required the genius of a Koch to give us the means of making so important a distinction. This difficulty was not merely confined to the lungs, hence the widespread utility of Koch's discovery. Why, without bacteriological evidence, is there a difficulty? Because the septic element is common to them all; the purely tubercular features are obscured by the effects of the mixed infection. Further comparison shows that the clinical charts are identical, the loss of weight and disturbances of assimilation proceed in the different diseases *pari passu.* In the case of phthisis, what was at first a purely tubercular process becomes obscured by the secondary, yet immeasurably harmful, septic and saprophytic changes in the broken-down lung tissue. Bacteriological examination of the sputum shows the tremendous predominance of ordinary bacilli and cocci—especially important are the strepto—forms. The tubercular organisms vary in number, but they are most numerous in the cheesy points and in the small masses, not altogether structureless, which have been ejected before undergoing more complete disintegration by saprophytes.

Surely that is evidence that the tubercular process is not hindered, but rather hastened, in its destructive effect.

In fact, the tubercular process may be likened to the slow action of the elements in crumbling to dust some old ruin belonging to the age of the feudal barons, let us say. But one day an army of workmen—representing an active secondary infection—proceed to rapidly demolish the structure, not in molecular proportions, but in a more massive and business-like manner.

Thus the peculiarity of the distribution of the tubercular organisms in the sputum evidently depends on the changes which occur in the cheesy pabulum as a result of the secondary infection. These changes necessarily interfere seriously with the chances of cicatrisation or fibrous encapsulation of the tubercular foci. Moreover, by rendering the sputum more mobile and voluminous, distribution of the disease through the bronchi is greatly facilitated. It is not unusual also to find broncho-pneumonic patches of septic origin surrounding the tubercular lesions in advanced phthisis.

Tubercular bacilli, the prime agencies in destroying pulmonary tissue by inducing caseo-necrotic changes therein, do, no doubt, the lion's share of this destructive work, in the first instance at least.

It is nevertheless the destiny of air-borne organisms to invade this cheesy pabulum as soon as ever the lesion communicates with the general air spaces through a bronchus.

The slow, imperfect oxidation characteristic of the tubercular process is now seconded by a more complete disintegration by saprophytes. The chemico-physical effect of their activity might be responsible for extensive disruption of the pulmonary tissue. In other words, caseo-necrosis is taken many steps further, and by septic and putrefactive organisms. Hence the copious expectoration of phthisis, its purulent nature, and its varying hues.

With regard to *post-mortem* evidence, anyone who has experience thereof must be inclined to form one opinion.

This complicating infection profoundly affects the prognosis. To recognise the twofold and complicated nature of the invasion is of the utmost importance to the patient. In the incipient stage, before there is much fever or other manifestation of septic complication, we usually offer a more or less hopeful prognosis. But let the disease acquire the septic element, then mark the changed aspect of our case. It has taken on a distinctly septic type—one not peculiar to tuberculosis at all. The tubercular element is only recognisable now by bacteriological methods.

At this point I would enter a protest against the practice of branding patients as tubercular without reliable bacteriological evidence. It is unfair, unscrupulous, and unwarrantable to do so, even though the case "looks it all over." Surely we can believe that without this test mistakes will be made. Thus we can account in a measure for some of the rapid and permanent cures reported from time to time of cases in which there has been extensive pulmonary lesions not proved to have been tuberculous.

TREATMENT.

There is a really hopeful outlook in respect to prevention. There is a much less sanguine prospect with regard to cure. Therefore, for the present, prevention should be our watchword. Further, I consider the risk from tubercular meat is quite unimportant in comparison with the danger, ever present, from those cases of phthisis aptly described as ambulatory volcanoes. Tubercular milk is a great source of danger to children, but, above all, legislate and educate with the object of restraining the perpetuation of the disease as a family or house disease.

I feel perfectly convinced that the sanatorial treatment so much in vogue now is chiefly beneficial by its influence on the septic element. Such open-air methods, with forced feeding, are highly beneficial in all forms of septic diseases, whether complicating phthisis or not. Therefore the success which follows its application in phthisis is additional testimony of the promise a more general application of the system of treatment would give.

The favourable effect on phthisis in particular is accounted for in general terms by many writers. Let me more particularly indicate two potent factors. Firstly, the septic element is improved by the conditions of the treatment. Secondly, the risk of septic re-infection or re-inforcement is reduced to a minimum by the isolation of cases and the sojourn in the relatively pure atmospheres recommended to our patients. The general consensus of opinion favours cool climates removed from settlement. In such situations micro-organic activity is poorly represented, and relatively of a low order. The more thoroughly we eliminate the septic element the more perfectly do we reduce the struggle to a single combat between man and his tubercular foe. Experience teaches us that under these circumstances the disease is often successfully thrown off, though it is rare that the fight is reduced to such equal terms. Where the struggle is hard-fought and even, the presence of putrifying material and the absorption of its derivatives is often the factor which turns the scale in favour of the disease.

Therefore, to treat the disease as a pure condition, is to miss the real extent of the situation.

Professor Pfeiffer, in a paper read before the International Congress of Tuberculosis at Berlin lately, makes the practical and important suggestion that septic cases should be treated apart from those without fever in sanatoria or elsewhere. Evidently the Professor would not have us undervalue the septic element. To those of us who hope for the day when we will be given a distinctly curative agency in regard to phthisis, I would say it behoves us to remember that we will nevertheless learn its inefficacy in the presence of established septic complications. In every disease these complicating infections will be a serious bar to convalescence in the future therapy of specific microbic diseases, and will require then, as now, our most earnest attention. Personally, with regard to phthisis, I favour the employment of antiseptic inhalations in cases with marked febrile disturbances, in conjunction with the general treatment. I exhibit a chart of a case of a bad type to show the effect of inhalation of guaiacol, menthol, and terebene. Should the line of treatment be adopted, I would recommend that the antiseptic should be frequently changed to prevent the organisms establishing a tolerance, which they readily do.

ACUTE PNEUMONIA.

Sometimes lobar pneumonia fails to resolve, or at least re-solution is delayed. The entrance of septic germs at this stage would be most disastrous. A case of phthisis in close proximity to such a case would be a double source of danger. The case runs an imminent risk of becoming septic or tuberculous, or both—a sufficient indication for the treatment of such cases apart.

Under no circumstances, therefore, should such cases be crowded together in hospitals. It is just as essential in pneumonia as in phthisis to select unvitiated surroundings. In this climate every advantage should be taken of verandas or tents. The only precaution necessary is that care should be taken to maintain the bodily temperature by good clothing.

DIPHTHERIA.

Diphtheria is a disease in which, though we have attained a marvellous degree of exactness in its treatment, we yet meet with a variety of cases having a relatively high mortality, characterised by mixed infection. This may be so marked as to interfere seriously with the diagnosis both by clinical and biological methods. In such cases, if in doubt, we often for safety's sake use serum. This will no doubt deal with the diphtheritic element if present; but what about the streptococcal infection?

It is a haphazard way to treat the case in the first instance by anti-diphtheritic serum. Would it be much more irrational to second its use by injecting anti-streptococcic serum? At any rate it is very necessary to apply local measures vigorously.

While I am on the subject of antitoxins, especially that of diphtheria, I will take the opportunity to mention that in young infants the amount of toxin produced in any microbic disease is approximately the same as would be produced in older children. But relatively there is more, as there is decidedly less of body weight. Therefore the relative potency of the serum is greatly enhanced. The minimum fatal dose is, of course, proportionately less in infants than in older children. The indications for serum are more urgent, and, if anything, the dose should be larger. This is a point which, as far as I have been able to observe, has not received the attention it deserves.

Time does not allow me to give further illustrations of the importance of this subject.

Nevertheless, I will refer to the fact that as many chronic and debilitating diseases are abruptly terminated by a complicating infection, it is wise to protect such cases as far as possible from the risk they cannot afford to run.

In medicine, as well as in surgery, "the previous case" is the source of much evil. Where no previous cases exist, or where the chance of harmful contamination from an infected focus is eliminated, there is little danger to our patient. We have, then, to contend with sporadic cases, which are not only few and far between, but are more successfully treated.

Therefore it is essential to treat patients threatened with this abrupt termination in places far removed from infected environment.

Such conditions other than those I have mentioned include—

Chronic alcoholism and chronic renal disease; chronic valvular disease; various forms of insanity, such as general paralysis; diabetes, leprosy, cancer,. debility from old age, and, perhaps, a great many others.

Dr. Turner said that Dr. Halford had dealt with a subject not only of great theoretical interest, but of practical importance in some respects at the present time, and destined to be of much greater practical importance in the future. One practical point was that in hospitals cases of early a-pyrexial phthisis should not be placed in the same ward as cases of advanced disease with septic complications. In the future they would have to diagnose not only the occurrence of a pneumonia, for instance, but whether this was caused by the pneumococcus, streptococcus, staphylococcus, bacillus of Friedlander, or several of these microbes in combination. If specific curative serums were introduced for any of these microbes, such discrimination would become imperative. The common occurrence of what are termed by Osler "terminal infections," should remind us that the pathogenic cocci are always present, always on the watch for a weak point through which to invade our organism. As an instance of secondary infections, he referred to a case of pneumonia in which fatal meningitis occurred during convalescence. Pneumococci were found by him in the lung, but the meningeal exudate gave a pure culture of the meningococcus intracellularis meningitidis of Weichselbaum.

Dr. Gill was quite in sympathy with Dr. Halford in his plea for a more exact diagnosis in cases of general septic infection. While it was no doubt true that the great majority of such cases were due to the "streptococcus pyogenes," yet some were probably due to staphylococcal infection, and others, again, to other organisms; but, unfortunately, the ordinary means of examination of the blood for organisms gave entirely negative and valueless results. But we would be fairly safe in treating the majority as cases of streptococcal infection. Some very striking cases of cure of "Ulcerative Endocarditis" by the use of anti-streptococcus serum had been recorded, and although such cases had been disappointingly few, yet they must be regarded as due to the use of the serum; and he thought, therefore, that the serum should be used in every case. From the fact that the serum cures a few, and fails in the majority, one must assume that there are probably many different races of streptococci, which we are as yet unable to differentiate. With regard to mixed infections in diphtheria, he thought that a good deal too much had been made of it. Streptococci and staphylococci were commonly found in diphtheritic throats, yet they appeared to be quite local, and not to be doing much harm. The great and rapid improvement that followed the injection of antitoxin in the foulest cases, showed that the diphtheria bacillus was really the only cause of the trouble.

Dr. Isbister: That disease predisposes to disease is a general statement which is more or less true. Infection by one organism commonly lowers the resisting powers of the subject to infection by other organisms. In a few instances the infection may be mixed from the outset, but in most instances the invasion of the one organism is antecedent to that of the other. The etiology and bacteriology of dual or mixed infection is worthy of especial study, as it may cause abnormality and confusion of symptoms, and so lead to difficulty in diagnosis, and at the same time materially influence prognosis. The question of mixed infection is of practical importance in serumtherapy. Before using a curative serum it is necessary to know what is the organism producing the toxin, and if two organisms be present theoretically two curative serums might be necessary. It is frequently impossible to determine the presence of a mixed infection; the site of the pathological lesion may be unapproachable. In the future it may be possible by the serum reaction to determine the nature of the infecting organisms. Lorrain Smith and Tennant have pointed out that the *Bacillus Coli communis* and the typhoid bacillus each produces its own specific agglutinative substance in the patient's blood, and that the one clumping reaction is independent of the presence or absence of the other.

Dr. Scot Skirving thanked Dr. Halford for his clear and instructive paper. He was glad to find there a reference to the incidence of phthisis after pneumonia. He understood Dr. Hirchfeld to say that he would not send the early phthisis case to an open-air sanatorium. For his part, he was strongly in favour of that course. He was also glad to hear the view that in such sanatoria—or in consumption hospitals proper—a judicious separation of patients should be carried out. To put a case of early uncomplicated phthisis alongside a patient riddled with a mixed infection seemed a most improper proceeding.

ŒDEMA IN INFANTS—NOT OF RENAL ORIGIN.

By W. Atkinson Wood, M.D., Ch.B., D.P.H. (Lond.),
Honorary Medical Officer to Outpatients at Children's Hospital, Melbourne.

L.E.; female; aged four months.—This child was brought to me with the history of being quite well till two months old; she then began to vomit her mother's milk, which was the only food she had had up to that time. A medical man then advised the mother to discontinue the breast milk, and give Horlick's milk instead. This was the only food she was getting for two months before I first saw her. Four weeks after taking Horlick's milk the child began to swell, first in the feet, then legs, face, arms, and hands, stomach, and, lastly, in the back.

On examination the child was intensely anæmic, and greatly swollen all over, the skin pitting deeply, and being almost translucent (see photo). The child was in no pain; it was not "a cross child." Temperature normal; no sweating nor beading of ribs, nor enlargement of epiphyses.

Family History.—The mother has been married for three years, and has had twins before. One was stillborn, and the other died at ten months' old of "vomiting and marasmus"; no miscarriages.

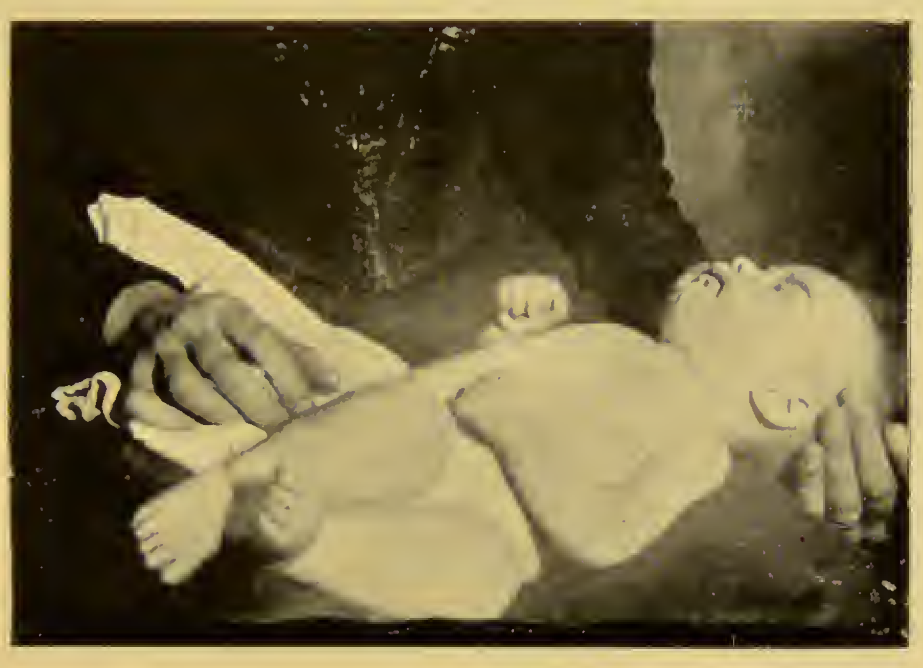
Urine clear, watery-looking, slightly turbid on standing, acid, traces of phosphates and carbonates, no albumen; not enough collected to estimate specific gravity. Microscopically some triple and amorphous phosphates.

Blood.—Red corpuscles, 2,732,000 per Cmm.; white corpuscles, 66,000 per Cmm.

Treatment.—The child was put on cow's milk and cream, with fresh meat-juice and small doses of an emulsion of cod liver oil; and, later on, on the advice of Dr. Snowball, some mercury ointment was placed on the binder. The food was taken well, and the child seemed to improve under treatment. The swelling decreased, but later on she refused food, emaciated rapidly, and died three weeks after I first saw her.

Post mortem.—All the organs macroscopically appeared normal, but the appended report of Dr. Mollison, who kindly examined them microscopically for me, showed them to present signs of congenital syphilis. Dr. Mollison made sections of the lung, liver, and kidney. The lung showed small areas of broncho-pneumonic consolidation, and the small arteries were thickened; in the liver the arterial change was very marked, the swelling of the inner coat being very prominent. There was also a uniform infiltration of the liver substance, with a delicate network of newly-formed connective tissue. In the kidney there was an increase in the interstitial fibrous tissue, with patches here and there of fibroid development; the arteries were also thickened. Dr. Mollison regarded the condition as being due to a congenital syphilitic taint.

The second case of general œdema was in a female child, aged fourteen months, first seen by me at the Children's Hospital on 28th May, 1899. For five months previously it had been vomiting, and had had diarrhœa off and on, with progressive loss of flesh. Three weeks before I saw her she began to swell in the feet, then in the legs, arms, face, and all over, the eyelids being so much swollen that she could not see out of them. This condition lasted for two weeks, when she was treated with cod liver oil, and the swelling at once began to disappear, and when I saw her first only the feet and hands showed signs of œdema. The child has been fed almost exclusively on the breast up till the present time. At eleven months she could walk, but at fourteen months she could not even sit up. She has lately been very cross, and has sweated a good deal, especially about the head. This sweating, however, disappeared when the swelling came on.



L. E., AGED FOUR MONTHS.

On examination she was typically rickety; prominent forehead and belly, beaded ribs, enlarged epiphyses. There was also some sponginess of gums and pain on moving limbs, indicating scurvy as well. There was a tender rash, resembling erythema nodosum, on the legs, arms, and buttocks.

Urine.—Clear, acid, no albumen.

Treatment.—The child was weaned and put on cow's milk, meat-juice, vegetable soup, fruit, and cod liver oil. Within a few days she improved markedly, and within a month she was, as the mother expressed it, "quite well." There was no congenital syphilitic history. The rickety condition was here due to breast-feeding beyond nine months, and it is interesting to note the disappearance of the sweating directly the general œdema appeared, as if the two were causally connected.

My excuse for reporting these two cases is that in medical literature so little reference is made to general œdema without albumen in the urine, although the cases are not uncommon amongst children.

Holt, who gives the best description, says:—"Nearly all the cases were in infants under six months, and the majority under three months; it is invariably associated with extreme malnutrition and anæmia coming on in three to four days, and affecting the feet, ankles, thighs, face, hands, and sometimes the abdominal walls and back. This may be quite marked, so that it may be impossible to open the eyes, and the extremities may be double their ordinary size. No explanation is found in the urine; it is not albuminous. Opportunities for examining the kidneys have been afforded in several instances, and these organs have been in all cases normal, even on microscopic examination."

Tarnier ascribed the condition to giving too much fluid food, and in my first case this explanation might hold, as the mother said that, although the child was not satisfied after a meal of Horlick, she vomited if more was given. The food was probably too fluid and bulky.

Local œdema has often been described with anæmia, and these cases may merely be an extension of the same process. If the œdema is a symptom of anæmia, what causes the anæmia?

In these two cases congenital syphilis and rickets seem to have been the exciting cause. I have found that mercury will often improve anæmia in infants, which would lead one to believe that anæmia without other symptoms is a common manifestation of slight congenital syphilis.

NOTES ON ACUTE YELLOW ATROPHY OF THE LIVER.

By A. Jarvie Hood, M.B. (Glasgow),

Honorary Physician, Sydney Hospital.

The comparative rarity of cases of acute yellow atrophy of the liver, at any rate of reported cases, is my reason for recording three cases which have come under my personal notice while in practice in New South Wales. As far as I can find out there have only been two recorded cases in this colony—namely, cases recorded by Drs. Creed and Scot-Skirving; but although their two cases presented all the classical symptoms of acute yellow atrophy, still they ended in recovery. I shall briefly record these three cases clinically:—

Case No. 1.—The first case which came under my notice occurred on the Clarence River. On the 18th December, 1887, I got an urgent message to go to see a patient, Mrs. J. C., who had suddenly become jaundiced. At that time I was attending quite a series of cases of jaundice, all of the catarrhal type, all of whom had recovered, or were recovering. I consequently thought that it was a case of a type similar to what I had been attending, but on my arrival I found matters very different to what I expected. She was a woman, aged thirty-two years, with two children, the youngest aged two and a-half years. She was then three months' pregnant. The history I got was as follows:—She had been quite well till five or six days before my visit, when she had begun to feel out of sorts. She had lost her appetite, had been suffering from frontal headache, constipation, and some slight nausea and discomfort which ultimately culminated in a certain amount of epigastric pain. The day previous to my visit jaundice had set in. On examination the following signs and symptoms were present: She complained of severe frontal headache, nausea, and vomiting, the latter occurring without any reference to the taking of food; her temperature was normal, tongue coated, and the bowels were constipated; her pulse was 70 to the minute. On examination her liver dulness was normal, the skin and conjunctivæ were deeply jaundiced, and the urine contained a large quantity of bile. I prescribed for her, but felt that the case was not exactly like the ones I had been attending, and was somewhat guarded in my diagnosis and prognosis to the husband. I saw her two days afterwards (as she lived at some distance from my residence), and found matters much more grave; the vomiting had become incessant, and consisted not only of the food she had taken, but was of a black or coffee-ground appearance; the temperature was then 102°, the pulse varied from 120 to 140, and diarrhœa had set in, the stools being black and offensive, and very frequent. The jaundice had become very intense, and she was semi-delirious, and very restless, the delirium alternating with drowsiness; that same evening she had a miscarriage. The liver dulness was much diminished, and there was slight tympanites. From that onwards she very rapidly got worse; her temperature rose to 103°, then twenty-four hours after my visit she became comatose, and continued so till her death on the 25th December. The jaundice had become very marked; the diarrhœa continued till the end; and before death the liver dulness was diminished to 1 inch. The urine contained a quantity of bile all through, but on the second occasion on which I saw her the specimen I then obtained contained leucin and tyrosin, but no trace of albumen was present. I was only allowed to make a very limited *post-mortem*, which was made six hours after death. The body was spare, and not very well nourished; due, no doubt, to the vomiting of pregnancy, which had been very troublesome for some weeks previous to my seeing her. The liver weighed only 12 oz., and was shrunken, soft and pulpy, and of a deep yellow colour. The intestines were stained yellow, but in several places submucous hæmorrhages had occurred. The lungs were congested and œdematous, and the other organs were stained yellow. Her family history was a good one, as was also her previous personal history, and she was apparently in perfect health, with the exception of morning sickness, till a few days before I saw her.

CASE NO. 2.—On the 21st July, 1892, I saw in consultation Mrs. W. M., aged twenty-four years, at Redfern, Sydney. She had been ill for about ten days. When I saw her she was in a state bordering on coma, with occasional restlessness and low delirium. She was deeply jaundiced, her tongue was dry, and almost black, her temperature was 101°, her pulse 120, and soft; there were also several petechial spots distributed over the lower limbs and abdomen. She was passing her evacuations in bed, the stools were black and fœtid, and very frequent; the urine was almost black, and contained bile. On physical examination the heart and lungs were apparently normal, the liver dulness was reduced to 1 inch in the nipple-line, and the abdomen was slightly tympanitic. The conjunctivæ were deeply tinged with bile, and the pupils contracted almost to a pin point. It was impossible to rouse her, and she gradually sank, and died the following morning in a state of profound coma. No *post-mortem* examination was allowed, but the sample of urine drawn off by the catheter contained crystals of leucin and tyrosin. The history I got was that a fortnight previous to my seeing her she had, whilst apparently in good health, gone to a dance, and danced pretty well all night; she was then three months pregnant. A couple of days after that she began to feel ill, feeling cold and generally out of sorts. Soon after, vomiting accompanied with jaundice set in, and on the sixth day she had a miscarriage. The symptoms gradually became very severe; and, notwithstanding treatment, she got into the condition in which I saw her. Previous to this she had been a strong, healthy woman, with one child aged two years.

Case No. 3.—Mrs. A. C., aged twenty-one years, was admitted to the Sydney Hospital under my care on the 6th November, 1897, suffering from jaundice, pain in the right hypochondriac region, and vomiting. Ten weeks previous to her admission she had had a bad confinement, with complete rupture of the perinæum, followed by puerperal fever. A month previously she was curetted, and made a complete recovery. About a fortnight ago, when apparently recovered from the curetting, headache and vomiting began, followed in three or four days by jaundice, and later by pain in the right hypochondrium. A day or two afterwards severe epistaxis occurred, and from that onwards she rapidly became worse, till 5th November, when the vomiting became almost incessant, and the jaundice suddenly became much deeper. Her relatives also noticed that she became very stupid, and on the following day—namely, the 6th, she was brought to the Sydney Hospital. For some time prior to her admission she had also complained of pain in her head, and the stools had become clay-coloured. On admission her condition was as follows:—She was well nourished, but deeply jaundiced, and was in a condition of stupor, alternating with extreme restlessness. To some extent she could be roused, and could answer some questions, but very slowly. The tongue was slightly furred, temperature 102°, pulse 100, and very soft, the pupils somewhat dilated and reacting slowly to light. The abdomen was neither distended nor retracted, but there was distinct pain on pressure in the right hypochondrium. The liver dulness measured 2½ inches in the nipple-line, but extended 1 inch beyond the left edge of the sternum. Before I saw her a dose of olive oil had been administered, but was immediately vomited; this was followed soon after by clay-coloured stools. The lungs and heart were normal, and there was no enlargement of the spleen. The urine contained a large quantity of bile, but no leucin or tyrosin. There was a peculiar heavy odour from the breath. On the 7th November she was distinctly worse. The jaundice was deeper, the pupils were still dilated, pulse 120 and soft, and her speech was rambling and incoherent. The vomiting was still frequent, and of a dark colour, and unpleasant in odour. She was very restless, and distinct twitchings of her lower extremities were noticed; the stools were dark and unformed. Temperature 101°. On the 8th November she was apparently sinking, the stupor was profound, and extreme restlessness obtained; pulse 130, pupils contracted to a pin point, and not sensitive to light. There was a distinct heavy odour about her, and towards the last, rather active delirium set in, accompanied by screaming and bellowing; she died the same night. The specimen of the urine drawn off shortly before death contained crystals of leucin, but no tyrosin. A *post-mortem* examination was not allowed, but six hours after death the liver was removed through an incision in the right loin, and weighed only 6 oz. It was shrunken and pale on the surface, and on section presented a bright yellow colour, with some dark patches; its consistence was soft and pulpy. I should have stated that previous to her pregnancy she had been a healthy woman, but in the early days of pregnancy she suffered much from retching, and during all her early life she had been inclined to vomit easily.

Remarks.—As regards these three cases it seems to me that the diagnosis in each was undoubted. Each patient had been apparently well till the actual illness set in; all within a fortnight of their deaths. Two of them were pregnant at the time, and both miscarried; the other one had been confined ten weeks before, and had had a severe labour, followed by puerperal fever. In two of them a *post-mortem* examination made the diagnosis absolute, but as regards the third, the symptoms, in my opinion, were characteristic.

Dr. Isbister: In Australia, acute yellow atrophy, although exceedingly rare, appears to be less uncommon than in England and most other countries. The liver of Dr. Hood's Sydney Hospital patient presented a most characteristic appearance. The various shades of orange and yellow were most brilliant, and not unlike that found after an excessive dose of phosphorus. There was strong evidence in some of the cases mentioned that acute yellow atrophy was a definite specific infection by some organism. It was in several instances associated with cases of milder epidemic jaundice. Its course and many of its symptoms were comparable with those of recognised specific infections. Drs. McPhedran and Macallum, of Toronto, thought that the disease was due to some poisonous albumen coming from some putrefactive or similar process in the intestine. Some observers report finding a micro-organism. Surely at first sight it would appear easier to prove that acute yellow atrophy was due to an organism than to show that the same is true of cirrhosis of the liver. Some varieties of cirrhosis have been shown to be of a specific organismal origin.

AMŒBIC DYSENTERY AND AMŒBIC ABSCESS IN AUSTRALIA.

By J. L. T. Isbister, B.Sc., M.B., Ch.B. (Adelaide),
Late Resident Pathologist to the Sydney Hospital.

In these days of increasing accuracy in pathology and etiology the range of disease included under the term "idiopathic" is gradually and surely becoming more limited. Those diseases which have for long been considered due to some unknown germ are gradually being brought into line with kindred diseases by the actual discovery of that long-suspected specific organism.

No part of the intestinal tract, perhaps no one viscus of the body, presents a more varied and wider field of discovery than the large intestine whose various so-called dysenteric lesions are found to be due to various organisms, and very much interesting work still remains to be done on the many varieties of acute and chronic lesions of this part of the alimentary canal. The large intestine appears to suffer from a greater variety of acute and chronic lesions than the small, and the pathology and bacteriology of these same is more uncertain and more speculative than in the case of the latter. The pathology, and especially the bacteriology, of these lesions is far from being definitely settled, consequently the terminology is somewhat confusing, for the clinical term "dysentery" is made to cover such a broad field both from a pathological and bacteriological point of view. In a recent paper Davidson[1] points out the difficulty of attempting to accurately define dysentery. He says: "Our clinical conception of acute dysentery is that of an inflammatory disease of the large intestine, with or without fever, characterised by mucous, serous, or bloody stools, containing epithelium, débris, or sloughs of the bowel, accompanied by tormina or tenesmus." Yet would not some of the forms of clinically so-called colitis agree most exactly with this definition, and agree not only in its clinical aspect but also pathologically, for it has been admitted by a very high authority that it would be almost impossible to distinguish at a *post-mortem* examination between some cases of dysentery and colitis.

The text-books describe the various forms of colitis—viz., catarrhal, ulcerative, and membranous. They mention the various forms of dysentery—viz., the epidemic dysentery of the temperate zones, the endemic dysentery of the tropics, and the dysentery of war and famine. Clinically we speak of catarrhal and diphtheritic and gangrenous dysentery. Surely the clinical term "dysentery" must embrace many diseases all of totally different primary origins.

What greater contrast could there be than that between two patients who happened to be in the Sydney Hospital at the same time. The one was sallow, emaciated, and worn by a long-standing, intermittent dysentery, which every now and again became semi-acute, but in the intervals allowed the patient a general respite from the most pronounced of the symptoms. The other was a very well-nourished woman in the prime of life stricken down with an acute epidemic dysentery, the whole attack lasting but a few days, and the attacks proving fatal not only to herself but to her eldest boy, fifteen years of age, and all but killing two others in the family. Both of these patients were clinically suffering from dysentery, and still other cases could be easily mentioned which would be called cases of dysentery, and yet would resemble neither the one nor the other of those just mentioned.

Manson[2] puts the whole matter very tersely when he says, "Dysentery simply means inflammation of the colon, and there are many forms of inflammation of the colon."

Quite a number of organisms have been found in various dysenteries, but the commonest are the *bacillus pyocyaneus*, an organism resembling the pseudo-diphtheria bacillus, the *bacillus coli communis*, *streptococci* and *staphylococci* The three last-mentioned may be but secondary to some primary organism which paves the way for these pus-forming ones.

In addition to the primary affections of this part of the alimentary tract, there are the various secondary catarrhal and ulcerative lesions found at times associated with well-recognised pathological conditions in other parts of the body, such, for example, as are sometimes found in cases of chronic nephritis, in some pyæmic conditions, and even in cases of pernicious anæmia. A discussion on the relations of these to the primary disease opens up a wide field indeed, but still wider is that covered by diseases in which the bowel is admittedly the primary seat of pathological change.

Of all these varieties of inflammation and ulceration of the large intestine there is one of especial interest, and it is an ulceration caused by the ravages of the amœba coli, and hence it is called "amœbic dysentery." This small motile jelly-like organism is closely allied, if not identical, with the amœba so commonly found in ponds and pools, and it has recently sprung into first rate eminence because of the extremely serious and commonly fatal results attending its presence.

The amœba of dysentery is, as a rule, larger than its fellow from the stagnant water-pool. It, however, varies in size in different cases of amœbic dysentery, and sometimes in the same case, as was noted in one of the patients who came under observation in the Sydney Hospital.

The diameter of an amœba is usually four to five times that of a red blood corpuscle. It is of a faint bluish-green colour, with a well-defined outline and a refractile body substance, the latter showing vacuoles and granular débris, and red blood corpuscles in process of absorption.

The outline of the amœba is spherical when at rest, rarely irregular, and then only when densely packed in masses. Like ordinary amœbæ, they are capable of movement by throwing out pseudopodial processes either one or more at a time, and the body of the amœba flowing, as it were, around the process. All these characteristics were well seen on many occasions in the various cases in the hospital.

In the human body the amœba is usually found in the cæcum and ascending colon, or any part of the large bowel, where it invades the intestinal coats, but especially the submucosa, causing a marked and characteristic thickening.

This thickening is at first localised, and if an incision be made into it a yellow thick material escapes.

The fluid contains amœbæ. At this spot the mucous coat ruptures, and so there is formed an ulcer whose edges are undermined, and whose boundaries are extended by the gradual excavations of the amœbæ.

The ulceration, as a rule, does not extend deeper than the muscular coat, but the ulcerated area may be very extensive, and large sloughs of mucous membrane be separated by the undermining of the amœbæ in the submucous tissues. The whole of the large bowel may be affected as in the case described by my honoured teacher, Dr. Verco, of South Australia.

The ulceration is not primarily a process of pus formation, but rather a liquefaction and passive necrosis of the tissues. In this respect the amœbic changes are totally different to those of pus-forming organisms, and this is a strong argument in favour of the amœbæ acting quite independently of any other organism.

With this ulceration of the intestine the patient may have no sign of dysentery whatever, although he usually suffers from a looseness of the bowels. There is with the dysentery a varying amount of constitutional disturbance which becomes well marked when the disease is once fairly established. The patient begins to lose weight, and has a gaunt appearance. He is of a distinctly sallow or dusky countenance, which Lafleur aptly compares to the colour of dirty chamois leather, and this certainly describes the three cases which were treated in the wards of the Sydney Hospital. The colour is no doubt partly due to some anæmia consequent on the destruction of the blood corpuscles by the amœbæ, and partly to the mal-assimilative powers of the bowel. There may be nausea and vomiting and hiccough, and Lafleur says anorexia. One of the cases to be mentioned hereafter had a most voracious appetite, except during an acute exacerbation. He said he would eat anything given to him except fish, which he stated always increased his dysentery very much indeed. Tenesmus is not common. Fever is not a prominent symptom, and it is irregular.

The course of the disease is a most variable one, and cannot be forecast with any degree of certainty. The case may run a rapid and unfavourable course, and death follow in a few weeks, or it may be chronic with repeated subacute attacks. The tendency to chronicity with repeated intermittent exacerbations is a most marked characteristic of amœbic dysentery. Not only is the disease itself a most serious one, but its complications are most dangerous. In acute gangrenous forms perforation of the gut may occur, and it is always fatal. The commonest and chief complication is abscess of the liver. The amœbæ are carried into the hepatic tissue either by the portal circulation, or else they escape through the base of the dysenteric ulcer into the general peritoneal cavity, and are carried to the upper or lower surface of the liver. There is much to support this latter theory, for the abscess is so frequently quite superficial in its origin. There may be one or more abscesses.

It is strange that hydatids and amœbæ should both find their way into this viscus, although starting from different points—the one not improbably from the stomach, and the other from the cæcum, or any part of the large bowel. Can it be that the liver offers a more suitable culture medium for their growth than any other abdominal viscus? It may be asked why do the amœbæ, when free in the peritoneal cavity, not attack the omentum, or even the spleen, whose upper surface is a particularly sheltered and undisturbed spot, where they could readily come to rest, and set about excavating an abscess cavity? The upper surface of the right lobe of the liver is the favourite site for an abscess, and of the four cases to be mentioned three of them developed abscesses here. When formed, the abscess usually perforates the diaphragm, and the amœbæ start operations on the base of the lung. This is what actually happened in three out of the four cases. The abscess frequently bursts into a bronchus, and so is discharged through the mouth. This complication of hepatic amœbic abscess is unfortunately only too common, and almost certainly fatal. Kartulis, working in Egypt, does not give the percentage of cases of hepatic abscess following amœbic dysentery, but out of 500 liver abscesses which he saw 55 per cent. to 60 per cent. contained amœbæ.

Councilman and Lafleur recorded fifteen cases of amœbic dysentery, 40 per cent. of which developed abscess. If the amœba once enters the liver, the patient may well all hope abandon, for the chances of recovery from so dread a complication are but slender.

It is forty years since amœbæ were first noticed in the stools of a certain patient, but no special importance was attached to this till Lösch, in 1871-73, fully investigated the subject, and it was he who conferred the name "amœba coli." The case was one of dysentery alone, without any accompanying abscess of liver or lung, and ended fatally after a couple of years. It was this observer who found that the disease could be produced in the bowel of a healthy dog by injecting into the rectum portions of stools containing amœbæ coli.

In 1881 Cunningham[3] reported that he had found the same, or, at least, a similar organism, in the stools of healthy people. The identity of these with those found in typical cases of amœbic dysentery is still unsettled, and the question in doubt.

From 1885 to 1890 Kartulis[4] investigated the etiology of Egyptian dysentery and of its relationship to liver abscess. He also performed inoculation experiments, and produced some erosions in the bowel of the cat, but he failed to produce any pathogenic lesion when he fed the cat on living amœbæ.

In 1890 Osler and Lafleur found the amœbæ in two cases of dysentery associated with liver abscess. Both cases ended fatally.

In 1891 Councilman[5] and Lafleur published a most exhaustive and complete account of fourteen cases of this dread disease, and I here acknowledge my indebtedness for much that is here written to the work of these observers.

The disease has now been recognised in Egypt, North America, Japan, and Brazil. India, the great home of dysentery and hepatic abscess, occupies a somewhat anomalous position, for recorded cases of amœbic dysentery from that country appear to be exceedingly rare. Apparently the origin of much of India's dysentery has yet to be discovered.

Davidson[6] says that tropical dysentery comprises several forms, but it is not yet fully known how much is amœbic in origin.

It is now certain that this most unwelcome and fatal guest is to be found in Australia, for it has been met with in both South Australia and New South Wales. It is also to be found in the adjacent New Hebrides Islands. On the 27th of May, 1898, a patient was admitted under Dr. Fiaschi to the Sydney Hospital suffering from what was subsequently diagnosed to be an amœbic abscess of the liver, perforating the diaphragm, and involving the lower part of the right lung. This abscess burst into a bronchus, and was subsequently drained by operation through the lower and posterior thoracic region. Although drainage was most complete, the disease still made headway, and the patient died some four months later. Various antiseptic sprays, such as quinine, sanitas, and potassium permanganate were used to stay the progress of the abscess, but all without success. At the *post-mortem* examination, the abscess cavity was found to extend from the second intercostal space above to the apex of the right kidney. There was but a small amount of amœbic ulceration of the bowel in the region of the cæcum and sigmoid flexure. The case was abnormal in that although there was amœbic ulceration yet the looseness of the bowels was but slight and only present a few days before death. Observers have mentioned this possibility of the occurrence of amœbic abscess without any previous history of dysentery; and at the *post-mortem* examination a certain small amount of dysenteric ulceration or dysenteric scarring has been found to be present.

This case was the subject of a paper read by Dr. Fiaschi and myself[7] before the New South Wales branch of the British Medical Association, and subsequently published in the *Australian Medical Gazette*, December, 1898.

It should be noted that the patient came to the hospital from the island of Aneiteum in the New Hebrides, and so the disease was not of Australian origin. It is curious that Dr. Gunn, of the Scotch Mission in those islands, records how a trading vessel in the year 1893 introduced a peculiarly severe and fatal form of *epidemic* dysentery to the New Hebrides Islands from Sydney itself, the indigenous endemic form not being especially severe at that time.

The first recorded case of amœbic dysentery and abscess arising in Australia is that mentioned by Dr. J. C. Verco,[8] of Adelaide. A most complete and exhaustive account of this case is to be found in the *Australian Medical Gazette* for February, 1899. The patient had lived in the southern parts of South Australia for the five years preceding his illness, and hence he in all probability contracted his disease there. Still he had lived previously to that in the tropics —in the Northern Territory—and it is just possible that he may have contracted it there. There was no previous history of dysentery prior to the formation of the abscess which subsequently developed. Manson has pointed out how that may be wanting, and actually was wanting, until just before death, in a patient who came from the New Hebrides, and yet who had had an abscess of the liver for at least seven months.

Perhaps the most remarkable point attaching to Dr. Verco's case was the fact that opening the abscess cavity but made matters worse. He himself says: "The manner in which the wound conducted itself was something quite new in my experience, and obliterated all hope of success to the operation. And this appears to me a most formidable obstacle to recovery. When an abscess cavity is opened and drained freely at its most dependent part, one expects it to grow smaller, and the tubes which are inserted easily enough at first to become more difficult of introduction from the growing in of the sides of the abscess and of the incision. But here the tubes became looser, and the incision through the pleura more capacious, and even the external wound larger. And this without any signs of inflammation." This was due to the amœbæ invading the freshly opened-up wound and destroying also the subcutaneous tissues. "Is it then," he says, "of any use to incise the chest wall in such a case?"

Two more well-defined cases of this interesting disease have since come under observation. Both of them were most certainly contracted in New South Wales, for the one man has not been out of the colony, and has spent almost all his life in Wollongong and Sydney. He was once—many years ago—as far west as Bathurst, in New South Wales. The other man—all the four cases have been in males—also lived in Wollongong, and never left the colony until after the onset of his fatal illness, when he was advised to take a trip for his health's sake and went to Bundaberg, in Queensland, but did not stay very long before returning home again, and then came to the Sydney Hospital.

Case 3, aged thirty-six years, was admitted to the hospital on 10th August, 1898. Fifteen months previously he had been well and strong, when his health began to fail gradually and unaccountably. Although at times his bowels were loose and he passed mucus and blood, still what concerned him more was his gradual yet steady loss of bodily strength and his increasing unfitness for work. He says he was feverish, and so took a six weeks' holiday; returned to his work again and continued at it for five months, but was still losing ground, and now began to complain of intense pain in the right shoulder. On one occasion he was stretching out of bed to reach some object when he felt something give way in his right side, and he immediately coughed up much yellow puriform material, and this he continued to spit up until several months later the pulmono-hepatic abscess was drained by operation.

His course in the hospital was an up-and-down one. For the first three months he coughed up anchovy-coloured sputum in very large quantities. This, at first sight, might have suggested a suppurating hydatid of the liver discharging upwards through the diaphragm and into a bronchus. The physical signs were those of consolidation at the base of the right lung, with no increase of liver dulness downwards. He was gradually losing ground, and so the abscess cavity was opened. Curiously enough, although the sputum showed amœbæ, yet the first gush of fluid discharged from the abscess cavity showed none at all, and was practically sterile. This sterility and absence of amœbæ from the first of the discharge are points upon which several writers have touched. Surely the frequent sterility of amœbic abscesses is strongly in favour of the amœbæ being the sole agents as Councilman and Lafleur suppose, and not amœbæ plus some cocci or bacteria contained in the amœbæ. The absence of amœbæ from the first discharge is mentioned by Manson, and he says that they frequently appear a few days later. This is what actually took place with the discharge from this abscess. They are in the very walls of the abscess cavity. Their antecedent presence in the sputum would be due to infection from the walls of the track leading through the lung to the bronchus.

After operation he improved and actually put on flesh. The discharge lessened, but still contained amœbæ. It was yellowish, red, and thick. The abscess cavity appeared to fill in, but the improvement was not lasting, for after the temperature had been normal for nearly a month the dysentery started again. The motions contained much mucus and many active amœbæ. The abscess cavity itself relit its original fires, and so things went from bad to worse despite all treatment with strong sprays of quinine. Quinine was given internally, and the bowel washed out with a solution of quinine (1 in 1,000 to 1 in 250). He died seven months after admission. A *post-mortem* could not be obtained.

Case 4, aged thirty-eight years, is the only one so far which has not died, and he is not yet cured, although much better. On admission, he had been ill for eight weeks with general weakness and loss of flesh together with dysentery; the motions contained blood and mucus, and the patient had much tenesmus. He complained bitterly of pain in the right hypochondrium, and also of pain starting from the right loin and shooting across the front of the abdomen. There were no rigors except at the onset of the illness, when the patient had three slight shivering attacks. He had fallen off in weight from 11 st. 3 lb. to 9 st. 2 lb. After admission the temperature was normal for a week, and then it suddenly rose to 102°, and patient was taken with an intense pain just beneath the right costal margin. It extended upwards to the right shoulder and down the right arm. This onset of pain was very sudden, and awoke the patient from his sleep. The bowels were loose, and the evacuations contained some bright-red blood after the attack—and amœbæ were always present. There was no increase of the liver dulness, although the patient was feverish. After a week the temperature had gradually fallen to normal, and acute symptoms had abated.

For the next seven weeks the temperature remained normal, and patient was then discharged to take up some light duties. He was improving, and had gained half-a-stone in weight. Two months later he was reported to be still improving, and there seems to be some hope of his ultimate recovery; but the prognosis must of necessity be very uncertain, for it is difficult to say when a patient is rid of all the amœbæ, and until free of them the result is uncertain.

Cases 3 and 4 very closely resemble one another, except that the latter, although very ill indeed, appears to have escaped with dysentery alone. However, it is possible that he has a latent hepatic abscess which may declare itself at a future date. He had symptoms strongly suggestive of abscess—intense pain in the shoulder and arm and along the right costal margin together with a variable temperature. But it is the surroundings of the two cases that are of especial interest, for the two men lived almost exactly under the same conditions. They were next-door neighbours, and their houses were within a few yards of each other. They both used the same underground tank for their water supply. They were both coal-miners and worked in the same pit; the one was thirty-six, and the other thirty-eight. Both were married and had families of several children, but all these, as well as the two wives, escaped all traces of dysentery.

It is more than a coincidence that two cases should occur under such circumstances. There is probably a common source of infection.

Lafleur says that concerning the source of the amœbæ and mode of infection little can be positively stated, for the life history of the amœbæ outside the body is not known, but in all probability the path of infection is the water supply.

A microscopical and bacteriological culture examination of the tank water was accordingly made. This tank was very dirty, and it had not been cleaned out for many a day. It was sunk into the rock for about a dozen feet, and was covered with a brick dome. The tank was not watertight, for the water never rose above a certain level. The source of the water was the rain catchment from the iron roofs of the two houses, but the earthenware pipes which conveyed the water to the tank were broken, and allowed surface water to flow in also.

The water had a heavy, brown sediment. Amongst other organisms some bacilli very like varieties of *bacillus coli communis* were found, but no amœbæ.

The water used at the mine itself appeared to be good water, and was brought some few miles in iron pipes, and its source was to all intents and purposes uncontaminated. Yet, if this mine water were the common source of infection, other cases ought to have occurred in the small mining township of Balgownie. It may be a coincidence, but three years ago a patient from this mining camp was operated upon in the Sydney Hospital by Dr. Goode for a large solitary abscess of the right lobe of the liver. It is true the abscess did not perforate the diaphragm and involve the lung as amœbic abscess so frequently does. The pus was not examined, but at all events it did not appear to be of hydatid origin. The patient recovered.

Dr. Wade, of Wollongong, also reports another case from the same district—one in which he opened a large abscess in the lower part of the thorax, which communicated through the diaphragm by two well-defined holes with an abscess cavity in the upper part of the right lobe of the liver. The patient died. In all probability both this case and Dr. Goode's were of amœbic origin, as well as others which Dr. Wade can recall, but in which no examination of the pus was made.

The prognosis of amœbic dysentery is not very good; of amœbic abscess it is very bad—all of the three cases have died. Most authorities place it at from 80 to 95 per cent.

Treatment.—Prophylactic. Although the hamlet of Balgownie looks very pretty, nestling on the mountain slope, and although it ought to be salubrious, lying so exposed to the ocean breezes, and although the mine is a good and safe one to work in, yet a doctor could hardly recommend the place as a health resort.

If duties compel residence in the neighbourhood, the first readily adopted prophylactic measure is the boiling or careful filtering of the water.

The complete life history of the amœba is, unfortunately, unknown. If it were, the disease might be combated and overcome in the same way as malaria has been partly conquered.

From dejecta of amœbic dysenteric cases there appears to be little risk of infection, for the organism does not live long in the motion after the latter is passed. It is usually said to disappear so soon as the motion becomes acid, but something more than mere acidity is apparently necessary, for in one of the cases the discharge from the abscess was markedly acid, and yet the amœbæ were very numerous and very actively mobile.

When the diagnosis of amœbic dysentery is established, the diet should be such as is recommended for all cases of dysentery—light and nourishing. Various drugs have been recommended as specifics. Quinine in weak solution destroys the motility of amœbæ outside the body, and so the drug has been largely used. In the Sydney Hospital it did not yield good results. One case was given a thorough trial. Quinine by mouth and quinine as a spray to the abscess cavity, but all without any real and appreciably good result. Bismuth has been recommended, but surely any means which would tend to constipation would but aggravate the trouble by retaining the amœbæ in the bowel, and giving them the opportunity of starting fresh ulceration. Ipecacuanha was tried in small doses, but only for a short time.

Dr. Verco suggests potassium iodide. Case 4 was treated with this drug. He was given twenty grains three times a day, and began to feel the usual effects of a heavy dose of the drug. The dysentery did not lessen under its influence.

In South Africa a preparation from the plant *Monsonia ovata* is used. This was also tried in case 4, but a protracted trial was not given, as there was difficulty in obtaining a sufficient supply of the drug. The patient improved whilst taking the drug, but too much importance must not be attached to the occurrence.

Perhaps most good is likely to be done by trying to reduce the number of amœbæ in the bowel by the use of enemata containing quinine in solution. This was tried in case 3, and although the patient died, still the method cannot be condemned on that evidence alone.

With a soft rubber tube, and the patient in the knee-elbow position, or lying on his left side, with the buttocks raised, a couple of pints are carefully injected into the bowel. If the patient complain of pain, the injection should be at once stopped. It should be retained for fifteen minutes, if possible.

In the carrying out of this procedure it is safest to use the soft rubber tube with funnel rather than a Higginson's syringe, as the pressure can be more gradually regulated. Even with the tube and funnel care must be exercised, and the latter not raised too high, as there is a certain amount of risk of rupturing a bowel partially destroyed by the excavations of the amœbæ.

Unfortunately, the cæcum is the commonest site of amœbic ulceration, and so most difficult of access by enemata; but not infrequently other parts are involved, and sometimes the whole of the large intestine, and perhaps a few inches of the small.

The strength of the solution used was from 1 in 1,000 up to 1 in 250. It will wash away many of the amœbæ, and probably kill not a few. If the amœbæ can be killed in their original hunting-field, the chance of hepatic abscess must be much diminished.

In treating such a disease as this, it appears to be all-important that the organism should be attacked and killed at its very fountain head. So long as there are amœbæ in the submucous coat of the bowel, so long are they free to increase and multiply and attack fresh fields and pastures new, either in the bowel itself or in the liver. Hence it appears to be most rational to adopt every possible means to kill the amœbæ in their primary hunting-ground. Dr. Verco, as mentioned, suggested attacking them through the blood stream by using potassium iodide, hoping that this drug might have the same bactericidal power over the amœbæ as it possesses with actinomycosis. Would it not be rational to adopt more energetic directly local means? Quinine is known to be most inimical to the amœba. Rectal injections are but imperfect means of reaching amœbæ in the cæcum, but, by opening the cæcum and sluicing through with a quinine solution from the cæcum to the anus, a direct and more perfect local action would be obtainable. The cæcum has already been opened successfully in cases of membranous colitis, with the object of giving the bowel rest[11] In amœbic dysentery a double purpose would be served by such an operation—namely, the local action of the quinine solution as well as the mechanical rest. Even the washing away of the amœbæ on the surface of the mucous membrane would lessen the chance of further ulceration, even if it did not control already existing ulceration.

The seriousness of amœbic ulceration surely justifies a serious remedy.

In conclusion, then, we have a disease which to these colonies is new, or, perhaps we should say, has been but lately recognised. It may be that the amœbæ were introduced but lately from some far distant country—nowadays, we need go no further afield than the New Hebrides—but, if this were the case, we would hardly expect to find them in a small village a few miles from Wollongong, which is a seaport only in name, visited by no oversea vessels, but by a few small coasting craft, chiefly colliers from Sydney. The amœba was here, no doubt, long before our time. Looking backward, it is a little difficult to trace his still and silent footsteps, but no doubt he has done his work none the less surely, although unacknowledged. Here and there a surgeon may recall a case of hepatic abscess, which appeared atypical of hydatid; it contained no cysts, and did not run the same smooth course to recovery which he had expected. The physician, from the limbo of forgetfulness, can just remember a case of intractable dysentery which had an irregular and intermittent course, and always lit up afresh just when he had satisfied himself of a satisfactory cure.

We can refer to hospital records and the dead past of the *post-mortem* reports. In the Sydney Hospital records we find for the six years, 1892 to 1897 inclusive, there were thirty-four cases of dysentery, with a mortality of 30 per cent.—or half as high again as the highest estimate given for dysentery amongst Europeans in India. Probably nothing definite can be deduced from these figures in respect to amœbic dysentery. Of abscess of the liver there were nineteen cases, with a death rate of 63 per cent. This, surely, is a very high percentage of mortality, and particularly so when we remember that it is probably much modified by suppurating hydatids which not infrequently recover. Can it be that the amœba largely accounts for a death rate which is 5 per cent. above that of the Indian army[9]?

The *post-mortem* records of the Sydney Hospital point more definitely to the possible workings of amœbæ. For the seven years—1892 to 1898, inclusive—there were twenty-three deaths from, or associated with, abscess of the liver.

Of these 50 per cent. are due to hydatid, a comparatively small percentage (15 per cent.) is secondary to suppuration in the appendix—gall, bladder, &c.—or associated with a general pyæmic infection. The remaining 35 per cent. of the abscesses are definitely said to be associated with ulceration of the large intestine.

Manson[10] says that in England 50 per cent. of hepatic abscesses are amœbic in origin. Probably all of these cases come from distant countries, and do not arise in England, and so we can make no fair comparison with these statistics. Is it not strange, however, that in these lands, where hydatid disease and hydatid abscess in the liver are so very common, that statistics of hepatic suppuration must be largely in the ascendant, yet there are 35 per cent. of cases *not associated with hydatid, but with an ulceration of the large intestine.* May not many of these be of amœbic origin? It is true that the amœba is much more deadly and claims far more victims from its hosts than the hydatid; but, after making all due allowance, it is highly probable that in Australia some cases of dysentery, and not a few deaths, are due to amœbæ.

NOTE.—With a lens magnifying three to four hundred diameters, the amœbæ were readily recognised in the sputum or abscess discharge when a thin film of fresh sputum or discharge was mounted with slide and cover glass. In the stools the mucus contained the majority of the amœbæ. No staining process is necessary, although with thionine blue a good differential staining result is obtained.

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DR. GILL congratulated the author on his paper. It was the result of conscientious work; and, as far as he knew, he was the first in Australia to describe such cases. The recognition of amœbic ulcers in the large intestine had thrown some light on what was a very obscure region to the pathologist.

"THE BRAIN FEVER OF THE DRAMA."

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Mr. President and Gentlemen,—

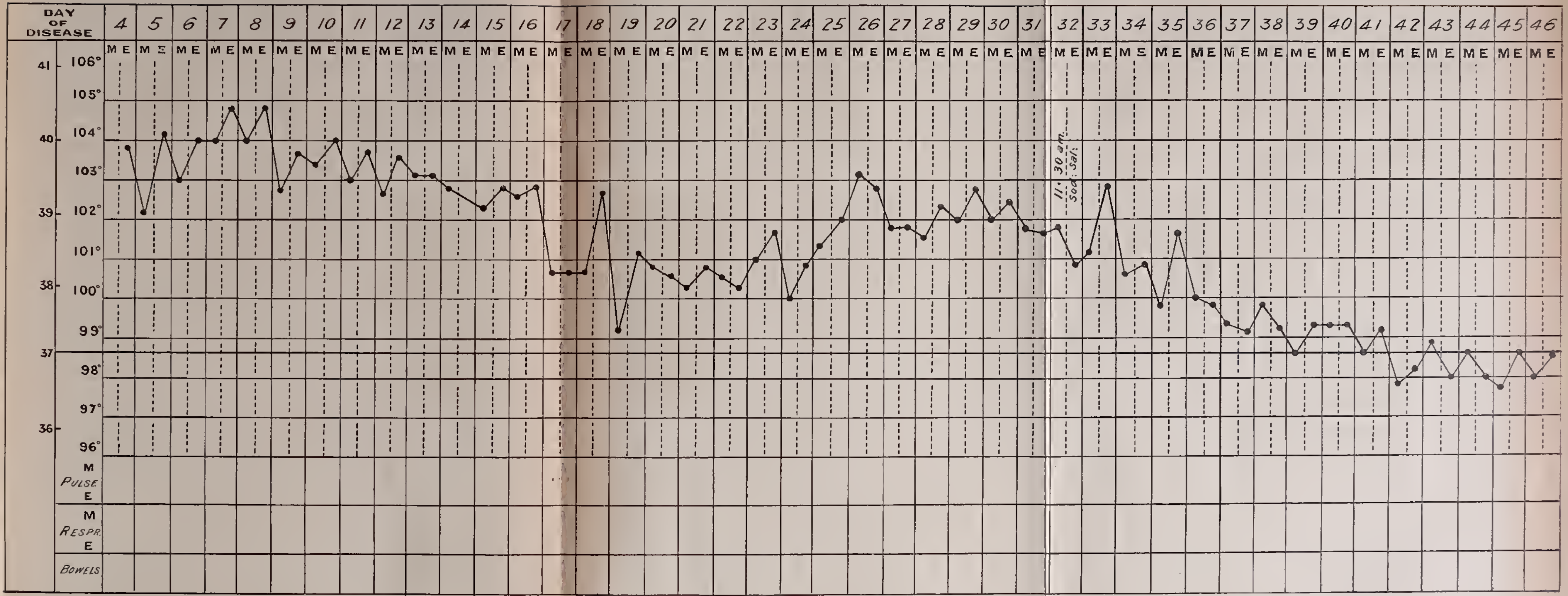
I feel that in reading a paper under the above heading some introduction is demanded. I intend to give such an introduction, and, at the same time, to ask physicians to bear with a surgeon who has essayed to read a paper in the medical section. I by no means claim to have discovered a new disease. The case I am about to describe presented symptoms which were of an extremely interesting character, and which called for a reasonable explanation.

I have endeavoured to give such an explanation, and only regret that I am unable to quote several cases of a similar character. This may be rectified. There may be amongst my audience others who have seen cases of a similar character, and who were unable to classify them.

All medical men who, for relaxation and pleasure, go to the theatre, will remember how, at the end of the first act of a modern drama, or possibly in the second, the heroine receives a frightful shock at seeing the villain attempt to shoot or stab her lover. She thinks her lover is dead, and promptly has "brain fever." It is an invariable consequence of severe mental shock in the case of the heroine. Six or twelve months are supposed to elapse before the next act. The exigencies of the play demand an interval. The heroine is not ill all the time; but, judging by the pallor of her face and the dressing wrap she wears in the next act, her convalescence has been delayed, and she always leans somewhat heavily on the rector's (her father's) arm. You do not have an opportunity of watching her illness. You only ascertain from the stage nurse that the heroine was desperately ill for some weeks, and was unconscious, and that her mind was a blank from the time of the shooting or stabbing until one day she recognised that she was being kindly attended to by loving hands. Could it have been enteric? No. For years I had wondered what this illness was. I had no opportunity of seeing the heroine after the shock—the curtain had dropped—except when the audience demanded her appearance before the curtain to receive its plaudits. In the next act she was convalescent. She had no sequelæ to guide one to a diagnosis, except mental debility and fear of another appearance of the villain. That did not help one much. At last I met such a case in actual practice, and as it must needs be interesting to the profession I herewith narrate it:—

Mrs. A., aet. 47, nullipara, married nineteen years, while apparently in her ordinary health, received news by telephone one morning, while at breakfast, of the sudden death of her sister, who resided some eight miles away. She at once drove to her sister's house, and was busily engaged during that day in duties connected with her sister's decease. She was deeply attached to her sister, and felt the loss most acutely. She returned to her own home that night—a drive of about eight miles—and again next day drove to her sister's house, and returned about 4 p.m. to her home, and went immediately to her bedroom with a splitting headache, and lay down on the sofa. Both days were windy and cold for the time of year. On account of excitement she had taken hardly any food on these two days. She was a very nervous woman. On leaving the sofa to go to bed she fainted. I have no accurate account of the case for the next two days, 2nd and 3rd February, but on 4th February her p.m. temperature was 103·8. The chart gives a good idea of the progress of

To accompany Dr. Hope Lewis' paper.



her complaint from that date. I saw the case in consultation with Dr. A. C. Purchas on 4th March, the thirty-second day of her disease, and her condition was then as follows:—Her body had kept up its fat very well; her pupils reacted to light sluggishly, and were unequal; her speech was slow and tedious in utterance; she had distinct "tâche cerebrale" on the skin of her chest; her tongue was red, clean, and dry; pulse thin and very rapid; she had the general appearance of a meningeal case; her symptoms all through had a neurotic character; she would say she could not swallow; if asked to turn round, she would say she could not do so; she had great intolerance of light and sound; the room was kept dark, and tan laid on the road near the house; she never had any diarrhœa, nor had she any spots; she had a great deal of flatulence and retching, and had bilious green vomiting; she complained of aching in the legs, which were rubbed well and thereby somewhat relieved; she was very fat at the commencement of her illness, and at the end, though the legs and arms got thin, the abdomen and back kept fat; she was very flushed at times; she had fluid food principally, and water and nutrient enemata as necessary; she took very little food by the mouth; she lost her hair during her convalescence. My diagnosis of her case at the time of the consultation was that she was suffering from uric acid meningitis, and with Dr. Purchas's approval she took 20-grain doses of salicylate of soda. There was a marked improvement in her general condition from this time. My opinion of this case is that it was a case of deposit of urate of soda on the meninges—in the same way as that salt is showered on the articular surface of a joint. Dr. Purchas, who has had a very large experience in enteric cases, was certain it was not a case of typhoid, and was at a loss to classify the disease under any satisfactory heading. Doubtless many practitioners have had similar cases of a feverish, prolonged attack following worry or serious and sudden mental shock, which have run a certain extended course to a successful termination, aided by or in spite of medicines. On questioning our patient and her husband, it was ascertained that she had rheumatic pains prior to this attack for some considerable time, and that she was of a rheumatic diathesis. Her pains had been most severe in the posterior cervical muscles and fascia.

Since observing this case, I have come across another patient who some years ago had an illness of a very similar character to the one already described, and which also was caused by, or at any rate came on after, a severe mental shock. This second patient's hair turned white during the illness, and has remained so since. Her age was thirty-eight at the time of her attack. I am well aware that in describing this case as one of uratic meningitis I am treading on delicate ground, but since my personal acquaintance with Dr. A. Haig, commenced some five years ago, I have been keeping a sharp lookout for cases that one can fairly and reasonably classify as being due to the vagaries of that potent toxic agent. This, I submit, is a reasonable explanation of the cause of the illness before described. It is, no doubt, a rare manifestation of the uratic storm, but that uric acid causes migraine is now an established fact. In the case of migraine, the position of uric acid is in the blood in greater proportion than it is in the large glands, joints, or muscles, and with it in that position we get high arterial tension. If, when in this condition, we get an injury or a severe impression conveyed to any particular part, my theory is that at that spot the uric acid storm bursts and expends its energy.

I saw Mrs. A. just before leaving Auckland. She is now in good health, with the exception of rheumatic pains about shoulders. She informs me that her mind was a complete blank from the second day on which she visited her deceased sister's residence until her convalescence commenced. She was not aware that she had been seen by me until she recovered.

A CASE OF OMENTAL CARCINOMA.

By J. R. Purdy, M.B., C.M. (Aberd.)
Examiner in Midwifery and Gynæcology, Otago University, New Zealand.

Mr. President and Gentlemen,—

The possibility of the occurrence of a primary carcinoma of the peritoneum has been disputed by many pathologists. Certainly, researches based on the ætiology of epithelial growths, together with the lack of authentic records of such cases, make one approach the subject with a good deal of scepticism.

The histological examination of the tumour and the absence of any other direct manifestation of the disease in the following case, however, clearly show that the condition, though so extremely rare as almost to be unique, is not beyond the bounds of possibility.

The patient, S. F., Judgeford, N.Z., a farmer, aged 57, first consulted me on 21st April, 1899. Complained of feeling of fulness and heaviness in the gastric region, with eructations, constipation, and flatulence.

Family history, good. Father still living, 87 years of age. Mother died at the age of 65; cause of death not certain, but no suspicion of malignancy.

May 5th: Seen by my brother, Dr. John S. Purdy. Patient had materially benefited by treatment, which was strictly that for gastric catarrh, combined with laxatives.

May 12th: Patient, though free from pain, complained of loss of appetite, disinclination for work, and a feeling of general weakness. After a careful examination could elicit no physical signs.

May 16th: Complained of uneasiness in the epigastrium. As the age of the patient, together with his sallow anæmic appearance, suggested the possibility of some incipient malignant growth, he was again subjected to a rigorous physical examination, which, however, gave only negative results.

May 19th: Patient entered Wellington Hospital.

May 27th: Left hospital. Consulted Dr. Kington Fyffe, Wellington, who elicited no definite physical signs, but agreed that the case was strongly suspicious of malignancy from the history of loss of flesh and the subjective symptoms.

May 30th: Saw patient again at home. Noticed the increase of pallor and general weakness. He now complained of a feeling of nausea. Liver dulness normal. From first to last there was no vomiting and no jaundice.

June 2nd: Patient had severe attack of colic, constipated, abdomen distended, tympanitic. Relieved by enema.

June 6th: Increase of nausea. Attacks of colicky pain more frequent and severe. Abdomen now distended and tympanitic, with dulness in right iliac flank. Noticed bulging over gastric area. No tenderness on pressure over epigastrium. Slight tenderness over right iliac region.

June 9th: Patient losing flesh rapidly. Ascites developing. As there was no history of indulgence in spirits or any hepatic signs, cirrhosis was eliminated from the diagnosis. Sallow, pale complexion, with an expression of extreme anxiety, and the increasing despondency of mind with the signs of peritonitis, now made us give a diagnosis of peritoneal cancer.

June 13th: On palpation, fluctuation, irregular areas of dulness, and resonance over the intestines elicited on percussion. Temp. subnormal, with occasional elevations of pyrexia, especially after defæcation. The taking of food caused colic. Bowels only responded to enema. No vomiting, no melæna, no jaundice.

June 14th: Owing to persistence of pain, morphia prescribed.

June 15th: Consultation with Dr. Kington Fyffe. Patient becoming cachectic.

June 16th: Pain constant and severe, except when under influence of morphia. Cachexia rapidly progressing.

June 20th: Enema caused severe pain. Ascites well developed. As exhaustion was so extreme, thought unsafe to tap.

June 23rd: Patient collapsed after evacuation of bowel by enema.

June 27th: Moribund.

June 28th: Death.

Post-mortem.—On opening abdomen a mass of greyish-pink tissue was seen hanging from the greater curvature of the stomach, which proved to be the omentum, drawn up and lying across the abdomen as a firm, hard, solid mass. Tumour measured 3½ by 2 inches. It was impossible to dissect it away from the stomach. Weight, 28 oz. Ascitic fluid, 4 gallons; hæmorrhagic, with sp. gr. 1,025. The edges of the tumour showed fatty degeneration, and in parts it was soft, but the mass itself was distinctly hard. Liver perfectly healthy, gall bladder and ducts not affected. Pancreas not implicated. Spleen congested, but no pathological changes. There was no suspicion of any malignant growth in any other part of the viscera. The stomach itself gave no evidence of malignancy. There were peritoneal adhesions. Mesenteric glands unaffected. Under the circumstances the *post-mortem* could not be completed, so that the interior of intestine was not examined.

Microscopic.—Mr. J. A. Gilruth, Government Pathologist, cut and examined a series of sections. He reports as follows:—"The structure consists of a stroma of fibrous tissue infiltrated with epithelial cells. *Fibrous tissue:* This surrounds the growth in the form of a capsule, varying in thickness, and compasses the greater portion of the body of the tumour. As a whole the fibrous tissue is dense, but here and there lobules of loose areolar tissue are present, especially in the deeper portions. Underneath the outermost layers there is a small amount of hæmorrhage, the blood corpuscles distending the fibrous spaces. *Epithelial cells:* These are of the cubical and columnar variety, and are found most distinctly lining the interfibrous spaces. Unfortunately, the bad fixative employed, together with the length of time therein, prevents the best effects of the staining reagents being shown. These cells, however, are found in their most normal rapidly proliferating condition in the narrow spaces, whereas in the larger spaces they are mostly desquamated, the nuclei staining with difficulty. There is no appearance of basement membrane between the epithelial cells and the fibrous tissue. '*Kupper's bodies*,' *Cancer parasites and protozoa:* Unfortunately, with the methods of fixation employed, it was not to be expected these would be brought out by the staining reagents. However, both with Hæmalum and Eosine, and the Biondi-Heidenhain method especially, there can be observed small circular bodies in the epithelial cells. These have not the typical appearance of any degeneration, and they are not vacuoles. With the latter method of staining the small nucleus-like structure in these bodies stains red, whereas the nuclei of the cells proper stain green."

Clinically, the points to be gleaned from this case are the absence of definite physical signs at the outset. The rapid loss of flesh and strength, with an early development of ascites, combined with the age and history, distinctly pointed to a malignant growth; yet it was practically impossible to say with certainty that it was present. In a case of cancerous peritonitis the exudations are no doubt due in part to the compression of veins by the tumour, and in part to the inflammation set up. Though hydroperitoneum is generally due to some obstruction of the portal circulation, yet in this case cyanosis of the liver was very slight; yet the tributaries of the portal vein were affected to some extent by the mechanical pressure of the neoplasm. Consequently the disturbance of the functions of the stomach and bowels giving rise to gastric catarrh, the first manifestation in the above case, can be thus explained, combined with the general effect of such a growth on the nervo-vascular supply of the gastro-intestinal tract. The drawing-off of some fluid would have been a valuable aid to diagnosis; the fluid in cirrhosis and tubercular disease being usually clear; sp. gr. 1,012-1,025. In our case it was distinctly hæmorrhagic, sp. gr. 1,025. The cachexia developed so rapidly that it was considered unwarrantable to interfere, a precaution justified by the subsequent *post-mortem*.

As to the origin of peritoneal carcinoma, some pathologists assert that no true epithelial growth can originate from a mesoblastic structure. "But the mesoblast itself is mainly, if not entirely, derived from the two fundamental layers of the blastoderm, the epiblast, and hypoblast, both of which are the precursors of the epithelial tissues of the adult, and their potentiality for epithelial development, though ordinarily in abeyance, might [according to W. H. Allchin (Clifford Allbutt's System of Medicine)] be retained through the intermediate stage of the mesoblast." The alternative theory of the body cavity being a derivative outgrowth of the primitive alimentary canal, its lining membrane being continuous with the hypoblastic covering, explains the possibility of the development of true carcinoma in the pleuro-peritoneum. A parallel case is recorded by Dr. S. West. Those who still hesitate to admit that carcinoma can arise from the peritoneum would class such a tumour as an "endothelial sarcoma" or an "alveolar sarcoma." But in the above case the epithelial cells have no attachment to the fibrous walls, but look like foreign bodies pushed in, whereas in alveolar sarcoma and lympho-sarcoma the corpuscles are attached to the fibrous tissue, and are undoubtedly derived from the stroma.

As to the cause of cancer in this situation, I am inclined to favour the view of Professor Hamilton, my old teacher, that in certain individuals there is a peculiarly unstable condition of the epithelia generally which predisposes to the formation of cancer tumours, and it appears to be this instability which can be transmitted from parent to child, and which renders the tendency to cancer in this respect hereditary. In such persons the application of an irritant has a much greater effect than on other people. One of the great arguments for this theory is the fact of it occurring so frequently in the mamma, which is an unstable organ and goes through various phases—(1) The evolution of the gland at puberty; (2) The involution of the gland in old age; (3) The disturbance of the epithelium during lactation. In all these conditions epithelium is intimately concerned, and is therefore unstable.

In the case read, the only history was a strain which the patient said he received six months previous to the development of severe symptoms. That a further reason is necessary with regard to this theory of epithelial instability is that in the cow, the udder is seldom the seat of carcinoma. Yet since the discovery by Plimmer that protozoa are the direct exciting cause, irritation, perhaps, paving the way, the fact that the udder of cows is not a favourite site of the disease only shows that it is not a focus for the development of cancerous parasites.

The recent experiment of Mr. Lack, in which he took epithelial cells from a healthy ovary of a rabbit and injected an emulsion into the peritoneum of another rabbit, the animal dying fourteen months later, with masses of growth in the abdominal and thoracic cavities characteristic of typical ovarian cancer, whilst showing that cancer can arise from healthy epithelium proliferating in a structure foreign to it, shows that cancer epithelium is merely normal epithelium only out of place. The tumour might have been classified under the old nomenclature as a cylindroma. But this term is now limited to growths of an undoubted sarcomatous origin. Owing to the incompleteness of the *post-mortem*, it is possible that there may have been a cancerous ulcer somewhere in the alimentary tract. The tissue is, however, undoubtedly carcinomatous, and the tumour described is so extremely rare in this situation that in the enormous mass of medical literature that now exists only one or two isolated cases are found.

References.

Clifford Allbutt's System of Medicine.
Hamilton's Pathology.
Osler's Medicine.
Finlayson's Manual.
Roberts' Medicine. Strümpfell.
The Transactions of the Pathological Society.

Dr. Gill thought that as the *post-mortem* examination was incomplete—the intestines not having been examined—it was impossible to say that the growth was primary in the omentum. If the growth were primary in the omentum, it would be a unique case; no record of such a case could be accepted, unless the *post-mortem* examination were perfectly complete. It was not justifiable to assume that there was no growth in the intestines because there had been no symptoms during life; every pathologist had met with cases in which primary growths had been found in organs where they had not been suspected during life.

Dr. Isbister: Such a form of growth as a primary carcinoma of omentum would be, indeed, a pathological curiosity, and it would be very much easier to account for its origin by supposing that it was secondary to some primary growth elsewhere. The difficulty of finding the primary growth is sometimes so great that even after a prolonged search it may not be found, as it may be very small and have given no symptoms during life. For this reason it is to be regretted that a more complete *post-mortem* examination could not be obtained. We are sometimes too ready to think that a macroscopic examination of the dead body will settle all doubts; such is not always true—*e.g.*, it may be that with a typhoid infection there is no ulceration of the ileum, and the presence of the specific organism can only be proved by a culture preparation from the spleen or other organs. The same absence of macroscopic signs may be evidenced by a certain few rapid deaths from acute septic poisoning.

EXCRETION OF URINARY WATER AND UREA FROM THE HUMAN BODY BY DIMINISHED KIDNEY WEIGHT.

By John B. Nash, M.D., M.R.C.S., Wallsend, New South Wales.

The subject-matter for this paper was suggested by a study of lectures published in the *Lancets* of March the 19th and 26th and of April the 2nd. They were entitled "Observations on the Pathology of the Kidneys," by Professor John Rose Bradford, Professor Superintendent of the Brown Institution.

As there were three patients in my practice with kidney weight diminished, it occurred to me that an estimation of the quantity of urine and of its nitrogenous constituents, excreted in a period extending over several weeks, might be of sufficient interest to make a suitable contribution to this section of the Congress; and it would apply to the human individual the principles laid down in the lectures quoted, and also contribute somewhat to our knowledge of the behaviour of the kidney tissue when its total weight, relative to the rest of the body, is diminished.

In two of the cases, one kidney—*i.e.*, one-half of the total kidney weight—and in the third, slightly less than one-fourth of the total kidney weight, had been removed.

Cases.

1. D.F., female, aet. 35 years. On March the 24th, 1896, the right kidney was removed. The 16th day of March, 1899, she was confined of a full-sized healthy male child. The estimations were made about three years after the operation, and at the time she was suckling her baby. They were begun on the 27th day of April, 1899, and they were continued daily till the 14th of June, 1899, a period of exactly seven weeks.
2. W.M., male, aet. 20 years. On May the 29th, 1897, slightly less than one-half of the right kidney was removed. The estimations, extending over a period of fifty-two days, were made two years after the operation. They were begun on the 24th of April, and ended on the 14th day of June, 1899.
3. J.N., female, aet. 56 years. On April the 22nd, 1899, the left kidney was removed. The estimations were begun on the day after the operation, and they were continued for fifty-three days, or up to the 14th of June, 1899.

Cases 1 and 2 had time to settle into the normal condition, and they exhibit the behaviour of diminished weight of renal tissue in bodies that had assumed a normal standard after complete recovery from the affections for which they were treated. There is one apparent difference, in that the first patient suffered from disease, the second met with an accident; this is of no moment, as the excreting tissue *in corpore* in each case must have been of a healthy type. Both individuals were upon an ordinary mixed diet, and they carried on their customary duties.

Case 3 shows the behaviour of one kidney immediately after it had been deprived of the assistance of its fellow, in which slightly more than half the acting structure was capable of performing the function, and, further, it shows how this single organ was equal to the requirements of the rest of the tissues in passing away the necessary quantity of water, nitrogenous extractives, and other solids after a somewhat serious operation.

In all the three cases the specimens examined were taken from the mixture of the total amount collected daily.

The standards of comparison used are those given in "Campbell Black: Urine and Urinary Analysis; edition 1895." Herein the quantity of fluid excreted by the kidneys in twenty-four hours is given as 1,000 to 1,200 c.c. in the female, and 1,400 to 1,500 c.c. in the male. The urea elimination is stated at about 25 gms. daily in the female, and 18 gms. to 30 gms. daily in the male.

The volumetric analysis of the urea was made by the hypobromite of soda process with the ureameter of Doremus.

That one kidney is capable of doing the scavenging for which two are generally provided has been demonstrated upon many occasions in the after history of persons from whom one kidney had been excised.

The more thoroughly we realise the capacity of a diminished weight of tissue to perform all the particular requisite functions of the body which it was ordained to fulfil, with the more self-satisfaction do we attack and remove a diseased area of this tissue. The experiments in the dog (and they are being found to apply to the human individual) have thrown much light on the physiology and pathology of the kidney. With the function of the healthy organ as an excreting tissue in some of its relations, this paper is intended to deal.

Professor J. Rose Bradford writes, "An animal (dog) with diminished kidney weight is unable to excrete a concentrated urine. . . . . The urea is eliminated by an increased flow of water occurring—*i.e.*, a hydruria follows; in this way the larger amount of urea is got rid of without the percentage in the fluid being greater. In a normal animal, the urea is got rid of by the urine containing a greater percentage of it."

D.F.—In the seven weeks during which the daily estimations were made, the highest concentration was 3·47 per cent. of urea once, 2·8 per cent. once, and 2·79 per cent. once; the daily average throughout the whole period was 1,520·5 c.c. of urine, containing 1·6 per cent. of urea; this is comparable to the 2·05 to 2·5 per cent., which is the approximate proportion excreted in the healthy female with two kidneys. The one kidney in this woman is able to excrete a urine in all ways similar to that we find in a normal woman.

The highest proportion, 3·79 per cent., is sometimes demanded of the kidneys in the average healthy female with variations in diet, and upon the particular day of this excess there was a relative hydruria, 1,875·5 c.c. of urine having been voided. The greatest quantity of fluid that was passed in any one day was, on two occasions, 2301·7 c.c., with urea 1·89 and 1·19 per cent. respectively.

W.M.—During the fifty-two days over which the estimations extended in this case, the highest concentration was 3·59 per cent. of urea, with an excretion of 937 c.c. of urine.

On fifteen occasions the proportion was over 2·25 per cent., while for the whole period the average quantities daily were 1,167 c.c. of urine and 2·13 per cent. of urea. This would seem to indicate that the kidney and one-half passed away a sufficiency of nitrogenous material, but that there was a slight deficiency in the quantity of fluid.

That the renal tissue left was capable of passing away more than an average daily amount of urine is evident, as 1,506 c.c., 1,534 c.c., 1,591 c.c., and 1,648 c.c. were voided on four of the days. There was no marked hydruria or polyuria.

J.N.—In the second and third twenty-four hours after the operation, the highest concentration of the urine was seen, the single kidney excreting a fluid with 4·56 per cent. of urea. Moreover, the largest quantity of urea excreted during any one day was 29·8 gms., and that in the second twenty-four hours after the operation, with urinary water, 653·5 c.c.; in the third diurnal period 28·3 gms. with 625 c.c. of water; in the following like time, 27·5 gms. with 653 c.c. of water. On the fiftieth, fifty-first, fifty-second, and fifty-third days the quantity of urine voided was respectively 1,676 c.c., 1,562 c.c., 1,428 c.c., and 1,335 c.c., with ·79, ·90, 1·02, and 1·49 per cent. of urea. The average for the fifty-three days was 889 c.c. of urine, with 14·1 gms. of urea, or 1·57 per cent. The quantity of fluid voided steadily increased, and towards the end of the estimations there was practically a hydruria, but not a polyuria

The charts attached show the daily amount of fluid in cubic centimetres and of urea in grammes.

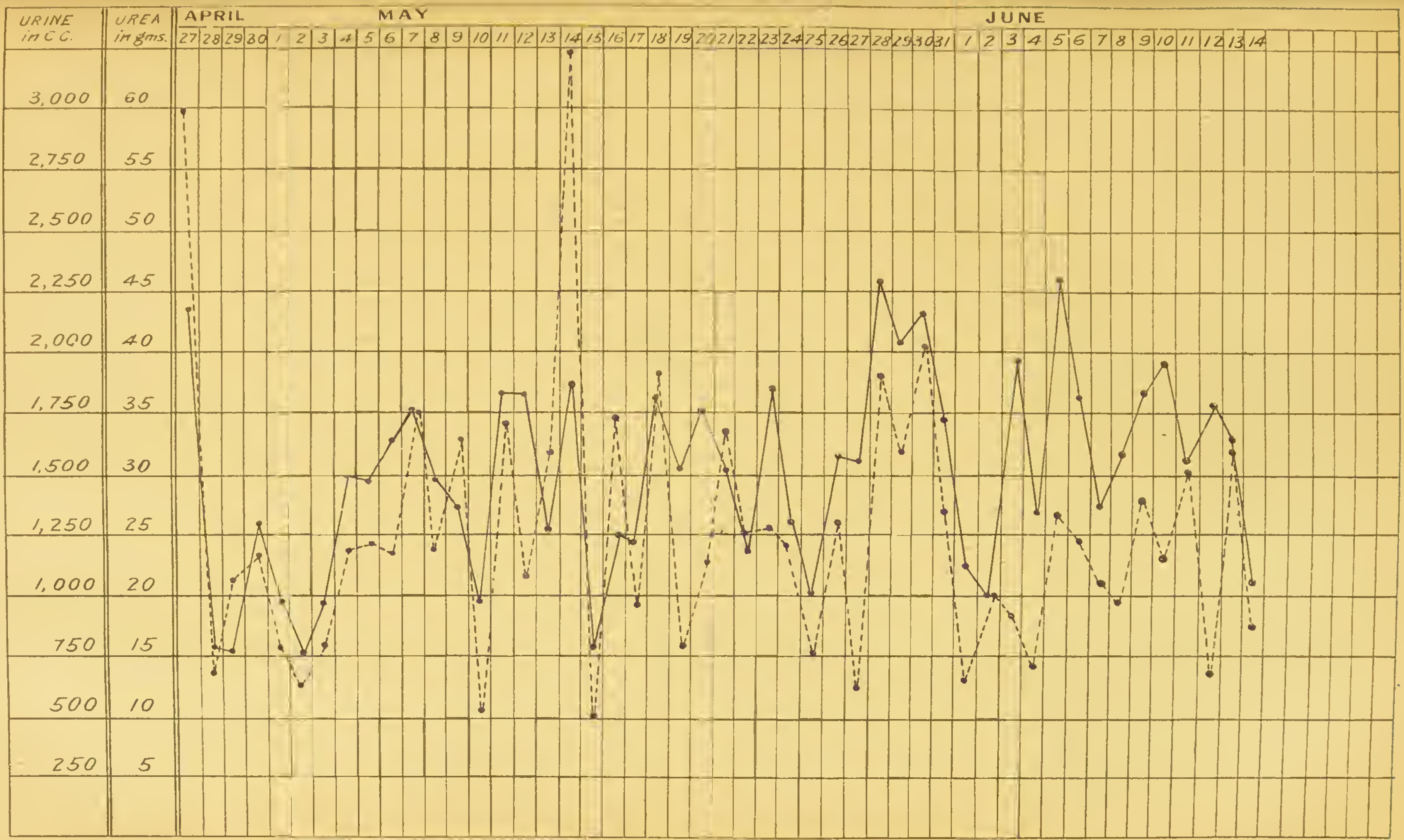
D.F.

Date.	Urine, C.C.	Urea, Gms.	Sp. Gravity.	Date.	Urine, C.C.	Urea, Gms.	Sp. Gravity.
27 April ...	2,188	59·9	1,021	22 May ...	1,193	25·1	1,022
28 ,, ...	795	13·6	1,015	23 ,, ...	1,875	25·6	1,014
29 ,, ...	795	22·2	1,024	24 ,, ...	1,307	24·5	1,028
30 ,, ...	1,307	23·8	1,020	25 ,, ...	1,023	15·1	1,022
1 May ...	994	15·8	1,018	26 ,, ...	1,591	27·2	1,016
2 ,, ...	767	13·1	1,017	27 ,, ...	1,562	12·5	1,013
3 ,, ...	994	15·9	1,014	28 ,, ...	2,301	43·3	1,020
4 ,, ...	1,506	24·	1,014	29 ,, ...	2,046	32·6	1,015
5 ,, ...	1,449	24·7	1,014	30 ,, ...	2,159	41·8	1,020
6 ,, ...	1,648	23·5	1,016	31 ,, ...	1,733	27·7	1,015
7 ,, ...	1,761	35·1	1,022	1 June ...	1,136	13·6	1,012
8 ,, ...	1,477	23·5	1,014	2 ,, ..	1,023	20·4	1,021
9 ,, ...	1,392	33·3	1,022	3 ,, ...	1,903	18·4	1,010
10 ,, ...	937	10·7	1,012	4 ,, ...	1,335	14·4	1,015
11 ,, ...	1,847	34·7	1,019	5 ,, ...	2,301	27·5	1,010
12 ,, ...	1,847	22·1	1,012	6 ,, ...	1,818	24·9	1,012
13 ,, ...	1,278	32·	1,022	7 ,, ...	1,135	21·3	1,015
14 ,, ...	1,875	65·2	1,026	8 ,, ...	1,619	19·4	1,011
15 ,, ...	795	10·4	1,011	9 ,, ...	1,875	27·8	1,014
16 ,, ...	1,250	34·2	1,018	10 ,, ...	1,903	22·8	1,011
17 ,, ...	1,221	19·5	1,012	11 ,, ...	1,562	30·1	1,023
18 ,, ...	1,845	38·	1,022	12 ,, ...	1,761	14·	1,010
19 ,, ...	1,534	16·6	1,010	13 ,, ...	1,676	32·5	1.022
20 ,, ...	1,790	23·4	1,014	14 ,, ...	1,079	17·2	1,015
21 ,, ...	1,506	34·3	1,022				

W.M.

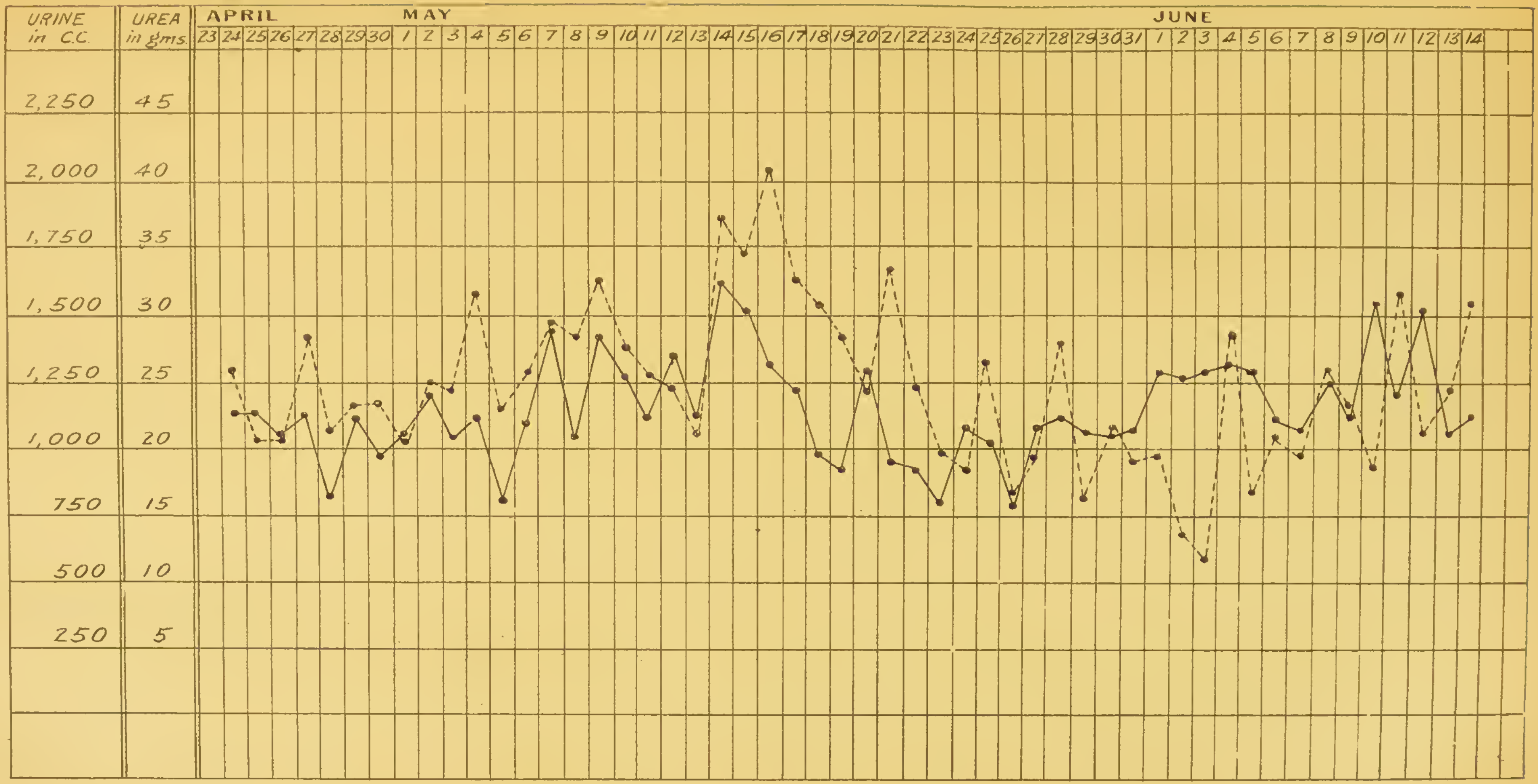
Date.	Urine, C.C.	Urea, Gms.	Sp. Gravity.	Date.	Urine, C.C.	Urea, Gms.	Sp. Gravity.
24 April ...	1,136	25·9	1,020	20 May ...	1,335	24·3	1.017
25 ,, ...	1,136	20·7	1,020	21 ,, ...	937	33·7	1,026
26 ,, ...	1,078	20·9	1,023	22 ,, ...	909	24·8	1,024
27 ,, ...	1,136	28·5	1,025	23 ,, ...	795	19·9	1,026
28 ,, ...	852	21·8	1,027	24 ,, ...	1,108	18·3	1,016
29 ,, ...	1,136	23·3	1,024	25 ,, ...	1,023	26·8	1,023
30 ,, ...	994	23·8	1,026	26 ,, ..	767	16·6	1,018
1 May ...	1,079	20·3	1,022	27 ,, ...	1,108	19·	1,018
2 ,, ...	1,221	25·	1,018	28 ,, ...	1,136	33·7	1.026
3 ,, ...	1,079	24·6	1,022	29 ,, ...	1,108	16·4	1,016
4 ,, ...	1,136	32·4	1,027	30 ,, ...	1,079	21·5	1,016
5 ,, ...	852	23·3	1,027	31 ,, ...	1,108	18·3	1,018
6 ,, ...	1,136	25·9	1,024	1 June ...	1,307	19·3	1,015
7 ,, ...	1,463	29·2	1,020	2 ,, ...	1,278	13·8	1,014
8 ,, ...	1,079	28·3	1,023	3 ,, ...	1,307	11·9	1.010
9 ,, ...	1,449	33·	1,020	4 ,, ...	1,364	28·	1.026
10 ,, ...	1,278	27·6	1,020	5 ,, ...	1,307	17·1	1,012
11 ,, ...	1,136	25·9	1,020	6 ,, ...	1,136	21·3	1,020
12 ,, ...	1,364	24·8	1,018	7 ,, ...	1,108	19·5	1,019
13 ,, ...	1,165	21·2	1,020	8 ,, ...	1,250	26·3	1,020
14 ,, ...	1,648	37·5	1,016	9 ,, ...	1,136	23·2	1.020
15 ,, ...	1,506	34·3	1,019	10 ,, ...	1,591	18·1	1,014
16 ,, ...	1,335	41·1	1,024	11 ,, ...	1.193	32·	1,025
17 ,, ...	1,221	33·4	1,022	12 ,, ...	1,534	21·	1,015
18 ,, ...	994	31·1	1,026	13 ,, ...	1.079	24 6	1,024
19 ,, ...	909	28·	1,024	14 ,, ...	1,136	31·7	1,026

D. F.



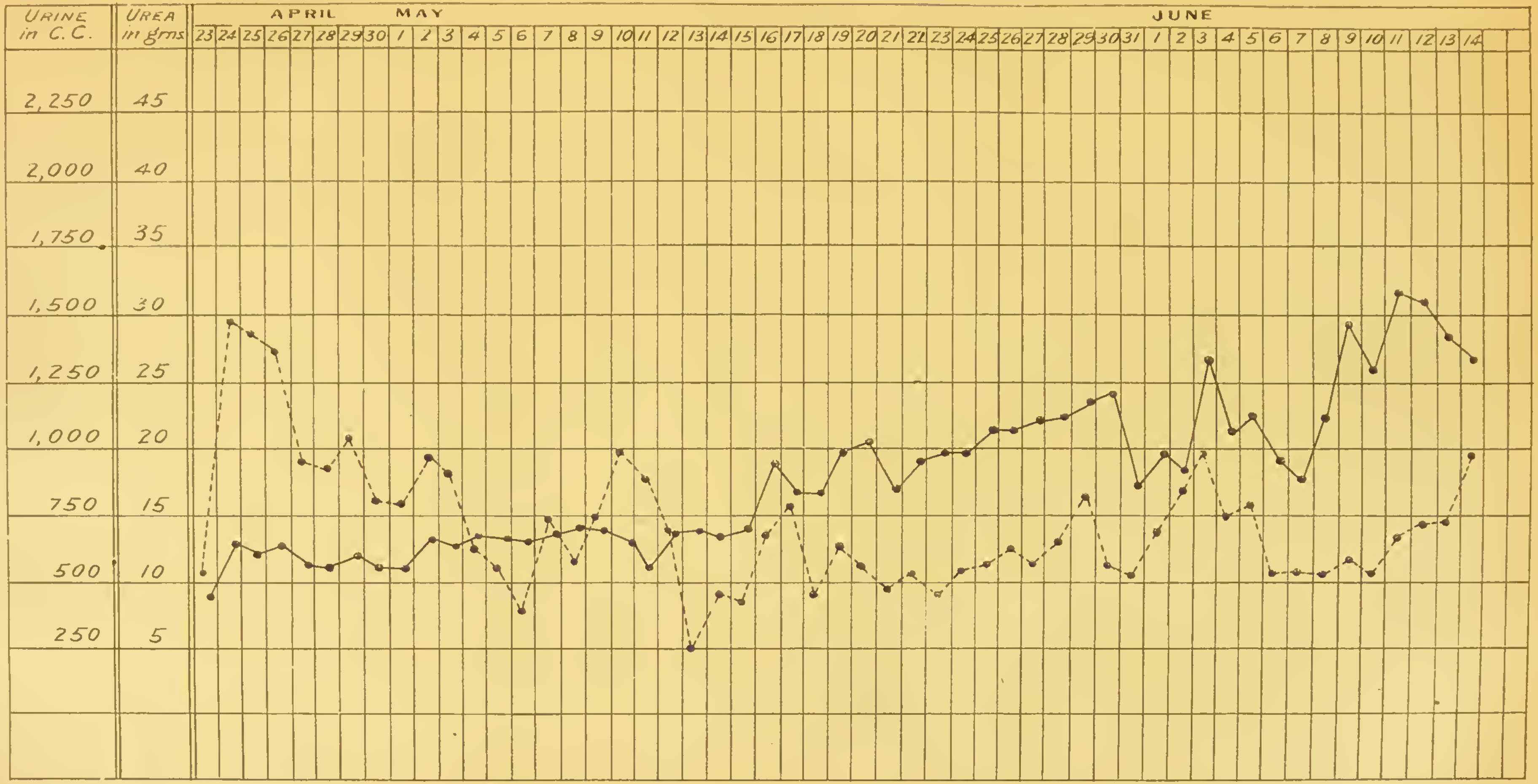
Black Line: Quantity of Urine. Dotted Line: Weight of Urea.

W.M.



Black Line: Quantity of Urine. Dotted Line: Weight of Urea.

J. N.



Black Line: Quantity of Urine. Dotted Line: Weight of Urea.

J.N.

Date.	Urine, C.C.	Urea, Gms.	Sp. Gravity.	Date.	Urine, C.C.	Urea, Gms.	Sp. Gravity.
23 April ...	454	10·37	1,027	20 May ...	1,023	11·	1,009
24 ,, ...	653	29·8	1,026	21 ,, ...	852	9·2	1,010
25 ,, ...	625	28·3	1,028	22 ,, ...	966	10·5	1,011
26 ,, ...	653	27·5	1,026	23 ,, ...	994	9·	1,014
27 ,, ...	568	18·8	1,024	24 ,, ...	994	10·7	1,014
28 ,, ...	568	18 1	1,024	25 ,, ...	1,079	11·	1,009
29 ,, ...	596	20·7	1,024	26 ,, ...	1,079	12·3	1,012
30 ,, ...	568	16·2	1,028	27 ,, ...	1,108	11·3	1,014
1 May ...	568	15·8	1,024	28 ,, ...	1,136	12·9	1,010
2 ,, ...	653	19·	1,020	29 ,, ...	1,193	16·3	1,015
3 ,, ...	625	18·1	1,022	30 ,, ...	1,221	11·1	1,011
4 ,, ..	682	12·4	1,015	31 ,, ...	852	10·2	1,013
5 ,, ...	653	11·1	1,016	1 June ...	994	13·6	1,014
6 ,, ...	653	7·7	1,011	2 ,, ...	909	17·1	1,017
7 ,, ...	682	14·3	1,018	3 ,, ...	1,335	19·8	1,018
8 ,, ..	710	11·3	1,014	4 ,, ...	1,108	14·5	1,016
9 ,, ...	710	14·9	1,018	5 ,, ...	1,136	15·5	1,014
10 ,, ...	658	19·3	1,018	6 ,, ...	966	10·4	1,011
11 ,, ...	508	17·5	1,020	7 ,, ...	795	10·9	1,013
12 ,, ...	710	13·7	1,016	8 ,, ...	1,136	10·3	1,010
13 ,, ...	710	4·9	1,010	9 ,, ...	1.449	11·5	1,010
14 ,, ...	653	8·9	1,014	10 ,, ...	1,278	10·2	1,010
15 ,, ...	710	8·1	1,010	11 ,, ...	1,676	13·3	1,011
16 ,, ...	937	13·3	1,012	12 ,, ...	1,562	14·2	1,012
17 ,, ...	852	15·5	1,016	13 ,, ...	1,420	14 6	1,014
18 ,, ...	852	8·7	1,014	14 ,, ...	1,335	19·8	1,016
19 ,, ...	994	12·5	1,012				

Weeks.	J.N.		W.M.		D.F.	
	Urine in C.C.	Urea in Gms.	Urine in C.C.	Urea in Gms.	Urine in C.C.	Urea in Gms.
I.	4,120	154·1	7,473	165·1	7,843	164·4
II.	4,404	100·7	7,970	180·8	10,173	175·1
III.	4,745	96·1	9,121	198 0	10,116	218·3
IV.	5,740	77·7	8,240	226·7	11,054	187 8
V.	7,075	74·3	6,848	159 2	12,418	200·3
VI.	7,644	101·1	8,553	129·4	11,423	140·7
VII.	7,871	83·5	8,724	158·0	11,480	151·0
VIII. (4 days) ...	5,995	62·0	(3 days) 3,751	77·3	...	...
Total (53 days) ...	47,598	749·6	(52 days) 60,684	1,291·9	(49 days) 74,509	1,237·7
Average per week	5,943·13	98·2	8,132·94	173·9	10,644·19	176·8
Average per day ...	898·01	14 1	1,167	24·9	1,520·59	25·25
Average urea in gms. per c.c. ...	0·01574 = 1·574%		0·02133 = 2·133%		0·0166 = 1·66%	
Average sp. gravity	1,015·4		1,020·6		1,016·4	

In the case of Mrs. D.F. a great variation between the minimum and maximum quantities in both urine and urea is to be seen.

Under 800 c c. of urine were voided on four days, while over 2,000 c.c. were passed on five days. During the whole period the average of 1,520 c.c. per day is well over the quantity voided daily by a healthy woman with two kidneys. Compared with the two other cases the urea variations were remarkable; more than 59 gms. were excreted on two occasions, the highest being 65·2 gms. on the eighteenth day of the estimations. Under 13 gms were produced three times. These figures show that the single kidney is capable of getting rid of twice as much water and three times as much urea in one day as is the average for two healthy kidneys. The woman was in perfect health on an average mixed diet, and suckling a fully-sized male child.

In the case of W.M., with his one kidney and one-half *in situ*, the urinary water was under 800 c.c. per day twice, and over 1,400 c.c. six times. For the whole period the daily average of 1,167 c.c. is about equal to what two healthy kidneys should excrete. The urea was below 14 gms. on two days, and 33 gms. or over on seven days. Between these two limits the quantities varied considerably. The loss of one-fourth of his kidney weight did not materially alter the excretion of either the fluids or the solids by the renal tissue. The patient was a healthy young male, of average activity, living on a mixed diet. If estimations were made on a like man under similar circumstances, but possessing his full proportion of kidney weight, much the same variations would be found in the daily quantity of fluids and solids passed.

J.N.—Here we have a condition more like to the dogs of Professor Rose Bradford, as the estimations were begun immediately after the operation. He writes—"All partial nephrectomies, single or double, slight or extensive, increase the amount of urinary water, to different degrees it is true; the amount of urea is not increased by either single partial nephrectomy, or by double partial nephrectomy, or yet by the removal of one kidney and a portion of the other, provided always that at least one-third of the original total kidney weight be left to the animal. . . ."

In J.N., at no time during the first five weeks did the urinary water reach 1,200 c.c. in one twenty-four hours, the nearest to that amount being 1,108 c.c. passed on the thirty-fifth day. This does not agree with the hydruria found in the dog. The urea for the first twenty-four hours stood at the normal healthy average; during the second, third, and fourth daily periods it was about twice the normal amount per oz. of urinary water, but only a little above the total required to be excreted daily by a healthy female with two kidneys. This differs from what happened in Professor Bradford's dogs, and shows that immediately after removal of one kidney in the human subject the kidney left *in corpore* is capable of excreting a somewhat concentrated urine. During the sixth and seventh weeks, the urinary water voided kept well up to a healthy average, while on the four days of the eighth week the quantity was about 300 c.c. above the average.

In these same periods the urea was about one-half of that usually excreted by two healthy kidneys. The chart as a whole shows a steady increase in the quantity of urinary water. At first, when this was small in amount, the patient had a low-tension pulse, she was unable to take much fluid wherewith to fill the bloodvessels, and a diminution in the filtering processes followed. After major operations there is usually wasting for the first few days; this is probably accompanied by an over-production of nitrogenous extractives in the tissues and blood, thence moving onwards to excretion through the kidneys. After the fourth day the urea fell to a normal degree, and only five times during the subsequent analysis was it up to 19 gms. in the twenty-four hours. The patient put on flesh, and soon regained more than her accustomed vigour.

In *The British Medical Journal* of the 29th July, 1899, Dr. W. L. Bell, of Lowestoft Hospital, England, reports a case of abdominal nephrectomy for renal calculus. At the end of the article he writes—"*Estimations of urine:* During the first twenty-four hours 14 oz. of highly-albuminous urine were excreted, but after this it remained free from albumen, and during the second twenty-four hours there were 15 oz., next three days 28 oz. each day, after which the amount passed generally varied from 35 to 45 oz. Careful measurements extended over a period of eight weeks."

Other references to the subject-matters in this paper will be found as follows:—*Lancet*, 18th February, 1896, page 417, Salford Royal Hospital:

"Nephrectomy," under Dr. Herbert Lund. *British Medical Journal*, 17th October, 1896, page 1100, Dr. Oscar Bloch: "A case in which half the kidney, invaded by morbid growth, was removed; with remarks on the Conservative Surgery of the Kidney." *Lancet*, 31st October, 1896, page 1229: "Abdominal Nephrectomy," by F. C. Wallis, at the Clinical Society of London.

In further illustration of the cases forming this article, a statement of weekly averages of urinary water and urea excreted are here shown; also a chart for the same purpose.

The following conclusions appear to me to be justified:—

1. If one kidney be removed, then three years subsequently the healthy kidney *in situ* will excrete more than the average quantity of urinary water produced by two normal kidneys.

2. This same kidney will excrete per cubic centimetre of urine and per diem the average amount of urea that would be produced by two healthy kidneys.

3. If one-half of one kidney be removed, then two years afterwards the urine voided will be the same in quantity as if two healthy kidneys were present.

4. The loss of the half of one kidney will cause no increase or diminution in the percentage or total amount of urea excreted.

5. If one kidney be removed, then the quantity of urinary water excreted by the other during the seven weeks immediately succeeding the operation will be below the average daily amount.

6. The urea produced by this one kidney will be less than the average proportion usually seen as the result of the work of two healthy kidneys, but probably not below the average excreted by two healthy kidneys from an individual recovering from a serious operation.

From the standpoint of the practical surgeon there is one important fact brought prominently forward in Professor Bradford's experiments and lectures. They teach us: Firstly, that in the dog two-thirds of the total kidney weight can be removed with impunity, and the one-third remaining is consistent with the prolonged and healthy existence of the animal; and, secondly, that there is a limit between the one-third and the one-fourth of the total kidney weight beyond which, if excision be practised, the kidney tissue left is not sufficient for the sustaining of life, as the animal will rapidly emaciate and die; not, however, from uræmia, as even with one-tenth of the total kidney weight a hydruria and a polyuria immediately followed. Any one of us may have the question brought before him in a case of kidney disease or a tumour growth of renal tissue, as to how much of the kidney substance may be with safety excised. The general trend of all these observations is in the direction of Professor Rose Bradford's results—that one-third of the total kidney weight in the human being would be as capable of sustaining a prolonged and healthy life as he found to be the case in the dog.

For the preparation of the accompanying charts and the working out of the averages and totals, I am much indebted to Dr. F. H. Cox, a graduate of the Sydney University.

Dr. Scot Skirving said that he would have liked to have heard further concerning the diet and amount of liquid given to these patients, and also concerning the excretory power of the kidneys prior to operation. It was also a matter of interest whether a patient stood a nephrectomy best in whom the operation was undertaken for chronic disease or some immediate lesion, always granting that the kidney, not the seat of operation, was normal in either class of case. In two cases of nephrectomy in which he had been concerned, subsequent pregnancy ran an absolutely normal course. As to albuminuria following nephrectomy, he had had two cases in which the urine passed for several days subsequent to the operation was well-nigh solid with albumen; both recovered. Dr. Nash's paper certainly gave one a clearer view of the amount of diminution of renal tissue which was at once compatible not only with life, but also with the justifiability of operative interference on the diseased structures.

AN EPIDEMIC OF PTOMAINE POISONING—113 CASES.

By S. J. Richards, M.B. et Ch. M. Syd., Mount Morgan.

Isolated cases of poisoning by ptomaines are common enough with every practitioner, when vomiting and diarrhœa with consequent depression, attack members of a family which has partaken of some particular food. Perhaps tinned fish, especially herrings, is most frequently to blame; but in some cases brawn, German sausage, and potted meats are the evident causes. In these last cases a suspicion generally arises that the article in question has not been of the freshest description.

But cabbage is occasionally found to have been the cause of severe diarrhœa; and upon inquiry it will probably be found that the cook has been inexperienced and has kept the lid upon the pot whilst boiling. Meat pies which have been baked without leaving sufficient escape for the steam are also a frequent cause for diarrhœa; and in this connection I must particularise the small meat pie of the shops, known as the "Scotch Pie," in the centre of which there is no hole left for the escape of the contained steam. In these last cases the formation of the poisonous property seems to be dependent upon the fact of the article having been cooked under pressure. Cooks of experience invariably allow a free escape of steam from the cabbage pot, and leave a hole in the centre of the meat pie. There is reason, therefore, in their tradition. Curiously, however, the meat pudding (boiled under tightly enclosed paste) does not usually provoke anything more serious than an attack of indigestion. As yet it is unknown whether the poisonous property arises from the setting free of some product similar to a ptomaine by the process of cooking under the above conditions; or whether, maybe, the fact of cooking under high pressure destroys a substance in the cooked article which would have otherwise neutralised a toxic substance. Either explanation is feasible. But there is much room for experimental and analytical work in this field. Cases of poisoning by ice cream prepared under filthy and unsanitary conditions are fairly common in summer time; and some fatal cases of poisoning by this substance were traced by Vaughan to a filthy cellar where the vendor prepared his wares, and where the milk was proved to have been in a state of decomposition before the flavouring was added.

We owe the term "ptomaine" to Selmi, who gave this name to certain nitrogenous cadaveric alkaloids that he isolated, and which he found formed crystalline salts by the addition of acids. Those first isolated proved to be non-toxic; and to this class belong *neuridin*, *cadaverin*, *putrescin*, *saprin*, and *methylamin*. The toxic ptomaines include *muscarin*, derived from various fungi and putrefying fish, and *neurin*, which two exert a physiological action antagonistic to atropin; *tyrotoxicon*, common in poisonous milk, cheese, and ice cream; *typhotoxin*, to which the specific action of the *bacillus typhosus* is evidently due; and *tetanin*, which produces characteristic tetanic convulsions, and which is a product of the tetanus bacillus. Besides these alkaloids

Brieger and Fraenkel have given the name "toxalbumins" to the toxic substances derived from other pathogenic bacteria. Included amongst these are the toxins of the bacilli of diphtheria and of tuberculosis—the latter the well-known "tuberculin." Indeed, the whole of these bodies are evidently products of bacterial activity. The train of symptoms in "gastro-enteritis" of infants is probably due also to toxines of bacteria which have developed in that excellent culture medium for micro-organisms, milk.

On 10th April, 1898, I was called to attend a very large number of cases in which the symptoms were vomiting, diarrhœa, pallor, coldness of extremities, rigors, cramps in the legs, abdominal pains, tingling and numbness in hands and feet, and generally most alarming collapse. The calls began to come in about 1 p.m., and from then until about midnight I was occupied to the utmost of my ability in getting round to see people of all ages suffering in this way. My first call was to the house of a local chemist, and I found himself, his wife, and four children lying about in a most distressingly helpless condition. I ordered small doses of chlorodyne and free stimulation with brandy, and hurried to other houses, where a similar condition of affairs was found. By now comparing answers to my inquiries as to the nature and source of their food supplies, I was able to eliminate meat and bread; and found that all my patients had been supplied with milk from the same man. In the midst of my hurried visits, I was able to visit this man's dairy to stop his evening round; and found the premises quite clean, and cans, etc., beyond reproach. The dairyman and his wife were surprised to hear that most of their customers were so dangerously ill, and indignant at the suggestion that the milk had been the cause. The man himself declared that his family had drunk some of the same milk, and that it had produced no ill-effects. (The latter part of this statement was subsequently found to be untrue.) Later in my visits I discovered that some whole households supplied by this man were not affected at all, and I then remembered that my own family had that morning consumed some of this same milk, and that I had drunk at my breakfast two cupfuls of coffee made with half milk. Again, rumours were heard of vomiting and diarrhœa in houses supplied by other milkmen, but upon investigation these cases could be plainly traced to acute indigestion following the usual heavy dinner. (The day was Easter Sunday.) Finally, after sifting the matter thoroughly, I discovered that every member of each household supplied by this man was affected in like manner, with the exception of those cases in which the milk had been boiled previously. My first inquiries in this direction were misleading, as many of the patients assured me that they also always boiled the milk. Further inquiry, however, proved that they used the milk unboiled for breakfast, and boiled it after breakfast to ensure it keeping fresh through the day. One small boy employed as "boots" in a large household was very ill who was said never to touch milk. But he admitted, some time later, that he took a good drink out of the billycan before he took it into the house.

With regard to severity, the symptoms varied in intensity directly with the amount of milk consumed unboiled, and with the length of time between when the milkman set out upon his rounds and the hour when the milk was drunk. (The morning was warm, and the bacteria and ptomaine evidently increased rapidly whilst the milk was being distributed.) Also indirectly as the age of the patient. The most severe case was a young child, who was supplied late in the milkman's round, and who had drunk nearly half a cupful of milk. The least serious cases were those where only a small quantity of the milk had been put into the morning's cup of almost boiling tea, and those where the milk had been "scalded"—*i.e.*, heated on the fire—before using. Those who had thoroughly boiled the milk before using it were, like myself, not affected at all. There were no fatal cases, fortunately, although ten or twelve cases nearly terminated in this way, and did not actually recover for nearly a week. The poisonous effects began to be manifested within an hour and a-half after consumption, in most cases with vomiting.

The milk in every case was normal in appearance, odour, and taste. I secured about a pint of it that evening for analysis. Next morning it was curdled, and the following day I obtained the assistance of Mr. H. A. B. Leipner, the analytical chemist of the Mount Morgan Gold-Mining Company, in analysing a portion of the sample. The other portion was sent to Mr. Henderson, the Government Analyst in Brisbane, and he agreed with us that tyrotoxicon was present in considerable amount. Vaughan is said to have first isolated this ptomaine. It is very unstable, and becomes decomposed by a temperature of 90° Centigrade. We also found that it disappeared on keeping the milk for some days, when both analytical and physiological tests failed to trace it.

No cases of this kind have occurred here either before or since. The dairyman in question assisted me very greatly in collecting data, as soon as he found that his milk was positively at fault, and that he was not liable to any punishment. He strongly disclaimed having put any water in the milk, but thought that the pack-saddle upon which his cans were slung may have been to blame. I found that he had used the same pack-saddle to convey cans of house refuse from the hospital to his pigs.

TROPICAL DISEASE IN NORTHERN AUSTRALIA.

F. Goldsmith, M.B. (Adel.)
Port Darwin.

It is with considerable diffidence that I approach the subject of Tropical Diseases of Northern Australia, firstly because my short and limited experience has not been sufficient to enable me to speak with much authority on the subject; and, secondly, in the absence of any means of bacteriological investigation, that most important branch of this study must be to a great extent left out, and my remarks will of necessity be almost wholly confined to disease in its clinical aspect.

Taking the last two years' hospital records as a fair index of the presence of disease in the Northern Territory, I find that a little over 10 per cent. of the cases admitted are put down as malarial fever; but the type, viewed from a clinical standpoint, differs very materially from those described in text books, so that possibly the *plasmodium* differs from the ordinary type, or the comparative mildness of the symptoms may be due to the presence of the malarial germ in less concentrated form than occurs in other localities, the disease here being of a mild form, and the more virulent types, such as hœmoglobinuric fever, being apparently entirely absent.

Looking at malaria from a clinical point of view, we get, at various times, quotidian, tertian, and quartan varieties, which may be intermingled, and a peculiar characteristic in all is the comparative absence of ague-like attacks. The patient simply feels out of sorts while the febrile attack is on, and in the majority of cases it is not preceded by shivering. During the febrile attack the temperature rises from 101° to 104°, perspiration breaks out, and the temperature falls till the next attack. There is usually no headache at any stage, and the patient simply complains of "feeling tired" and weak.

With regard to the progress of the disease, there are two courses, which may be respectively called malarial febricula and typho-malaria. These two classes describe themselves. In the former, the patient recovers quickly, after two or three days' treatment, and he is left rather weak, and quickly pulls himself together without any further rise of temperature. I enclose two charts of this class, which are the severest cases I have had, yielding easily to treatment. In the majority of cases of malarial febricula, the temperature reaches normal by the third day and does not again rise.

With regard to the typho-malarial type, it is less common; and in two years' experience I have only come across three cases. It does not differ materially from the febricular type, but is more resistant to treatment, and runs a course of three or four weeks before the temperature remains normal. The patient does not appear to be any more liable to ague attacks than in the febricular form, but lies quietly as in typhoid fever, which disease it resembles to a remarkable extent, save that the temperature throughout tends to be of a remittent or intermittent type rather than continuous, and in one case the temperature came down by crisis accompanied by some degree of collapse, instead of a gradual fall, and did not rise again. In all cases of this class the spleen was enlarged. Of course, I bear in mind that these may have been cases of typhoid fever running an unusual course, but I do not think it likely, as they were isolated and came in at intervals, and, as far as I can gather, no other cases of a similar nature have occurred in the Territory. It may be that both types are due to the same *plasmodium*, but that the system has been more saturated with the malarial poison in the one type than in the other. On the next occasion of a person coming under my notice suffering from the typho-malarial type, I intend to get the blood examined and settle, once for all, whether it is typhoid fever following an unusual course or a distinct type of malarial fever. As all the patients recovered, the evidence that might have been observable by a *post-mortem* examination was not available. The patient who had the disease in the severest form told me that he had suffered from a very similar attack in New Guinea, which is rather in favour of it being malarial, as it is very unusual for enteric fever to twice attack the same person. I enclose a chart of one of the cases of this type; it will be observed that though the temperature dropped after small doses of antipyrin, it dropped just the same when that drug had been withheld; it will be also noticed that the bowels, during the height of the attack, were confined.

With regard to treatment, I find quinine mixture, in which a few drops of ac. hydrochlor. dil. are added, answers every purpose. I generally give about 10 or 12 gr. daily in this form. If the temperature is high, with dry skin, a small dose of antipyrin is generally useful to promote perspiration and reduce the temperature. In the two years I have been here I have not had a death occur from malaria. With regard to the localities from which the malaria patients come, it is remarkable that the buffalo hunters, who spend their time during the malarial (dry) season in the most mosquito-infected districts, seem to enjoy a practical immunity from malaria, while those living in mining townships, more especially alluvial districts, are the greatest sufferers. Is it due to the fact that the old theory that malaria is more common and more virulent where the soil has been recently turned, is correct? Or is it, that the *plasmodium*, requiring two hosts for its development and propagation—in the buffalo country the population is so scanty and water so plentiful—that the mosquitoes, though in myriads, do not produce sufficient *plasmodia* to contaminate the water enough to overcome the resisting power of the persons drinking it? And the buffalo hunters being robust, abstemious men, living a healthy outdoor life, their resisting power to disease would probably be greater than that of dwellers in townships.

Of filaria I have only seen two cases while here; one an aboriginal woman, with elephantiasis of the vulva, which I amputated, and from which she made a good recovery. The other was a case of chyluria, occurring in a Japanese man. It was a severe case, and the milk-coloured urine would coagulate as soon as passed, and sometimes even in the bladder, when a bougie would have to be passed and the clot broken up. I treated him with diuretics to dilute the urine as much as possible, and encouraged him to drink plenty of fluids. I note that the urine passed during the day was milky, and that passed during the night was normal in colour, though the tendency to form coagula was present in each. This case had a peculiar termination. He got gradually thinner and weaker, and seemed about to die of inanition. Although he was ravenously hungry, his food did him no good, as it was all passed as chyle before reaching the circulation. At last, when apparently on the point of death from starvation, his friends applied for permission to remove him. I gave them the required permission, and told them that he would probably die in the course of a few days. They took him away in a springless cart over a rough road home, and from that time his urine was normal, and he rapidly regained health and strength. The jolting probably displaced the thrombus in the thoracic duct, the varix in the bladder healed up, and the chyle followed its normal direction. This case opens up a point in treatment of similar cases as to whether it might not be permissible and reasonable to try rough jolting in apparently incurable cases, with the object of displacing the thrombus blocking the thoracic duct.

With regard to tropical abcess of the liver, P. Manson says in his manual that Australia seems to enjoy a practical immunity. I have, however, had two cases in which there is no doubt that the disease was contracted in Tropical Australia ; they both had lived in the Northern Territory for several years, both had had attacks of malaria, and both were heavy drinkers. Both cases were fatal. In one the abscess was in the right lobe, and burst into the colon, and the other had several abscesses, mostly situated in the left lobe. In each case the diagnosis was made during life, but as no pus was discovered on aspiration I did not feel justified in proceeding to operation. In neither case could I elicit any history of previous dysenteric attack.

There is a comparatively little-described disease occurs here which is mentioned by Dr. Manson in his manual under the heading of "Ulcerating Granuloma of Pudenda." It is characterised by extensive ulceration in the pudenda and groins of both sexes. I have seen it in Europeans, Asiatics, and aboriginals, and it is contagious to a certain extent, probably only through some breach of the surface, and transmitted during the sexual act. It does not appear to be highly contagious, and attacks skin, mucous membrane, or glands ; though the latter only secondarily. It is distinct from syphilis in that it does not yield to syphilitic treatment, and is apparently a distinct disease in itself of venereal origin. A few notes of mine on the subject appeared in the *Journal of Tropical Medicine* for January, 1899, and I note that other observers agree with me in deciding that it is venereal in origin, though not syphilitic, and that it is contagious.

Beri-beri has appeared on two or three occasions among the prisoners confined in gaol, mostly among the Chinese. Medical treatment alone does not appear to do much good, though I generally give digitalis and pot. acetate mixture. I place more faith on a more liberal diet with fresh meat, green vegetables, and vinegar, to effect a cure. The Chinese themselves place great faith in vinegar, and invariably ask for it when suffering from this disease, and I have had very good results from the above treatment.

Leprosy occurs here, generally of the anæsthetic type, in isolated cases among the Chinese. There is a tribe of aborigines living on the Alligator River, where the disease appears to be endemic, nearly all the members of this particular tribe being more or less attacked. It is not known how the disease first came there, though there is a theory that it may have been originally introduced by the Malays who pay annual visits in proas from the Macassar Straits, and have intercourse with the blacks. The natives of this tribe subsist largely on fish ; but, then, so do other tribes which are not attacked ; neither does the disease appear to spread to natives of other tribes with which the Alligator natives have intercourse. The type of leprosy among the aborigines partakes both of the anæsthetic and tubercular variety.

DISEASE.

Malaria

Notes of Case.

Name { D — C. —

Age 30 (?)

Diet

Case Book No.

Ward A

Treatment.

Quin' Sulph gr IV
Ac HCl dil ♏V
Aq ad ℥i

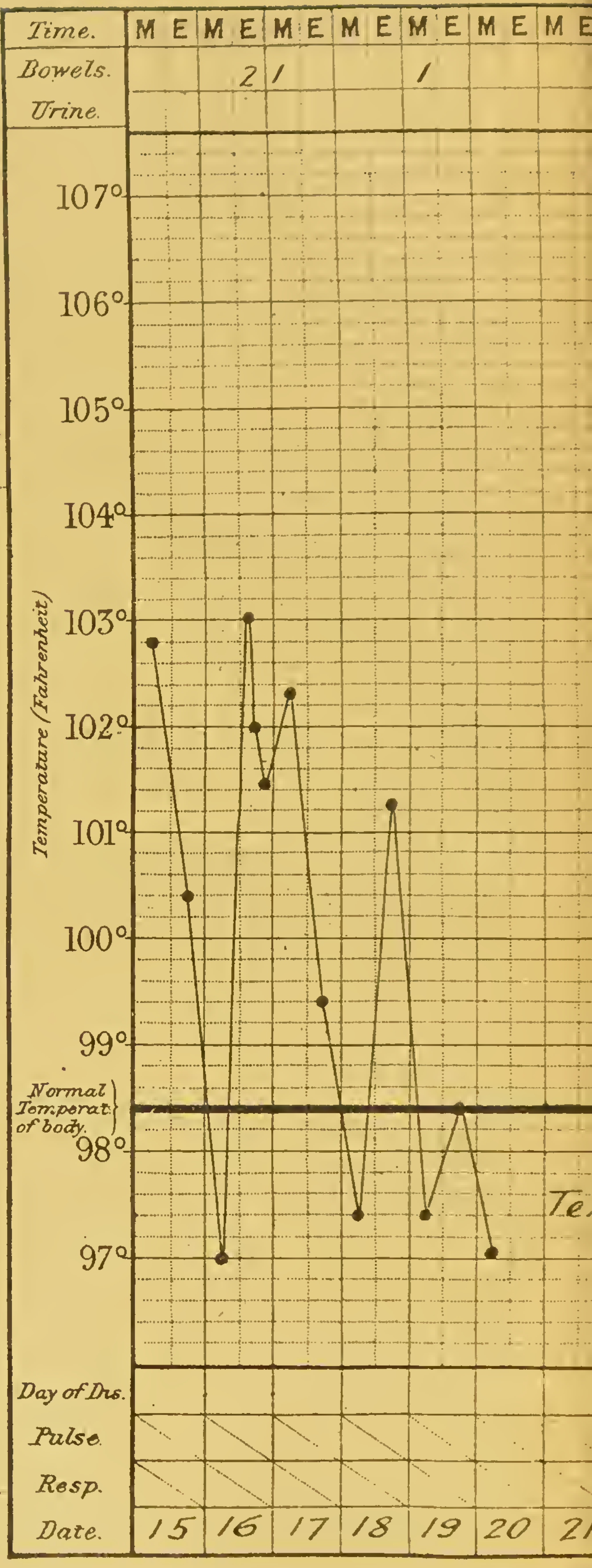
Antipyrin gr V
to Aq ℥i

when Temp. above 102°

Felt out of sorts for about a week before admission headache and general malaise.

Date of admission.
15 — 5 —

Date of discharge.
30 — 5 —



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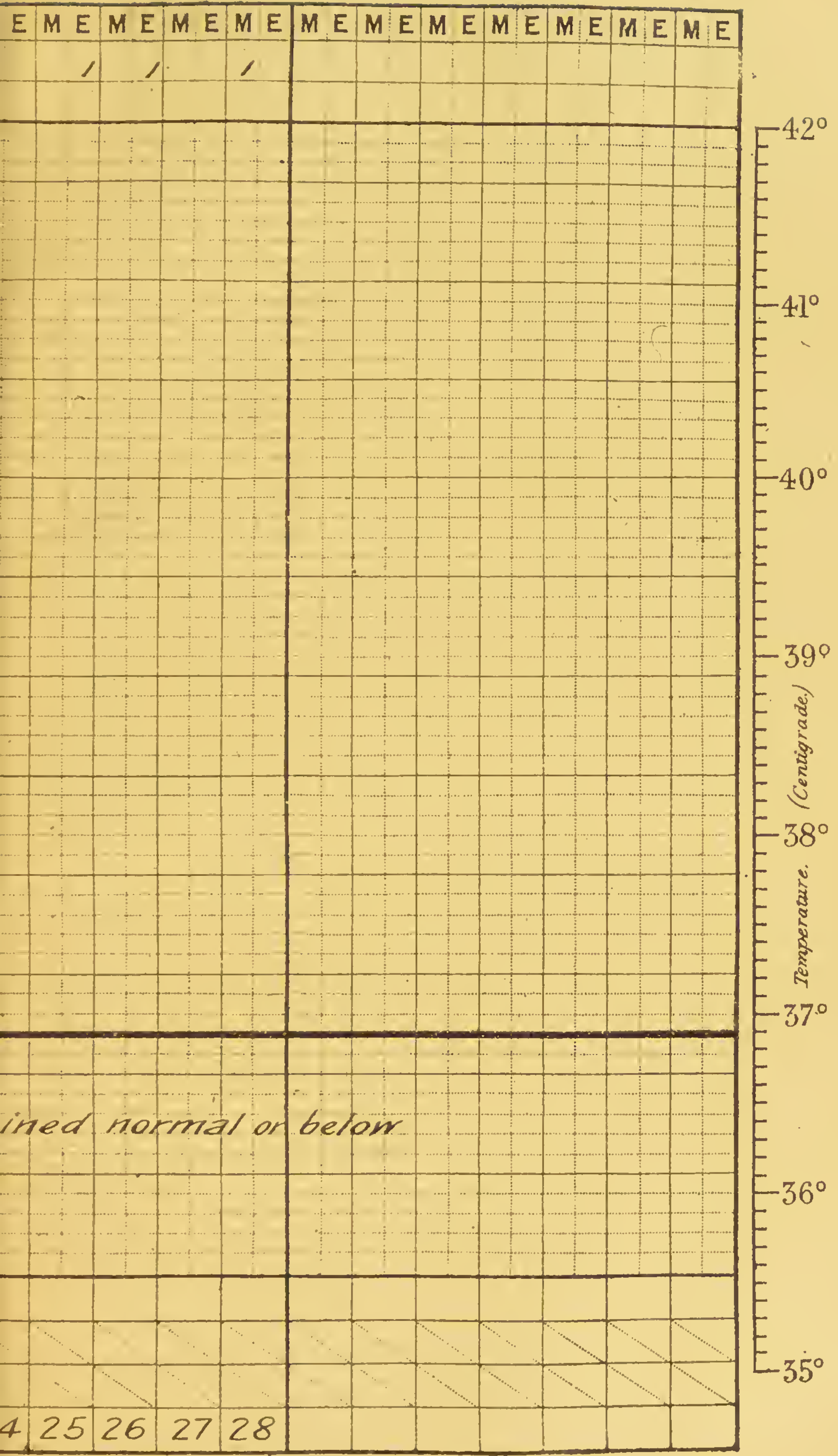


oon & C°, 7 Serle Street, Lincoln's Inn

Gould's Clinical Chart.

DISEASE.

Malaria

Notes of Case.

Name O – K –

Age 23

Diet

Case Book No.

Ward A

Treatment

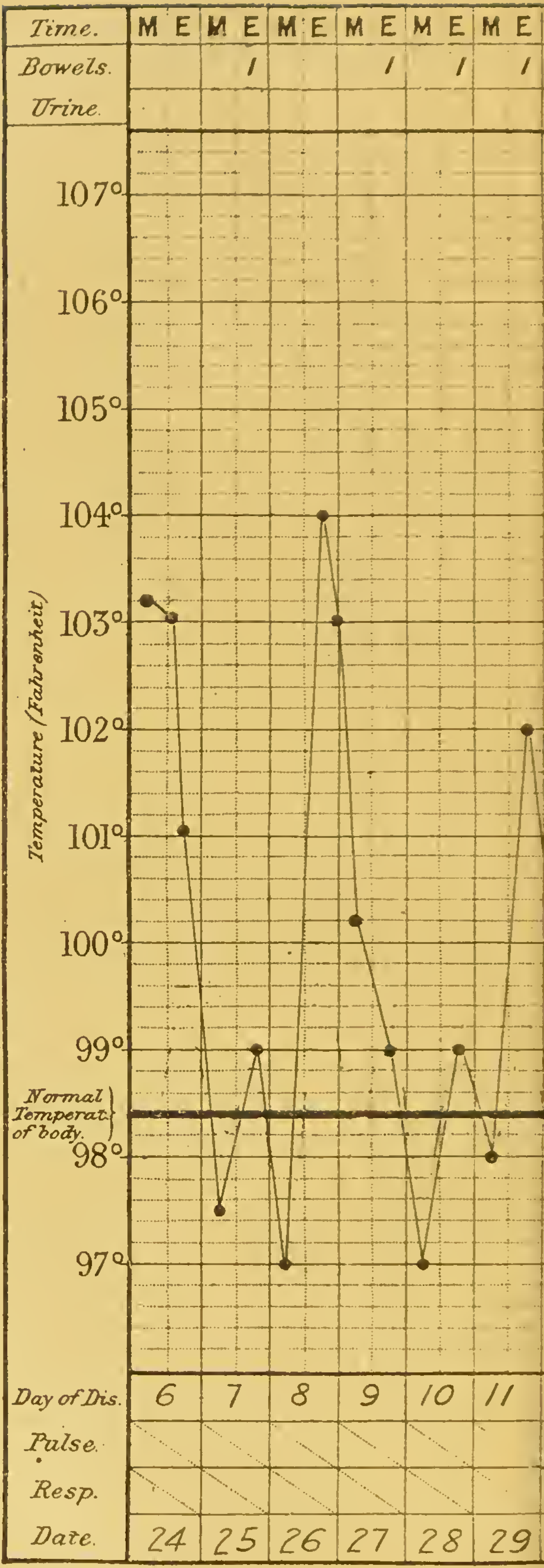
℞ Quin. Sulph gr IV
Ac HCl dil ♏ V
Aq ad ℥I

Antipyrin gr V
when Temp. above 102°

Ill 5 days before admission
no shivering fits no pain
only feels weak & out of sorts

Date of admission.
24 – 7 – 99

Date of discharge.
6 – 8 – 99



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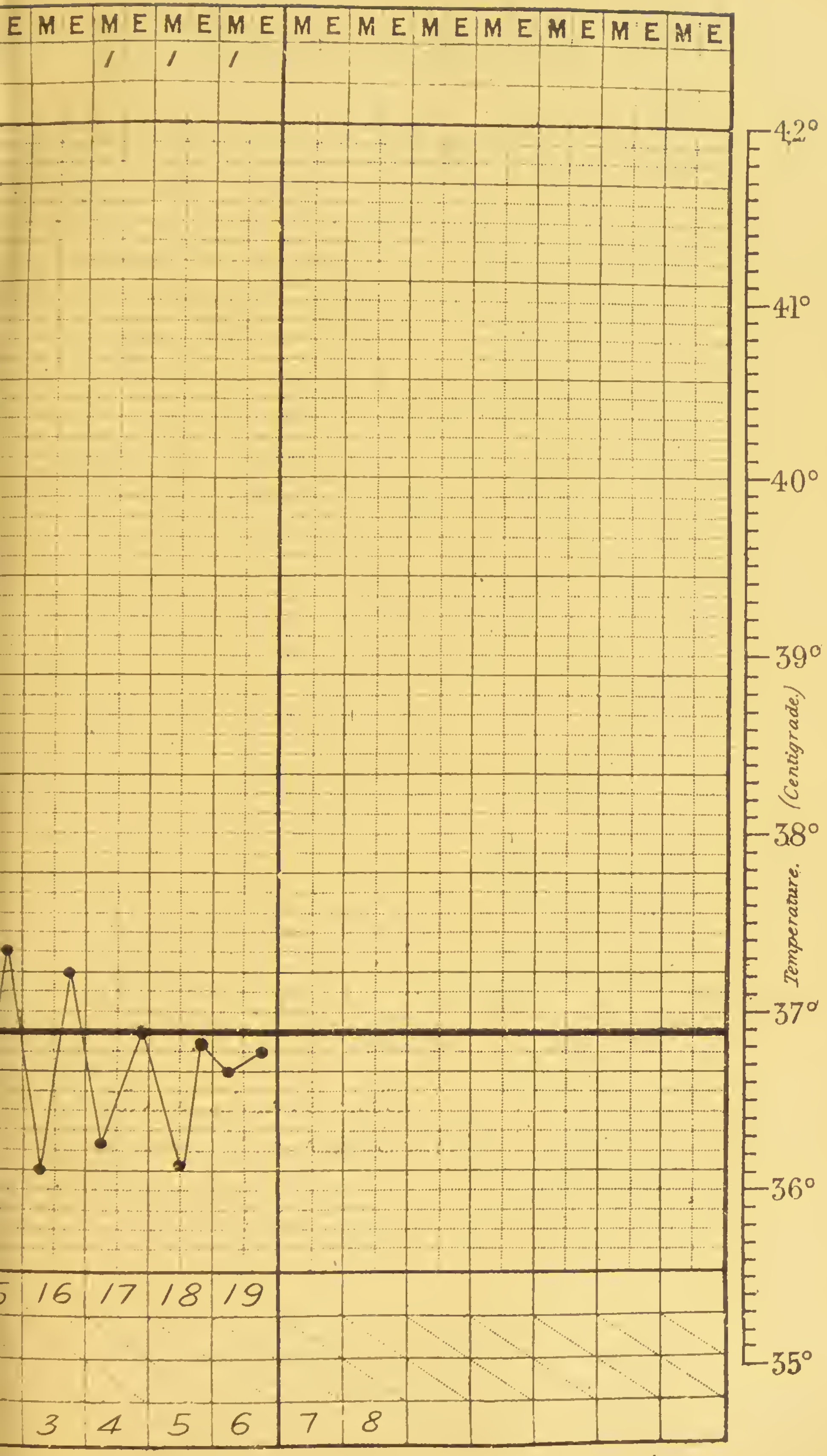


oon & C°. 7 Serle Street Lincoln's Inn

Gould's Clinical Chart

DISEASE.

Typho-malaria

Notes of Case.

Name { H — W —

Age 26

Diet

Case Book No.

Treatment

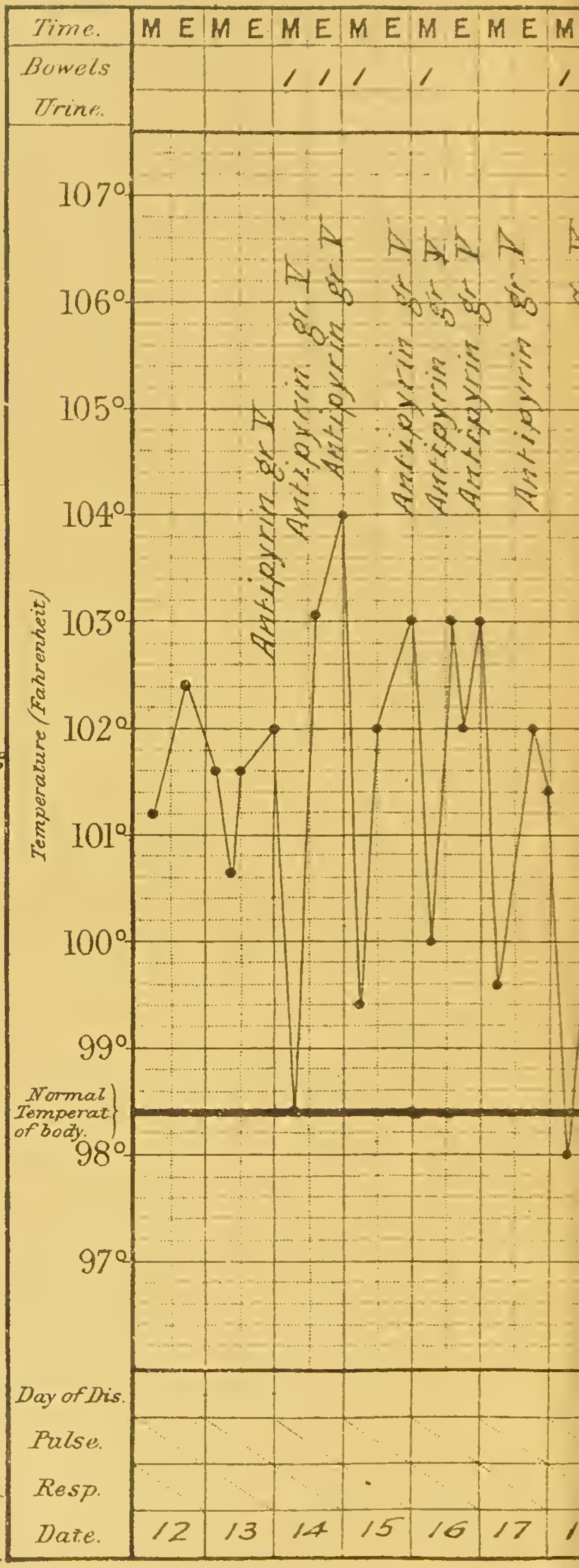
Hst Amm Acet
to 27-2-98
then Hst Quinine

Ill about 4 weeks no shivering general malaise, headache, loss of appetite, etc.

Spots on abdomen, roseola about 20 in number.

Ward A

Date of admission.
12 — 2 — 98
Date of discharge.
8 — 4 — 98



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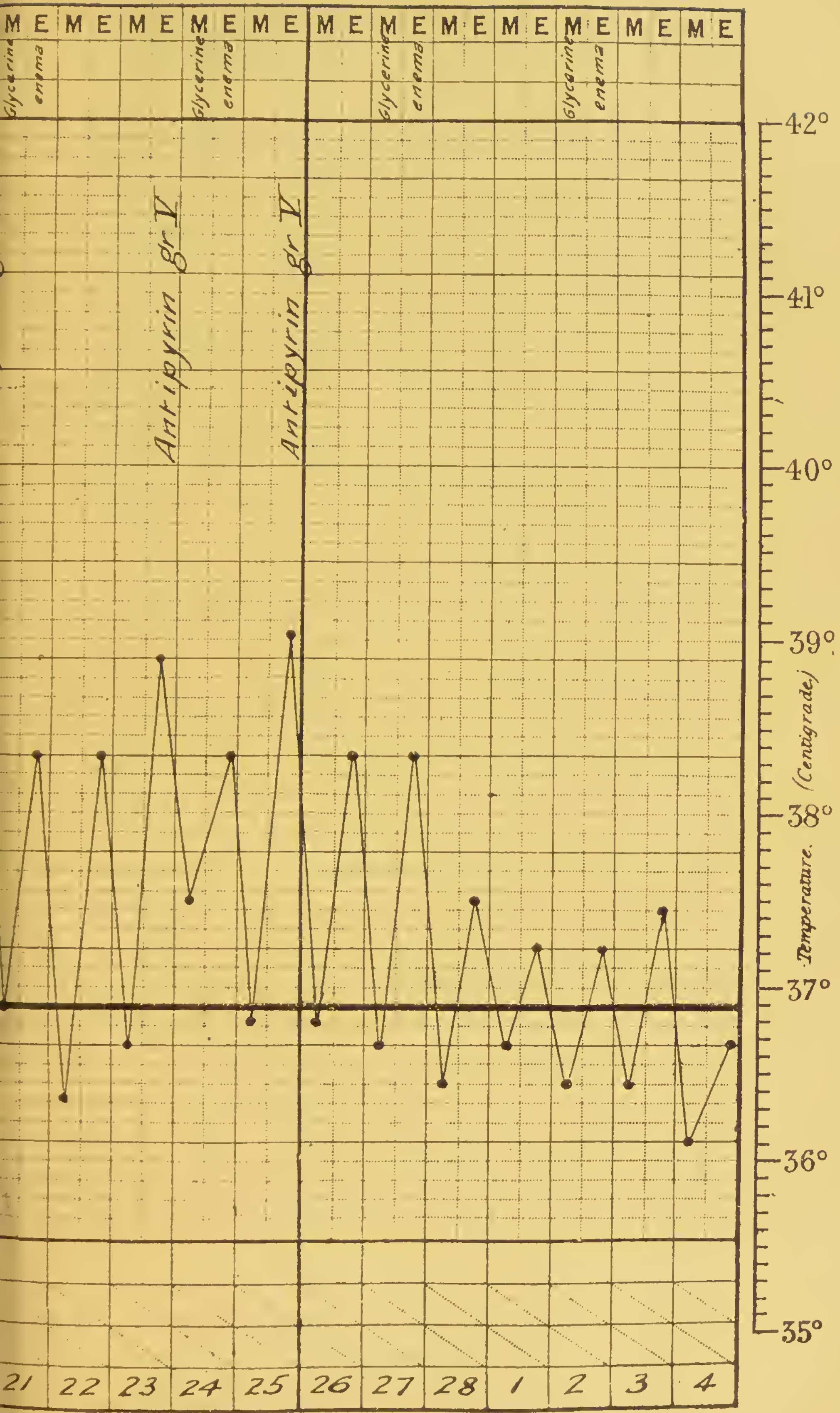

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Gould's Clinical Chart.

DISEASE.

Typho-
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Notes of Case.

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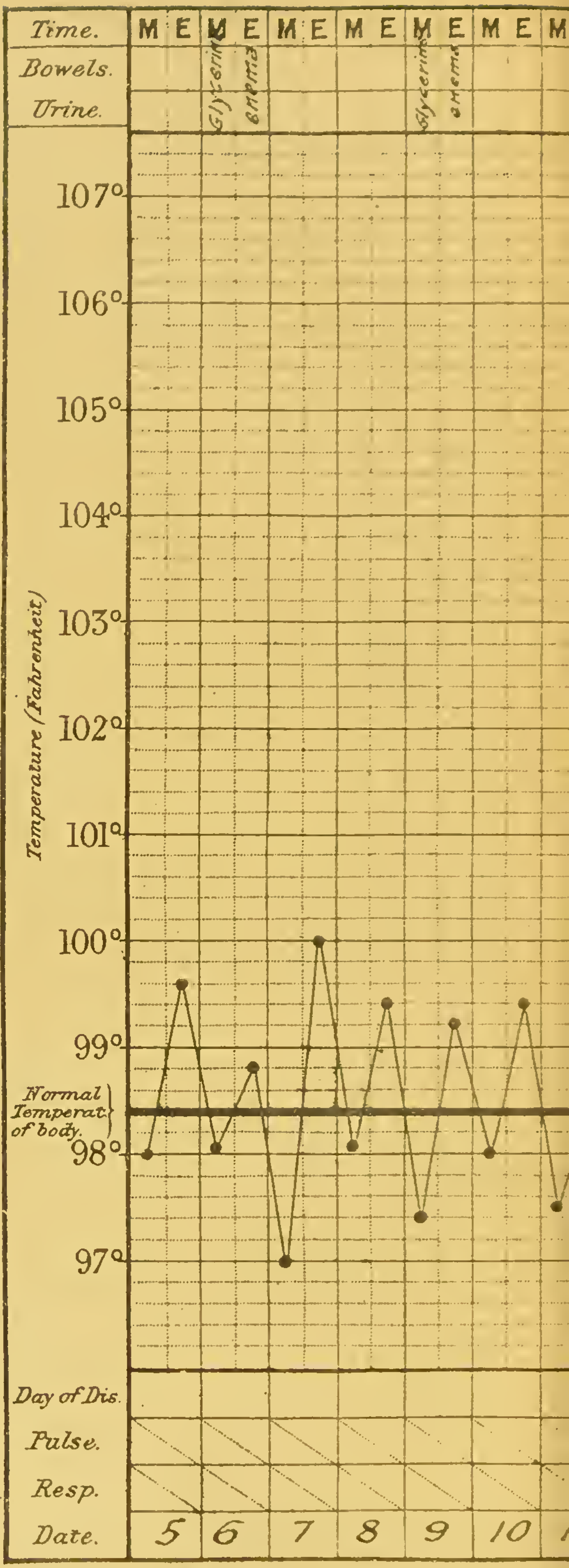

Date of admission.
12 : 2 : 98
Date of discharge.
28 · 4 · 98

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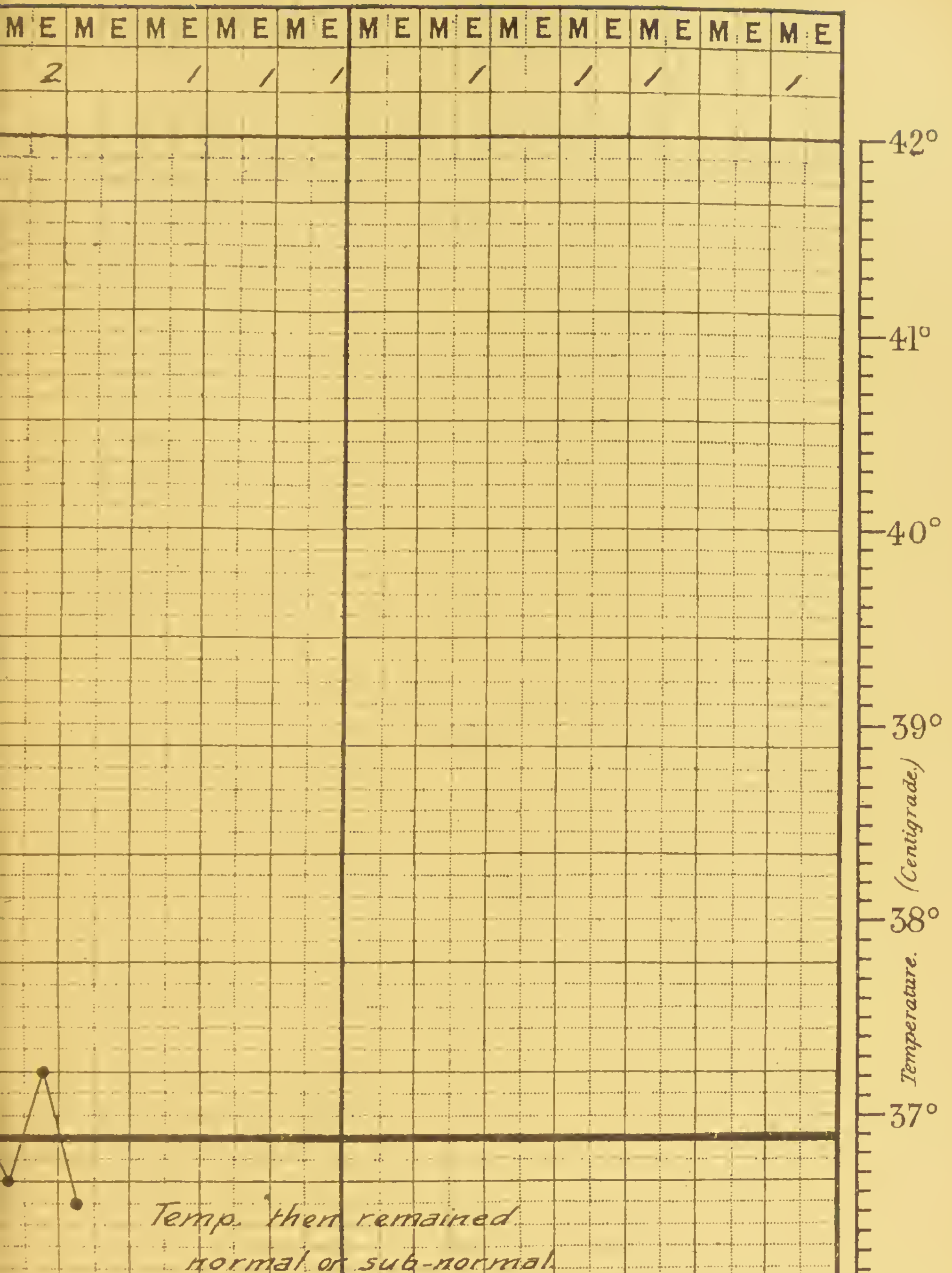

rspoon & C°. 7 Serle Street. Lincoln's Inn   Gould's Clinical Chart

THE INFLUENCE OF HÆMORRHAGE ON THE COURSE OF PULMONARY CONSUMPTION.

By Eugen Hirschfeld, M.D. (Strasburg).
Brisbane.

Every medical man who has seen the disease, both at home and in the Australian colonies, is aware that the course of pulmonary consumption is considerably modified by the altered climatic conditions which prevail in Australia. Theoretical considerations tell us that the influence of a hot climate upon the course of any infectious disease may be exercised in several directions. First, the conditions of existence for the micro-organism outside the body are altered by the increased temperature which sometimes approximates to blood heat, by the intensity of insolation, by the alternation of dry and wet seasons, &c. In the second place, the predisposition of the individual must differ as he is becoming acclimatised. Thirdly, the hot climate will directly influence the course of the disease when once it has attacked the individual. Our knowledge of disease, as it has been built up by centuries of research of European investigators, must, therefore, be revised to a certain extent by our specific Australian experience, the result of which will be either corroboration or modification of what we are already acquainted with. I shall, therefore, need no excuse in dealing with this subject from the standpoint of a purely Australian experience.

This paper is based exclusively on personal observations in eighty-six cases of hæmorrhage which occurred in the course of pulmonary consumption. Fifty-six of these patients were males and thirty females. This proportion does not imply that the occurrence of hæmoptysis is more frequent amongst men than women, as the number of phthisical men was greater than of women in the proportion of 115 males and 63 females. This increased prevalence of phthisis amongst men is due to purely local conditions, which we need not refer to upon this occasion. In order to study the influence of hæmoptysis, it is necessary to divide our cases into two classes—(1) those with incipient phthisis, and (2) those in which the disease has led to extensive formation of cavities. In a certain number of our cases, which were all taken from private practice, it was possible to obtain accurate observations through the patients themselves and their relatives. It was explained to them how important the knowledge of an accurate history of their complaint was for its treatment, and they readily complied to keep a record of their temperature and pulse and other symptoms, sometimes extending over months. Hourly observations of their temperature were taken by a few enthusiastic patients. The result of these observations was of considerable importance, principally in connection with the occurrence of catarrhal pneumonia after hæmoptysis.

INCIPIENT PHTHISIS.

A large proportion of our Australian patients suffering from incipient phthisis recover without ever having brought up blood, while, on the other hand, it is very rare to see a patient pass into chronic pulmonary consumption who does not expectorate blood at one time or the other. In connection with this comparative rarity of hæmoptysis in incipient phthisis, I should like to mention that bleeding from the respiratory tract on the whole is less frequent in Queensland than at home. In over 500 cases of typhoid fever, epistaxis occurred during the incubation of the fever in three instances, while it is a not infrequent symptom of European typhoid. A possible error in the diagnosis of the source of the blood may occur through the presence of adenoid vegetations, especially when the latter are coexisting with apparent lung mischief. The easily bleeding mass of vegetations is often responsible in children for blood mixed with mucus being brought up in the morning. Still in these cases the correct diagnosis will be easily arrived at from other typical symptoms. But the following case presented some difficulties: A tall, overgrown lad, about twenty years of age, with a distinct lowered dulness of the left apex, had been suffering from dry, hard cough, with an expectoration of blood in his phlegm. Naturally the presence of blood was attributed to the lung mischief. Although the quantity of blood lost was small, all the usual remedies—even continued rest in bed—were of no avail, until finally attention to the adenoid vegetations in the naso-pharynx stopped tho apparent hæmoptysis. In this case mucus and blood had been loosened by the cough, and were brought up with it, thus creating the impression that they came from the lungs. The *prognosis* of hæmorrhage *quoad vitam* in early phthisis is, under proper treatment, almost always favourable, even though the loss of blood be very large. I can only record one fatal case. A. P, sixty-five years of age, tanner, living about 12 miles out of Brisbane, consulted me on 6th November, 1895. He had been coughing for the last twelve months, had been suffering from night sweats, and had lost altogether 3 stone. Thorax was barrel-shaped, and lungs apparently normal, with the exception of intensified inspiration, especially towards the end. Height, 5 ft. 7 in.; weight, 8 st 7 lb.; pulse, 94, with an occasional intermission, full and hard; temperature, 98·4° (11 a.m) under the arm. On 12th November he had increased considerably. Weight, 8 st. 11½ lb., a gain of 4½ lb., and the cough had lessened. On 15th November I was informed by the son of the patient that his father had died quite suddenly within two minutes through blood rushing from his mouth as if "thrown out by a pump," as he described it. This was evidently a case of apoplexa pulmonum, for which probably senile arterio-sclerotic alterations in the wall of the bloodvessel were, to a certain extent, responsible. Apart from this isolated case, recovery took place in every other instance, although in some cases, the mass of blood thrown up, not only on one but on repeated occasions, appeared to be extremely alarming, so much so that in one instance the relatives sent for the priest before they sent for the doctor. We are, therefore, generally able to give a favourable prognosis to the patient and his relatives, which often in itself contributes towards the cessation of the bleeding by relieving the mind of the patient from undue fear and excitement. Only in the phthisis of old people it is advisable to speak more cautiously, having regard to possible arterio-sclerotic changes.

The immediate fear of death being removed, we come now to consider the subsequent influence of the hæmorrhage—(*a*) upon the lungs, (*b*) upon the constitution of the patient. It goes without saying that if the quantity of blood is but small, or if the latter comes up mixed with the phlegm, the hæmoptysis can affect the condition of the lungs but little, except that frequently the loss of a little blood precedes a large hæmorrhage. Sometimes a patient declares that he feels better after having brought up some blood, and it is quite possible that inflammatory symptoms are relieved through the loss of blood; generally, though, the appearance of blood in the expectoration produces a wholesome fear.

A patient who has had a pulmonary hæmorrhage of considerable extent usually shows a subnormal temperature immediately afterwards. On the second or third day the temperature begins to rise, and the pulse rate increases in frequency correspondingly. While tho rise of temperature was constant, whenever such observations were made, it rarely exceeded 101° F.; neither did it continue over more than two to four or six days. The significance of this feverish disturbance may be interpreted in different ways. We are aware that the decomposition and absorption of blood (even without bacteria) can cause a rise of temperature. Still the small quantity of blood retained in or aspirated by the bronchi—it is only in advanced phthisis with extensive cavity formation that a large quantity of blood may be retained in the lung—could by itself not be responsiblo for tho fever; especially when we remember that if only sufficient blood were retained to fill a larger bronchus, other symptoms would at once denote such an occurrence. The most feasible explanation of the rise of temperature is, that the small quantity of blood retained is sufficient to set up an irritation of the alveoli and finest ramifications of the bronchi, with the result that slight broncho-pneumonic processes take place. If the patient rises too soon the temperature remains above normal for a longer period. Physical examination does not reveal anything characteristic, as the crackling râles which can be heard after a hæmorrhage may be caused by the retained blood or by broncho-pneumonic symptoms. But I should like to draw attention to the fact that in several instances in which the patient gave a history of a former hæmoptysis the lung symptoms were principally localised at the base instead of the apex. This seems to point to a broncho-pneumonic process due to aspirated blood. This question—whether catarrhal pneumonia follows in the train of pulmonary hæmorrhage—is of considerable practical importance principally with regard to our therapeutic action. Acting upon this theory, I have entirely given up the routine administration of morphia, which is generally recommended, especially as it is possible to control the cough by other means. We must also remember that the recumbent position combined with the suddenly lowered resisting power of the organism are particularly liable to favour the development of catarrhal hypostatic pneumonia.

In early phthisis it is the rule for these broncho-pneumonic conditions to clear up entirely under suitable treatment. If proper attention is paid to the general condition of the patient, we should be able to prevent in most cases these broncho-pneumonia foci from becoming the seat of extended tubercular disease. But we must always bear in mind that such is possible, and likely, if free expectoration is not promoted as long as crackling râles can be heard over patches which had been healthy prior to the hæmorrhage, and, secondly, if the patient issues from the attack of bleeding with his constitution considerably impaired. This leads us to the discussion of the influence of hæmorrhage upon the general health of the consumptive patient.

It may perhaps appear paradoxical, but I have had the experience time after time that an attack of hæmorrhage is the best thing that can happen to a patient in the early stages of pulmonary consumption, provided it is properly managed, and I believe that just in connection with this side of the question our Australian experience differs considerably from that of European medical men. In the first instance, it is surprising how readily even the loss of a large amount of blood is made up in our warm climate, as long as the original disease is not in an advanced stage. I have made a fairly large number of enumerations of red and white blood corpuscles in healthy people and in disease. The average number of red blood corpuscles per cub. mm. of healthy women ranges between 4,200,000 and 4,800,000; it is comparatively rare to find over 5,000,000 in people who have been born in Australia or who have resided for a considerable period in Queensland. The following case is, therefore, of some interest:—E. von B., twenty-six years of age, had three children, noticed first cough about seven years ago, since when it disappeared and reappeared on and off; occasional night sweats; râles over the left apex. Twelve weeks ago the patient had a severe flooding in consequence of miscarriage, which left her weak. Blood on being examined (200 squares were counted) showed 5,057,000 red blood corpuscles, which is more than the average of healthy women. The explanation of this ready repair of lost blood seems to me to lie in the following: The balance of bodily heat is maintained in a warm climate by lessened production of heat as the dissipation of heat is naturally diminished in consequence of the high outside temperature; consequently Australians require a smaller number of red blood corpuscles corresponding to the lessened production of heat. We therefore find an average of, roughly, 500,000 less red blood corpuscles in this country amongst healthy people. If now, through a loss of blood, the blood-making glands are stimulated into increased activity, a smaller portion of the produced heat is required to make good the smaller loss of heat in the hot climate, and a greater portion remains for apposition in consequence. But in order to make good the loss of blood and assist the blood-making glands, it is necessary that the utmost attention should be paid by the medical attendant to the general nutrition of the patient immediately after hæmorrhage has taken place. It is possible, with a loss of over 2 quarts of blood within a week, to so nourish the patient that he rises from his bed heavier than before the hæmoptysis. In such cases I rely entirely on milk. The patient receives in the first few days nothing but 6 oz. of milk every half-hour while he is awake. The total quantity of milk thus administered in twenty-four hours is over 3 quarts, and is well borne by the patient. The great point is to administer it in small quantities and at stated intervals. It gives the patient something to think about and something to do when he has to watch for the time for taking his food. We all know how difficult it is to enforce directions about diet being carried out accurately by the ordinary patient and his relatives. If, on the other hand, we make him take his food as he takes his medicine, the instructions are carried out by him in a more exact manner. The attention to the general nutrition is, of course, a matter of paramount importance in the treatment of the ordinary phthisical patient; it is the very A B C of the therapy of phthisis, but what I want to emphasise is this: It depends upon the management of the nutrition of the patient immediately after his hæmorrhage whether he will come out of his attack weakened in his constitution, less able to resist the extension of the tubercular disease, or, not only improved but often even cured.

The main reason, however, why hæmoptysis frequently proves a blessing to the consumptive in his early stage is its influence upon his mind and character. We all know how much the success of the treatment of consumption depends upon the patient himself in his co-operation with his medical attendant. A consumptive who is a fool has got no show; he is certain to go to the bad. But it is sometimes difficult to impress a patient, with an apical catarrh and tubercle bacilli in the sputum, with the necessity of foregoing a good many pleasures in life and of making sacrifices which, perhaps, his present fair general health does not seem to warrant and which *are* necessary for his cure. The sight of blood, however, alarms even the most careless man; though of infinitely less importance than the presence of tubercle bacilli in the expectoration, it proves to him a warning which he is not likely to forget. He is prepared to subordinate every other consideration to his desire of getting better. You cannot convince the ordinary consumptive in his first stage with a fair nutrition that he is suffering from the same disease as the poor emaciated beggar in the third stage, with his hectic flush, his breathlessness, and almost constant cough. The hæmorrhage furnishes the concrete proof. He becomes amenable to discipline, and the time he has to spend in enforced rest gives him leisure to become acquainted with the principles of diet and general line of conduct which have to guide him if he wants to get cured and remain so.

ADVANCED PHTHISIS.

From being an almost harmless occurrence in incipient phthisis, pulmonary hæmorrhage assumes a much more serious aspect as the original disease advances. As soon as it has come to the formation of cavities sufficiently large to be ascertained by physical examination, bleeding from the lungs becomes a grave symptom.

1. *Prognosis quoad vitam.*—Even in advanced cases of phthisis the immediate danger to life from the loss of blood is comparatively small. I saw two patients die from it. One was a woman of thirty-four, with a small-sized cavern. She had been in comparatively good health, looking after her housework prior to the attack. She died within a short time from internal hæmorrhage, very little blood coming up. The rush of blood evidently suffocated her. The other case was a boy of twenty-five, who also had been in fairly good health, looking after his work (clerk), when the hæmorrhage supervened. He was ill for nearly a week before he succumbed from several repetitions of the attack. Finally he died through both his lungs being flooded with the blood. In the light of subsequent

events. I had the impression that it might perhaps have been possible to save him if one had had the courage to make a venesection between the different attacks, as the high-bounding pulse which preceded a renewal of the bleeding seemed to point to depletion. But I repeat on the whole, even where the cavity is very large, the chance of the patient dying from the immediate effect of hæmorrhage is small, under suitable treatment.

The effect of the hæmorrhage upon the *lungs* is the immediate establishment of broncho-pneumonic conditions, beginning with a rise of temperature on the second or third day. While in early phthisis the pneumonic symptoms, as a rule, are but transitory, the ultimate result is vastly different as the stage of the disease becomes more advanced. In the first place, we see patients in whom the infiltration rapidly extends over the whole of one lung with absolute dulness on percussion. It is the exception that such a case improves again. Generally, the patient goes rapidly to the bad with the symptoms of pneumonic phthisis. In a second category of cases, portions of lung, which, prior to the attack, have been normal, show râles which persist for a considerable time. If the general health of the patient is still fairly good, or can be influenced favourably by the treatment, these patches may clear up, and the lost ground thus be recovered. More frequently, however, the extension of the tubercular process proves to be a permanent one. Thirdly—and that is in the minority of cases—the broncho-pneumonia, as in early phthisis, passes away without having damaged the lungs to any extent. And here, again, I may be permitted to draw attention to the danger of morphia, which, indeed, is even more injurious in the bleeding of advanced cases than of incipient phthisis. The risk of making the pneumonic conditions permanent through suppressing expectoration becomes more imminent the smaller the strength of the patient. Which of the three events is going to result from the hæmorrhage depends upon the general condition of the patient, the amount of blood lost, and the state of his lungs.

Amongst the after effects of pulmonary hæmorrhage the occurrence of miliary tuberculosis, more particularly of tubercular meningitis, has been observed by us repeatedly. It is of great importance to remember its possible occurrence, as the onset of the meningitis after the bleeding is very insidious, and frequently overlooked until it is fully established. The patient appears to become absent-minded, and, while formerly taking a deep, hopeful interest in his case (as most consumptives are in the habit of doing), and inquiring at each visit whether he has been improving or losing ground, he gradually becomes more impassive and does not trouble to ask any questions. The dulness increases; the patient dozes a good deal, waking frequently out of his sleep. So far, the symptoms may be attributed to the inanition of the brain through the loss of blood, or to the effects of the opium preparation he has been taking in most cases. Soon, however, any hope or doubt which one may have nourished disappears. From all sides the symptoms begin to close in; the well-known typical signs of meningitis develope, and the patient generally dies before the stupor has become complete. An early diagnosis is important, as its comparatively frequent occurrence after hæmorrhage is not noted by other observers; and, unless the appearance of this complication is kept in view, the relatives of the patient (from the similarity of initial symptoms) will become fully persuaded that he got too much morphia, which, of course, has nothing to do with the causation of meningitis. The most frequent localisations of the tubercle, after the meninges, were the larynx, then the bowels. The effect of the pulmonary hæmorrhage upon the constitution depends upon the amount of blood lost. As soon as the cavity has attained a fair size, the patient is generally unable to retrieve the loss of large quantities of blood. But too often, even though getting over his pneumonic trouble, he is unable to recover the lost ground, and becomes a complete invalid. The position, of course, is much less serious if the loss of blood has been smaller. Very slight hæmoptysis, even in advanced phthisis, sometimes exerts a favourable influence.

THE TREATMENT OF DIPHTHERIA.

A. Jefferis Turner, M.D. Lond.,
Brisbane.

In the year 1891 a new era in the treatment of diphtheria was introduced by a remarkable series of investigations published by Professor Behring, now of Marburg University. Although, as Behring remarks, all the essential points in the antitoxin treatment of diphtheria had been by that time fully worked out in the laboratory, the subject did not at first attract very much attention. It was not until 1894 when the acquisition of serum from large animals, particularly horses, permitted the extension of the methods discovered in the pathological laboratory to the treatment of human beings, that general interest was aroused. The treatment rapidly spread, and by the commencement of 1895 was on its trial throughout the whole civilised world. In the five years that have since elapsed the diminution in the hospital mortality of diphtheria has been nothing short of marvellous to all but the few who had made themselves acquainted with the thorough and unimpeachable experimental basis on which the new treatment has been established. The records of hospital experience have, in this instance, been in complete accord with the conclusions of the laboratory. In private practice also, in Australia at least, antitoxin has been very widely used, with very gratifying results, in the reduction of the mortality from this disease. Nevertheless, its use is not yet universal, and it is every now and then our sad experience to see children admitted into hospital dying from diphtheria, to whom an injection of serum has never been administered, although they have been under medical treatment. In England this appears to be the case to a larger extent. One reads with astonishment* that the reduction of the diphtheria mortality in London is almost entirely confined to cases treated in hospital, and that it appears very doubtful whether antitoxin is much used in general practice. Though the contrary statement is true here, I believe that the remedy, even by practitioners of repute, is not always administered in sufficient doses. In mild and early cases a small dose of 600 to 1,000 units is sufficient, but in advanced and desperate conditions many practitioners are satisfied with injecting 1,000 to 1,500 units, when 4,000, 6,000, or even 8,000 or more units are really needed. The treatment of diphtheria appears to be, therefore, still a proper subject for discussion, and I shall endeavour in the first place to lay before you, as briefly as possible, the *facts* on which my opinions are based.

The records of the Brisbane Children's Hospital have been very carefully kept for the past ten years, and the majority of the diphtheria cases during that time have come under my personal observation. I am, therefore, in a position to guarantee the similarity and continuity of the statistical material.

During the pre-antitoxin period of five and a-half years, the diagnosis was based solely on clinical observation. Doubtless a few cases not really diphtheritic were included, and these would make the mortality appear lower than it really was. I do not think they made much difference in the totals.

The antitoxin period is divided into five sections, of which the results of the two first have already been published.† In them we have included no cases but such as were verified by bacteriological examination. The new treatment being on its trial, such a course seemed advisable. During the next two sections we have included only cases which were verified bacteriologically, or which, on clinical grounds, could be recognised as diphtheria beyond any reasonable doubt.

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* Goodall, *British Medical Journal*, 1899, vol. 1, p. 197.

† *Intercolonial Medical Journal*, October, 1896. The figures given of the pre-antitoxin period contain a few unimportant errors which are here corrected.

In the last period we have had to rely on clinical evidence alone,* but this has been interpreted with great strictness, much greater than in the pre-antitoxin period. In the absence of bacteriological examination, the diagnosis of early and mild cases must often be doubtful; but in the severer cases, including the laryngeal cases, I am convinced that no appreciable fallacy has been introduced. All cases of possible catarrhal laryngitis have been excluded.

The first division of the antitoxin period may be regarded as, to some extent, transitional. We used a serum of unknown but certainly very low antitoxin strength, of which 23 c.c. was usually injected. In the second division the same serum was mainly used in doses up to 100 c.c, and much better results were obtained. Since then we have used a serum of known antitoxin strength, and severe cases have usually received 4,000 units. Latterly, we have increased this dose in some cases to 6,000, 8,000, or even 12,000 units, and we believe that the lowered mortality of the last division is partly attributable to this increase.

The following tables give the total mortality for both pre-antitoxin and antitoxin periods :—

CASES OF DIPHTHERIA TREATED IN THE CHILDREN'S HOSPITAL, BRISBANE.

——	Admissions.	Deaths.	Mortality.
PRE-ANTITOXIN PERIOD—			Per Cent.
July, 1889, to June, 1891, two years	73	34	46·6
July, 1891, to June, 1893, two years	114	48	42·1
July, 1893, to January, 1895, eighteen months ...	116	46	39·7
Total	303	128	42·2

LARYNGEAL CASES.

——	Admissions.	Deaths.	Mortality.
ANTITOXIN PERIOD—			
Small doses—			Per Cent.
January, 1895, to November, 1895, eleven months ...	40	10	25·0
Large doses—			
November, 1895, to September, 1896, ten months ...	43	5	11·6
September, 1896, to December, 1897, fifteen months...	83	8	9·8
January to December, 1898, twelve months	45	7	15·6
January to August, 1899, eight months	106	10	9·4
Total	317	40	12·6

In presenting these statistics I must not omit to acknowledge my indebtedness to the hospital residents, L. N. Ashworth, M.B., B.S. (Melb.); T. E. Green, M.B., B.S. (Melb.); Hugh Bell, F.R.C.S. (Eng.); and P. Dixon, M.B., Ch.M. (Syd.), to whose assiduous care the good results are greatly attributable, and but for whose careful records I should be unable to write the present paper.

Although the whole number of cases is small as compared with other statistics that have been published, they have the advantage of having been treated in the same institution and under the same personal observation. Their lesson appears clear enough, but it is necessary, in order to exclude any possible doubt, to consider objections that have been raised by various critics to other similar statistics.

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* Owing to pressure of other work I ceased making these examinations, but I hope they will be soon resumed by the hospital residents, for which purpose a laboratory is being fitted up.

It has been objected that the introduction of a new treatment is accompanied by greater attention to the cases, and more care on the part of the residents and nursing staff. As far as our hospital is concerned, I can testify that the greatest possible pains were taken over these cases during the pre-antitoxin period. It will be shown presently that nearly half of the cases were so severe as to necessitate intubation or tracheotomy, and, allowing for this, the results obtained compared favourably with other institutions at the same date.

Again, it has been urged that the introduction of bacteriological methods of diagnosis has led to the inclusion of mild cases which would not formerly have been recognised as diphtheria, and so caused an apparent reduction of mortality. This certainly does not apply to our present figures. Cases not recognisable clinically as diphtheria, but in which diphtheria bacilli are present, have occurred with us very rarely. We have only record of three such cases (which came under observation in 1897), and these cases have been omitted from our statistics.

Lastly, it is asserted that the type of the disease has changed, and that, as a whole, it is of a milder character than formerly. For this assertion there is no foundation; in fact, it is a pure assumption, which can be satisfactorily met. Some observers have attempted to meet it by classifying their cases as mild, severe, very severe, and so on. This is hardly satisfactory, as the classification depends too much on the individual opinion of the observer. There is, I think, a better way. All are agreed that diphtheria affecting the larynx is a serious disease. By including all cases with a distinct laryngeal stridor,* it is possible to draw a sharp and objective line including a large proportion of the cases which may be known as laryngeal cases. These cases are always serious, and among them the mortality used to be very high. It has now been reduced by more than two-thirds, as is shown by the following table:—

LARYNGEAL CASES.

—	Cases.	Deaths.	Mortality.
PRE-ANTITOXIN PERIOD—			Per Cent.
July, 1891, to January, 1895†	147	87	59·2
ANTITOXIN PERIOD—			
Small doses—			
January, 1895, to November, 1895	22	9	40·9
Larger doses—			
November, 1895, to September, 1896	30	4	13·3
September, 1896, to December, 1897	37	7	18·9
January to December, 1898	20	6	30·0
January to August, 1899	68	7	10·3
Total	177	33	18·6

In the pre-antitoxin period very few cases recovered without operative interference—only about one case out of twelve. Now, although we never fail to intubate a laryngeal case as soon as it appears necessary, nearly half the cases recover without interference.

No doubt, with earlier treatment, the proportion would be larger, for many of the hospital cases require operative interference immediately or very soon after admission.

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* Cases showing merely hoarseness, croupy cough, or even visible membrane on the epiglottis are excluded. It is always possible to distinguish laryngeal stridor from pharyngeal stertorous breathing.

† Record incomplete before July, 1891.

LARYNGEAL CASES.

—	Cases.	Recovered without Operation.	Proportion.
Pre-antitoxin Period—			Per Cent.
July, 1891, to January, 1895	147	12	8·4
Antitoxin Period—			
Small doses—			
January, 1895, to November, 1895	22	3	13·6
Larger doses—			
November, 1895, to September, 1896	30	12	40·0
September, 1896, to December, 1897	37	12	32·2
January to December, 1898	20	8	40·0
January to August, 1899	68	33	48·5
Total	177	68	38·4

Operative interference is still necessary in more than half of the hospital cases. In private practice it should be much less frequently necessary. But even in hospital cases requiring operation the mortality has been reduced from nearly two-thirds of the cases operated on to about one-fourth of the cases operated on.

OPERATION CASES (INTUBATION OR TRACHEOTOMY, OR BOTH).

—	Operation Cases.	Deaths.	Mortality.
Pre-antitoxin Period—			Per Cent.
July, 1899, to June, 1891 (two years)	42	28	66·7
July, 1891, to June, 1893 (two years)	60	41	68·3
July, 1893, to January, 1895 (eighteen months) . .	64	40	62·5
Total	166	109	65·7
Antitoxin Period—			
Small doses, January to November, 1895 (eleven months)	19	9	47·4
Larger doses, November, 1895, to September, 1896 (ten months)	18	4	22·2
September, 1896, to December, 1897 (fifteen months)	25	7	28·0
January to December, 1898 (twelve months)	12	4	33·3
January to August, 1899 (eight months)	35	7	20·0
Total	109	31	28·4

I regard the total results obtained in these laryngeal cases as of the greatest value, inasmuch as they deal with a sharply defined group of cases of a very grave form of diphtheria, and are not liable to erroneous diagnosis in any appreciable proportion, so long as cases of possible catarrhal laryngitis are excluded, as we have been careful to do. They are furthermore fairly comparable with the hospital returns of laryngeal cases in other parts of the world, which the total admissions are not, for these are affected by the proportion of mild tonsillar cases admitted. The total results may be conveniently stated as follows:—Of 100 cases of diphtheria with affection of the larynx treated in the hospital in the pre-antitoxin period, eight recovered without operative interference, thirty-three recovered after either intubation or tracheotomy, and fifty-nine died. Of 100 similar cases treated with antitoxin in sufficiently large doses, as many as forty now recover without operative interference, another forty-five after operation, and only fifteen die.

I do not wish to weary you with figures, and will therefore only give you one more table, which shows the difference in the mortality of all the diphtheria cases admitted under the old and new methods of treatment at each year of life.

MORTALITY OF ALL CASES OF DIPHTHERIA AT EACH YEAR OF LIFE BEFORE AND AFTER THE INTRODUCTION OF ANTITOXIN.

Age Last Birthday.	PRE-ANTITOXIN PERIOD.			ANTITOXIN PERIOD.		
	Cases.	Deaths.	Mortality.	Cases.	Deaths.	Mortality.
Under one year	11	9	81·8 per cent.	7	2	28·6 per cent.
One year	44	32	72·7 ,,	35	10	28·6 ,,
Two years	56	27	48·2 ,,	48	10	20·8 ,,
Three years	44	19	43·2 ,,	44	6	13·6 ,,
Four years	53	14	26·4 ,,	47	3	6·4 ,,
Five years	38	9	23·7 ,,	54	4	7·4 ,,
Six years	23	10	38·5 ,,	42	4	8·2 ,,
Seven years	16	5		19	1	
Eight years	6	1	16·7 ,,	8	...	0·0 ,,
Nine years	7	...		6	...	
Ten years	...	...		5	...	
Eleven years	1	1		2	...	
Twelve years	3	...		...	...	
Thirteen years	1	1		...	...	
Total	303	128	42·2 per cent.	317	40	12·6 per cent.

The decrease in mortality is striking at all ages, and is particularly well marked during the first two years of life, which had, under old methods of treatment, such a fearful deathrate.

To these statistics I might add others equally convincing from Australia, from America, from France, from Germany, from England. If hospital records can prove anything, the controversy regarding the efficacy of antitoxin in the treatment of diphtheria may be regarded as closed. But it yet remains to consider in what way the remedy should be used to obtain the best possible results in reducing the mortality of the disease. It will clear the way to the understanding of this problem if we consider it very briefly from the standpoint of recent pathology.

Diphtheria is known to be caused by a bacillus which secretes a powerful toxin. The local irritation produces a fibrinous exudation which forms the diphtheritic membrane, and the diphtheria bacilli are confined to this membrane and the fluid discharged from it. The inflammatory processes have a tendency to spread locally, especially into the air-passages. In this way the toxic organism may reach the lungs, but it does not invade the other organs of the body, which contain no more than a few stray individuals of no practical importance, who may happen to be washed there by the blood-current. Apart from the mechanical obstruction of the breathing, all the danger of the disease results from the absorption of diphtheria toxin from the local focus of disease. The antitoxin contained in the serum derived from artificially immunised horses has no direct effect on the diphtheria bacilli, but neutralises the toxin produced by them. This it does by some form of chemical action, neutralising the toxin in a test-tube in strictly equivalent proportions, the two behaving in this respect exactly like an acid and an alkali. As a matter of experience it is found that when their toxin is thus neutralised the diphtheria bacilli in the local lesions become harmless; they are dealt with by the tissues as ordinary saprophytic bacteria, and the case so treated makes a speedy recovery to the normal condition. When mixed in a test-tube the proportion of antitoxin required to neutralise many times the lethal dose of toxin is very small. When the two are injected into an animal simultaneously or with only a short interval, the proportion is somewhat larger, but still remains small; for antitoxin and toxin behave to each other in the blood-stream as they do in the test-tube. But if a multiple of the lethal dose of toxin be injected twenty-four hours or more before the antitoxin, the proportion of the latter required to save life becomes enormously increased, and a thousand-fold the dose sufficient in the former instance may be now inadequate. The explanation of this is that the toxin is no longer wholly contained in the blood-stream. It has entered into some kind of combination with those tissue cells which it particularly affects. In the case of tetanus, for example, a minute dose of antitoxin may be sufficient if injected simultaneously, while the largest possible dose may be insufficient when the toxin has attached itself to the substance of the cerebro-spinal motor cells. That it does so attach itself is shown by the fact that if the toxin is mixed with an emulsion of cerebro-spinal tissue in sufficient proportion before injection it becomes harmless. In tetanus, the combination, once formed, is unfortunately very stable; but in the case of the diphtheria toxin it appears that, by maintaining a very high degree of antitoxin strength in the blood, the toxin may be neutralised even after it has fixed itself in the tissues. But, to do this, very large doses are necessary, and the longer the interval allowed before their use, the larger must they be.

Applying these considerations to practice, we should, in the first place, inject our remedy as early as possible, as soon as we have made our diagnosis, and frequently, in doubtful cases, before a positive diagnosis is possible. In very early cases, an ordinary dose of 600 to 1,000 units is quite enough. The course of the disease is rapidly arrested, and convalescence is speedy and complete. But it is quite different with cases of long duration, presenting extensive membrane extending, it may be, into the nose or larynx, or in cases showing symptoms of toxæmia. In these the patient, in spite of appearances in some cases, is in a condition of imminent peril. The toxin is already fixed in his tissues, and death from implication of the central nervous system, heart, or kidneys will very likely occur. We may now be too late to avert the fatal result, but fortunately we may often do so by using very large and, if necessary, repeated doses. In a really bad case of this description I should administer from 6,000 units at once, and perhaps inject an additional 2,000 or 3,000 units or more within the next twenty-four or forty-eight hours. No time must be lost in producing a high degree of antitoxin strength in the patient's blood. According to my experience, about twenty-four or sometimes forty-eight hours elapse before improvement begins; it is then often rapid. The antitoxin appears to require this time to produce its effect, and in cases which die within twenty-four or forty-eight hours of administration the largest doses sometimes do not appear to have any effect. Notwithstanding this, the good effects of large doses in advanced cases have been well illustrated in our experience. During the first eleven months of antitoxin treatment our dosage was too small, and the mortality only showed a reduction of 17 per cent., but by using much larger doses we immediately obtained a further reduction of 13 per cent., and this has been well maintained since. The following two cases will serve to illustrate recovery from grave toxæmia:—

Minnie D., aged four years, admitted on the fifth day of illness. Membranous patches on tonsils and pharnyx. Breathing with stridor and recession, colour dusky, 5,000 units injected. Intubated soon after admission. That evening, though breathing was easy, the child had a bad colour, lips were pale and dusky, pulse rapid and weak, and she was very restless. These symptoms pointed to grave toxæmia, and 3,000 additional units were injected. Prognosis unfavourable.

Second day.—No improvement. Tube coughed up once and replaced. Râles and rhonchi in chest.

Third day.—Condition still very grave. Urine with one-sixth albumen; 4,000 units injected. Tube was coughed up, and she had several attacks of dyspnœa, which were not relieved by reintubation. After a severe attack with cyanosis, Dr. Dixon performed tracheotomy, and extracted a large cast of membrane from trachea. After operation, pulse was extremely shabby.

Fourth day.—Slight improvement, gradually leading to convalesence. The total amount of antitoxin administered was 12,000 units.

Elsie C., aged six years, admitted on the eighth day of illness. Membranous patches on tonsils and pharynx. Breathing with stridor and recession; 6,000 units injected.

Second day.—Breathing worse. Intubated. During operation coughed up a large piece of membrane; another 3.000 units injected, pulse poor, urine scanty, 2 oz. since admission, one-third albumen. Ordered pilocarpine grain one-twentieth, and hot-air bath which caused free perspiration.

Third day.—Urine 3 oz., about one-quarter albumen. Hot-air bath repeated. Another 3,000 units injected. Pulse still poor.

Fourth day.—Urine 3 oz., one-sixth albumen. Hot-air bath repeated. Seems brighter. Intubation tube removed.

Fifth day.—Better; perspiring freely, urine 5 oz., trace albumen. From this point steady improvement to convalescence. In all, 12,000 units were administered.

There is much to be learnt from an analysis of the fatal cases. In 1897, 1898, and 1899 we have had twenty-five deaths; of these, sixteen occurred within forty-eight hours and eight within twenty-four hours of admission; they may be classified, according to the main cause of death, as follows:—

	Cases.
Primary toxæmia	6
Bronchial membrane (in two cases combined with pneumonia)	4
Pneumonia	9
Secondary toxæmia (with scanty urine or suppression)	5
Accidental asphyxia (from coughing out tracheotomy tube during night) ...	1
Total	25

The first two headings caused ten deaths, all of which occurred within forty-eight hours of admission. One, an infant of eighteen months, was admitted on the third day of illness. At this age the disease runs an exceptionally rapid course. Two were admitted on the fourth day, the remainder on the fifth to seventh day of illness. In all the cases under this heading the disease was apparently too far advanced on admission to be combated. They died as most cases used to die before the antitoxin treatment was introduced.

Pneumonia was responsible for nine deaths, of which seven occurred within forty-eight hours. Since the introduction of the new treatment this cause of death has come into greater prominence; for while the antitoxin will neutralise the diphtheria poison, it has no effect on this complication, which is due to a secondary infection. In two cases examined by me this was found to be due to the pneumococcus, but other observers have found the streptococcus pyogenes to be frequently the exciting cause. Two of those cases, both children two years of age, were admitted, according to the parents' statements, on the second day of illness. If the statements were correct, they show that even early treatment of the diphtheria by antitoxin will not always prevent a fatal issue from pneumonia. Clinically these cases may be recognised by their extremely rapid, shallow breathing, with high temperature and rapid failing pulse. Physical signs are sometimes to be elicited. Several cases were verified *post-mortem.* I have seen cases complicated with well-marked pneumonia recover, but it has more often proved fatal.

Under secondary toxæmia I have included a very important class of cases which I had observed before the introduction of serum treatment, but which has assumed a higher relative importance lately. They are nearly always cases of long duration on admission. The earliest was admitted on the fourth day of illness, but this is exceptional; one on the fifth, the remainder had been ill a week or more. They have had extensive areas of membrane in nearly every instance. Some have at first appeared to be doing well, but after several days' treatment the urine is observed to be scanty and loaded with albumen; sometimes there is complete anuria. At the same time the child develops a painfully marked restlessness, is totally sleepless, but retains consciousness to the end, while the extremely rapid pulse fades away and often becomes imperceptible at the wrist for many hours before death. These are often classified as cases of heart failure, but they are not properly regarded as cases of simple heart failure. That the condition of the kidneys is an important factor in the result I have no doubt, but they are not cases of uræmia in the usual acceptance of this term. Convulsions I have never observed, and coma is absent, being replaced by an exactly opposite condition. I have never seen a case, in which these symptoms had developed, recover under any form of treatment; but of late I have endeavoured to anticipate their occurrence in cases with scanty albuminous urine by the injection of pilocarpine (gr. one-twentieth) and hot-air baths, as in the case of Elsie C., given above. I believe nasal cases are particularly liable to develop these symptoms, and such cases ought to receive a large initial dose of antitoxin, although their condition at the time may not appear serious.

If we classify our cases since the commencement of the antitoxin period according to the day of the illness on which antitoxin was first administered, we obtain the following:—

——	Cases.	Deaths.	Mortality.
Treatment commenced on first day	7	...	
Treatment commenced on second day	39	2	
Treatment commenced on third day	50	6	
Treatment commenced on fourth day	51	4	
			Per cent.
Total...	147	12	8·2
Treatment commenced on fifth day or later	131	22	16·8
Cases of uncertain duration	39	6	15·4

The two cases fatal from pneumonia, although antitoxin was administered on the second day of illness, have already been mentioned. Of the six deaths among those admitted on the third day, one occurred during the first period of antitoxin treatment, when the doses administered were too small. Of the remaining five, four died from pneumonia; the last case was an infant of eighteen months, admitted moribund from toxæmia on the third day of illness, and has already been alluded to. It is the only case we have seen since 1895 in which death occurred from uncomplicated diphtheria treated with antitoxin during the first three days of the disease.

These results emphasise the importance of early treatment. Every medical practitioner ought to have a supply of diphtheria serum and a suitable syringe at hand. In the large towns there is no difficulty in obtaining a supply, and it is easy to keep a few bottles of the liquid serum, and to replace them as needed. The liquid preparation is the easiest for use, and may be kept for six months or more without losing its efficacy. Practitioners in the bush townships should keep a supply of the dried serum, which can probably be relied on to retain its strength unimpaired for years. It is absurd that a practitioner several days' journey from the metropolis, meeting with a case of diphtheria, should have to telegraph for antitoxin. This is too often not any use to him by the time he gets it.

All other forms of treatment are of very minor importance. Routine applications to the throat have been altogether abandoned in the Children's Hospital. We only use them when the membrane is sloughy and offensive. It has been argued on theoretical grounds that, as antitoxin only neutralises the poison, something should be done to kill the bacilli. But I have found by bacteriological examination that in the majority of cases the bacilli disappear within a few weeks without any local treatment; and in those cases in which they persisted longer, I have been unable to hasten their disappearance by such treatment. Local applications are, in nine cases out of ten, unnecessary; and unnecessary treatment is bad treatment. When, however, there is much nasal discharge, gentle syringing with salt and water (a teaspoonful to a pint) is advisable. Internal medication has also been abandoned as a routine measure. When the pulse is showing signs of failure we have given digitalis, strychnine, and alcohol in moderate doses, and these may be of some service. When the urine is very scanty and albuminous, I have lately administered pilocarpine hypodermically (the dose should not exceed gr. one-twentieth), followed by a hot-air bath, administered by covering the child with a cradle and blankets, under which the moist hot air of a spirit lamp flame is conducted through a bent metal tube. In cases dying from bronchial membrane or pneumonia, inhalation of oxygen gas is of value in relieving distress and promoting euthanasia. I have not seen it save life.

To sum up the whole matter, the treatment of diphtheria consists in the administration of antitoxin early and in sufficient dose. A trustworthy preparation of known antitoxin strength should be employed.* There need be no fear of administering an overdose; the only objection to giving an unnecessarily large dose is its cost. To give too little for this reason is a very poor economy. The saving in the number of doctor's visits, not to mention funeral expenses, much more than covers the cost of the serum. In the case of poor patients, I have been accustomed to explain the danger of the disease, and have never found them object to the expense of the remedy. Those who cannot afford this are suitable cases for hospital treatment. I would certainly decline to take any responsibility or to continue to attend a case if the treatment were objected to, and I hope other practitioners will do likewise. I would appeal particularly to those engaged in club practice, who are more bound to their patients, to explain to them the need for immediate injection of serum or immediate removal to the hospital. Too many patients of this kind have been brought to us too late.

We may say that if parents and medical men did their duty—the former in calling assistance early, the latter in applying the remedy early and in sufficient dose—the death rate from diphtheria might be reduced to a small fraction of what it is at present, although it has been materially lowered in this colony of late years. Indeed, with the necessary qualifications, we may say no child ought to die of diphtheria. There are no doubt some cases complicated by pneumonia at a very early stage, some others at a distance from medical aid, some few in which the diagnosis in the early stages is extremely obscure.† But these are rare exceptions; for the most part death from this disease is avoidable, and ought to be prevented.

Dr. G. T. Howard (Melbourne) said that the Melbourne Hospital was the only institution about Melbourne which would admit cases of diphtheria. There were a great many cases, and the mortality in the pre-antitoxin period used to be very large, as shown by Dr. Turner; but since the introduction of the antitoxin there had been such a persistent lessening of the mortality that the Melbourne Hospital staff were one and all firmly convinced that to the antitoxin alone was due the credit of these results. He suggested the advisability of using—in remote country districts, especially in very hot regions where the liquid might be expected to deteriorate—the dried serum. The cost was less than one-sixth of the liquid, and, according to Dr. Ritchie, of Horsham, Victoria, who had used it in nearly 100 cases, it was quite as efficacious.

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* We have, for the past three years, used that prepared under the supervision of Professor Behring, tested for the German Government by Professor Ehrlich.

† For instance, cases sometimes occur of laryngeal diphtheria without any visible membrane, and these are sometimes very hard to distinguish from catarrhal laryngitis. In cases of doubt, a full dose of antitoxin should be given. Sometimes an attack of diphtheria becomes engrafted on a previous catarrh or tonsillitis, and is then very likely to be overlooked in the early stages.

Dr. Scot Skirving, in reviewing the discussion which had taken place, said that if they did nothing else in the Section, these two papers would have been a good enough reward in knowledge gained. They had had demonstration of two things: 1. The absolute value of antitoxin, a value which he was sorry to hear was being now doubted in some quarters in England—hence the necessity of again reiterating its use. 2. The value of very large doses. Dr. Gill had used doses quite beyond his experience, but after the results attained he would certainly not hesitate to inject the larger measures.

NOTES ON THE ORIGIN OF SEX.

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Gentlemen,—In this paper, which I have the honour of reading before you to-day, I do not intend to enlarge upon the physiological processes which occur during menstruation, ovulation, or conception, but rather desire to place before you shortly the results of some sixteen years of personal observation and experiment directed towards the solution of a great problem hitherto hidden from us.

The most casual observer must have been struck with the fact that the ratio of male to female children at birth varies so slightly in different countries, and amongst different races, as to almost constitute a law. I find that the consensus of statisticians upon this point, taking some millions of births throughout Europe into consideration, places this ratio at about 106 males to 100 females. In striking contrast to this average preserved, regardless of environment, throughout the human race as a whole, stands out the disproportion between the sexes often observable in different individual families. This disproportion must be produced by some cause, and this cause must evidently exist in the parents responsible for the creation of such families. Is it not possible to discover in what way these parents are responsible for such disproportion? Can we not find some quality, some characteristic, or possibly some pathological condition in either parent which would give us the clue to this phenomenon? And further, if we do find some such condition, would it not be feasible to follow up such a clue, and, enlarging upon it in practice, maybe produce for ourselves whatever ratio between the sexes we may desire.

With some such process of reasoning in my mind, I resolved, in the year 1883, during a twelve months' residence in the Rotunda Hospital, Dublin, to take advantage of the right of entrée to many hundreds of homes in the slums of that city, and there to find if the overgrown families of the Irish lower orders could provide me with a reply to my questions. I find by my notes that I visited, for this purpose, between 400 and 500 families, containing in many instances over twelve children to the family, in none less than three, and affording me a mass of most interesting material for my purpose.

The lower Irish marry young, are rarely sterile, and in the matter of procreating their species take no thought for the morrow. I accordingly had a fair field for investigation, unhampered by the many side issues of our modern civilisation. In the majority of families I found the greatest disparity between the sexes of the children, amounting in some instances to whole families of ten or more of one sex; in others, again, the sexes were evenly balanced. In compiling my notes, I find that, of a total of 2,540 children noted, the sexes were:—Males, 1,313; females, 1,227; a fairly balanced ratio, which, taken *en masse*, made by contrast the disproportion mentioned in the majority of families all tho moro striking.

With this material to work upon, I cast about for a solution of the question:—Why had A B, aged thirty, cab-driver, with a wife aged twenty-seven, a family of one girl and six boys; while his neighbour A D, aged thirty-three, labourer, with a wife aged twenty-nine, had a family of seven girls?

I had many such examples to meditate upon, and in all instances this one striking fact impressed itself upon my attention, viz.:—The family which contained an undue majority of male children possessed also a mother proportionately more vigorous, and in better health, than her husband; while in the family with an undue majority of female children the father was proportionately superior in vigour and health to his weakly wife. In many families this rule was exceedingly striking; in others, where the sexes were more evenly matched, so also were the parents. In none of my cases investigated could I find any special trait in the parent which ran through any series of cases, or was sufficiently marked to lead me to suppose that it had any special bearing upon this determination of the sex of the offspring; but, in all, the good health—or, in contra-distinction, the ill-health, whether hereditary or acquired by dissipation or otherwise—of the parent was the guiding cause of such determination.

I desire here to emphasise and define what I mean by the word "health." In these notes I use the word in a broad sense of the term, and as meaning practically that condition brought about as the resultant of those forces which create in their owner a longer or a shorter "expectation of life." In every case, before coming to a conclusion, I investigated, where it was possible, not only the personal health of the parent, but also such hereditary influences and family history as would guide me to a correct opinion as to that person's expectation of life. I have come in latter years, in working out my theory, to attach more and more importance to the hereditary constitution possessed by the parents as a factor in determining their relative health, specially considered with respect to their procreative abilities.

In short, gentlemen, my investigations afforded me many a striking illustration of what is now the largely supported theory of the crossed-heredity of sex. The more I analysed my investigations, the more this theory seemed to me to be rational, and I determined that, whenever the opportunity should offer, I would endeavour to put into practice, in special and suitable cases, what Nature in her vast laboratory was apparently so methodically and unostentatiously regulating.

I began by formulating for myself a hypothesis, which I have since then seen no reason to alter or modify, viz.:—"The sex of the child is determined at the moment of conception, and is the opposite to whichever parent is at that moment in relatively the more vigorous health."

It then appeared to me that, if such a hypothesis be correct, it should be possible to control the health of a given pair of parents to such an extent, and so to influence their relativo strengths, as for the time being to reverse the ratio of their procreative abilities—*e.g.*, a married couple has a family consisting of girls, and it is of great importance that a son should be born. Would it not be possible, by improving tho health of the mother, or by depressing tho condition of the father, or, if necessary, by both of these methods simultaneously, to reverse the ratio formerly existing between the sex-producing abilities of these parents?

In the first instance I resolved, in order to test my theory, to make an experiment upon the lower animals, and for this purpose chose a pair of fox terriers which I possessed at the time. This was in the year 1886, when I was practising medicine in St. Kilda, Victoria. The pair was very evenly matched in size (the dog weighed 15 lb. and the slut 13 lb.), and in age (three years), and I had already had two litters of puppies from them; the first litter of five containing three bitches and two dogs, and the second of three containing two dogs and a bitch. Both animals were in excellent health, and there was apparently nothing to choose between them in this respect. I determined to depress the health of the dog, and to further elevate that of the slut. I had a period of two weeks in which to effect this purpose, and during this time the dog was kept in close confinement, given little or no exercise, and fed sparingly on bread and water. He became thinner, losing 2 lb. in a fortnight, and his whole condition suffered. The slut was given plenty of fresh air and exercise, was fed bountifully on dog biscuit, meat and milk, and, by the time that she was ready to be lined, was in perfect condition, and in marked contrast to her almost impotent and unwilling mate. In due course the litter arrived, and to my delight, consisted ot five dog pups.

This was so far convincing to me, but may have been a mere coincidence; so I decided to reverse the conditions for my next litter, and on the next occasion, four months having elapsed, the slut was the underfed prisoner for the first ten days of her heat, while her lord and master received more than his ordinary care and attention, in the way of exercise and diet, in the way I have already described. The result of this coupling was a litter of four, three bitches and one dog—again fairly satisfactory.

I will not weary you with details of further experiments, many of which were made on similar lines with these and other dogs; let it suffice to say that they extended over a period of twelve years, and, with hardly an exception, gave results strikingly satisfactory to the working out of my theory. I also endeavoured to apply the same methods in breeding guinea pigs, but with not very satisfactory results; partly owing, I believe, to the difficulty in controlling the health of these animals, whilst still preserving the vitality requisite for breeding purposes. Personally, my experiments on the lower animals have been practically limited to those on dogs; exigencies of general practice giving little opportunity or time to go further afield.

I have, however, received striking confirmation of my theory from the manager of a large station in New South Wales, who informs me that it has been his practice for some time past, when he has desired to perpetuate any special strain of cattle, and required for that purpose a heifer from a given pair of such cattle, to stall-feed the bull and turn the cow out to grass for a definite period prior to her being served by that animal, or with some such similar treatment to depress the female parent, and elevate the male, with the result that he has been rarely disappointed ot his heifer. The reverse treatment he has found to produce him a bull calf. I see no reason why the same methods could not be applied to horses, or sheep, if necessary.

My first opportunity for experiment upon man was in 1888, when I was consulted by a German hotel-keeper, aged thirty-eight, with reference to his wife, aged thirty-five. In course of conversation he deplored the fact that his wife had presented him with eight children, all boys. I found his wife a fine specimen of womanhood, of excellent family history, faultless physique, and certainly of superior stamina to her husband. I found that the latter had originally come to Australia to check a threatened pulmonary phthisis. The cause of the woman's illness, concerning which I was consulted, necessitated surgical treatment, and after a three weeks' confinement to bed she returned home to convalesce, in health considerably enfeebled and depressed below her usual wont.

I could not resist the temptation of propounding my theory to this hotel-keeper, and, when he heard my arguments, he expressed the greatest willingness to submit himself to whatsoever conditions I desired to impose, if only they would result in the birth of the much-desired daughter. During his wife's illness I accordingly placed him under treatment, and, with a generous diet, enforcement of regular hours, and a course of stimulating and tonic drugs, he became as fit in condition as it was possible to make him. His wife, on the contrary, as I have said, was rendered, by convalescence from her operation, feeble and depressed in general health, and, for the time being, physically his inferior, so there was no further need for treatment to add to her want of condition. The result of the experiment was eminently satisfactory. Nine months after her return home the wife presented a fine daughter to her now no longer sceptical husband.

I was now pretty confident of the success of my method, and during the following year had under treatment three families among my more intimate friends, whose children were arriving all of similar sexes, and who were sufficiently desirous of possessing a variety of sexes to place themselves under treatment for that object.

Families A and B each contained two children, all girls, while family C contained four boys. My problem was to produce a boy in each of the families A and B, and to present a girl to family C. In all these three cases my methods were adopted, and in every instance with complete success.

The plan I laid down for adoption in these instances I have pursued in many other cases which I have had the opportunity of treating up to the present time; and one description of my methods of procedure may fairly serve, to illustrate the whole series. I first make a careful examination of both parents, with a view to discovering and, if necessary, treating and eliminating any habit, disease, functional disorder, or pathological defect that may exist, and have any bearing upon the state of health of the weaker parent. This I consider of great importance. Should any such exist, it must, first of all, be removed, or bettered as far as practicable. In some cases, for instance, I found on investigation the inferior parent suffering from albuminuria, hitherto unsuspected, but eloquent of his inferiority; such a condition, if tractable, was treated; but, in a few of such pathological conditions, of course, treatment was powerless to remove the cause of inferiority. These causes having been eliminated, I next forbid all sexual intercourse for a period of one month, dating from a given menstrual period; and during this month I place both parents under treatment, with a view to building up and elevating the general health of the hitherto inferior parent, and at the same time depressing the general health and tone of the hitherto superior parent.

I achieve the former condition—(*a*) With a suitable *régime* of stimulating diet, specially adapted to the constitution to be dealt with, and varying, of course, considerably in different cases. (*b*) By insisting on regular hours for sleep, exercise in the open air and sunshine. (*c*) With a course of suitable tonic and stimulating drugs. (*d*) By way of the many other means well known to us all, and used by us for building up the general tone of the system—*e.g.*, change of air, if necessary; cessation from over-strain in mental or physical work; and removal, if possible, of worries, anxieties, and the like.

I achieve, on the contrary, the latter condition—(*a*) By recommending a course of low diet, into which non-nitrogenous elements largely enter. (*b*) By ordering a maximum of sedentary and brain work, with a minimum allowance of sleep. (*c*) By prescribing a course of nerve sedatives—*e.g.*, the bromides, iodides, &c., for the latter half of the month of probation.

It is, as a rule, a simple matter, by these means, to reverse for the time being, and sufficiently for our purpose, the balance of superiority of health hitherto existing between the parents; and I believe, though I have not the facts to go upon, that a less period of treatment than one month would, in many cases where the balance is more evenly poised, be equally efficacious.

The month having expired, intercourse is now permitted on the third day after cessation of menstruation, at which period experience has taught us that the female is more readily impregnated, and after this month of abstinence I rarely found that impregnation did not result. Pregnancy having been fairly established, I stop all further treatment, my observations and experiments inclining me to the belief, now generally accepted, that in man the sex is once for all determined at the moment of conception, and further treatment being on that account superfluous.

With such methods, except, of course, in cases where incurable disease in either parent rendered any experiment nugatory, I have in very few instances been disappointed in the desired result. When success has not been achieved, I have always been able to trace some dereliction on the part of the patients themselves, in the carrying out of details imposed; but where I have been loyally followed by both parents, who have fallen in with the line of treatment in a rational and whole-hearted manner, I have not had a single failure. The difficulty, as you will readily understand, is in attaining the necessary depression of tone in the superior parent, without, at the same time, inflicting any harm; and I found that the most satisfactory, and at the same time least harmful means, in addition to the rules specified, lay in a course of iodide of potassium, prescribed for ten days or a fortnight before intercourse. Any temporary depression induced by such treatment speedily disappeared upon resumption of ordinary *régime.*

During the past twelve years I have had many opportunities of testing my theory on these lines, and I possess records of most satisfactory results, which I would fain place before you *in extenso* were I not for obvious reasons prevented from so doing. In brief, I have had under treatment during this period forty-five cases; in six instances treatment was futile, and was not persevered with on account of advanced organic disease—phthisis, cancer, and marked chronic nephritis—having depressed the inferior parent beyond all hope of even temporary elevation; of the remaining thirty-nine families all were picked examples for the application of my methods; each family contained an undue proportion of either male or female children, and in every instance I found the same conditions existing, in a more or less marked degree—viz., the majority of male children possessed a proportionately more healthy mother, the majority of female children a proportionately more healthy father. To reverse this proportion of health between the parents was in some instances no easy task, but I think you will agree with me that the results achieved—viz., thirty-two successes and but seven failures—were very gratifying. In four of the cases which I have marked as failures, no pregnancy could be induced, and these can hardly be considered as coming within the test; and the remaining three were undertaken in so half-hearted a manner by the parents themselves that I was not surprised when they declared my method to be of no avail.

I would here like to draw your attention to a few of the more modern theories respecting the determination of sex. It is quite impossible even to enumerate the many hundreds of such theories extant, some of which border on the grotesque, but the following better known and more generally received opinions seem to me to have a special bearing upon the views I have endeavoured to lay before you :—

Girou, in 1823, propounded his theory of "Comparative Vigour," asserting that the sex of the offspring followed that of the moro vigorous parent.

Let us suppose, for a moment, that this is so, and that the more vigorous parent does perpetuate its own sex. The father, then, in his strength, begets a family of males; transmits to them his strength and power to further beget other families of males, and so on through generations. If such could be, there gradually arises a colony of males, all potent to propagate their own sex, but without the ability to beget daughters; and what becomes of the world's beautifully balanced proportion between the sexes, or, further still, of the human race itself? On the other hand, we see, as I affirm, the same healthy father begetting a family of female children. These inherit their father's strength and power to procreate once more the opposite sex, and here we have, in a few generations, the equipoise of sexes established. Many times during the past years have I observed illustrations of such a compensation in succeeding generations, where a family exclusively of boys has grown to manhood, married, and begotten a large preponderance of girls in the generation following.

Of Girou's theory, Geddes and Thompson, in their "Evolution of Sex" (1889), say: "It cannot be said that facts bear out the case, thus: Consumptive mothers produce a great excess of daughters, while Girou's theory would lead us to expect the opposite." My own observations corroborate this opinion of Geddes and Thompson, and I quote their words as an apt illustration of the principle that the healthier parent procreates children of the opposite sex.

Hofacker and Sadler, in 1828 and 1830, formulated their law that "the sex of the child depends upon that of the parent whose age is in excess of that of the other," and quote statistics showing conclusively that in proportion to the excess of age of either parent so does the excess of the corresponding sex of the offspring increase. Here in these statistics, I hold, is another illustration of my theory—for the aged husband, having a poorer "expectation of life" than his young and vigorous wife, she becomes the "healthier" parent, and almost invariably bears male children to him.

Coming to later years, Charles Darwin does not attempt to form any theory, but "sees that the whole problem is so intricate that it is safer to leave its solution for the future."

Düsing, in 1883, collects a mass of most interesting data to show that the numbers which go to make up the proportion between the sexes are in a sense self-regulating; that is to say, that Nature seems to provide means, if one sex be in a decided minority, of producing a majority of that sex, or of always producing the sex that is wanted. For example, he notes that after a war, when there is a lack of males, more male children are born.

Amongst other regulating factors he dwells on frequency of sexual intercourse with its consequent rapid childbearing, and notes that wherever we have a minority of females from any enforced condition "their fertilisation tends to occur more frequently, and thus they produce a majority of their own sex."

I quote these examples from his work, and believe that my theory will readily explain the method in which these disturbances in the proportion of the sexes are regulated. Thus, troops after a war are in a condition of health considerably below the average, and when mating on their return are in the great majority of instances inferior in health to the women awaiting them. The offspring from such unions, therefore, are the opposite to the healthier parent—viz., males.

Again, rapid childbearing we all know enfeebles the mother, and Düsing's observation that the offspring from such cases is most frequently female may be explained on similar grounds—that in these instances the mother, between her rapidly succeeding pregnancies, is allowed no opportunity of recovering strength, and consequently the father becomes the healthier parent.

Thury (1863) and Düsing both show that an ovum fertilised soon after its liberation tends to produce a female, while one fertilised later on, produced a male. In this instance, again, I hold that the female, weakened, as a great majority of women undoubtedly are, by her menstruation, becomes for the time the less healthy parent, and if impregnated at that moment bears a female accordingly, while later on in the month the reverse condition may hold good.

Starkweather, in 1883, suggests "that sex is determined by the superior parent." As factors in such "superiority," he takes cerebral development and activity, temperament, state of health, state of nutrition, &c.; but, as Geddes and Thompson (*op. cit.*) say, "The arguments adduced in favour of this view are scarcely worthy of it, since, save a chapter of pseudo-physiological discussion of vital forces and polarities, of superiority—nervous, electrical—&c., they rest mainly on the vague and shifting grounds of physiognomy and temperament."

Lastly, in 1898, Schenck, of Vienna, put before the world a theory that the urine of a woman bearing only female children invariably contained a larger or smaller quantity of sugar. He claimed that he was able, by appropriate treatment, not only to eliminate this condition, but also, as a consequence, to ensure her, while under such treatment, becoming in her next pregnancy the mother of a male child. I grant that he is correct, both in his discovery and in the soundness of his observations thereon; but I again claim that his work is but a narrow illustration of the wider theory which I uphold, for is not his glycosuric mother the less healthy parent, and, as a consequence, only able to give birth to female children, and does he not, when removing this pathological condition, cause her to become, for the time being, the healthier parent, and so capable of procreating the opposite sex? I know of no more striking illustration for my purpose than this discovery of Professor Schenck.

Finally, I find here in Australia that the same law still holds good that I observed in the slums of Dublin—viz., "The healthier parent procreates children of the opposite sex"; and I place, accordingly, these notes before you to-day for what they are worth, and with the hope that, by so doing, I may interest some of my practical brother-workers amongst the mysteries of Nature sufficiently to induce them to pursue the subject upon the same lines that I have travelled. I myself firmly believe that the broad principle upon which my experiments are based is the correct one to follow. Most observations upon such a subject must of necsssity be empirical, but the corollary I have attempted to deduce appears to me to be rational, and the proposition as a whole to be tenable.

I therefore reassert confidently my axiom, "The sex of the child is determined at the moment of conception, and is the opposite to whichever parent is at that moment in relatively the more vigorous health." At this stage, however, I trust I may go further, and add, as I have endeavoured to-day to prove to you to be possible, that, "in nearly every instance, this essential condition of relative health may be, for the time, swayed in one direction or the other, at the will of the physician, sufficiently to determine whichsoever sex he may desire to produce."

I have avoided, as I stated in the beginning of my paper, touching upon the physiological processes of ovulation, impregnation, or conception. What may be the nature of that impulse which decides, at the moment of launching the spermatozoon on its mission, as to whether its product of union with the ovum is to become male or female—whether we may classify that impulse as nervous, chemical or otherwise—concerns me not. It is sufficient for my purpose that it exists. I am satisfied that we, as practical physicians, in having the power and means at our command of temporarily adding to or taking from human beings more or less of their health, have thereby that impulse within our control. I am content to let my more scientific and specialising brethren investigate its nature, and elucidate its mysteries, in the physiological or chemical laboratory.

I desire that these few notes should be accepted in the spirit in which they have been written—as a poor effort to deal practically with a practical subject; and if they may, gentlemen, have interested you, or in the future prove in any way to have helped to unravel one of Nature's most jealously guarded secrets, then my object shall have been attained.

BUBONIC PLAGUE: ONE OF THE EARLIEST RECORDED EPIDEMICS (1141 B.C.).

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In a few of the many articles on plague which have recently appeared in the medical Press, there has been reference to certain passages in Holy Writ which contain a description of what has been regarded as an epidemic of bubonic plague. Some three years ago we collected notes concerning the history of plague, and, *inter alia*, some upon the passages in question. For various reasons the notes were not published at the time they were written, but we now venture to submit them for your consideration. But before proceeding to discuss the passages themselves, we would call attention to such characteristics of plague as seem to have bearing on their interpretation.

Bubonic plague is a disease which has been known and dreaded for many centuries on account of its tendency to occur in widespread epidemics, causing immense loss of life. Usually these epidemics have appeared quite suddenly, spread like wildfire, killed thousands of men and lower animals, and after a few years disappeared again. But, although attracting special attention only at the times of its epidemic prevalence, the disease does not die out altogether in the intervals. It merely becomes restricted in its incidence to certain areas which constitute its endemic seats or centres, and within which it is constantly in existence. At the present time there appear to be five such centres—one in the north of Africa in the Bengazi district, one in the Azir district of Arabia, one in the mountains of Khurdistan in Persia, one in the Himalayas in India, and one in the Yunnan district of China. All the more recent epidemics have been traced to one or other of these centres; the most recent—that which appeared in China in 1894, and is still prevalent in India, and has spread to Egypt and Portugal and other parts—is said to have had its sources in the Yunnan centre, where plague is known to have recurred every year since 1860.[2]

This last epidemic in India has furnished the opportunity for the study of the disease by modern methods of research. As all the world knows, the specific microbe has been discovered, a method of protective inoculation devised, and two different curative serums prepared. Further, the disease has been found to have at least three clinical varieties—septicæmic, pulmonary, and bubonic—which, however, are usually more or less combined. The general tenor of the reports is to the effect that plague appears as a serious disease, characterised at times by very rapid prostration, collapse, coma, and death within twenty-four hours of the onset;[3] at others, enduring longer, in which case the classical buboes are apt to appear. The buboes are usually to be found in the groins, but may occur in the armpits, along the neck, and elsewhere. The other striking feature of the disease is hæmorrhage, which may take place from all the orifices of the body—nose, mouth, bowel, and bladder, or beneath the skin, or internally.[4] The epidemic fatality is excessive, from 50 to 90 per cent. of attacks proving fatal.[5]

Epizootics among cattle, sheep, mice, and rats, precedent to, or coincident with, the outbreak of the disease in man, are recorded in many of the older reports, often under the designation of "murrain." Recent accounts contain the same statements.[6] Observations made during the present epidemic in India have shown that the disease in rats is due to the same cause as in man—viz., infection with *bacillus pestis bubonicæ* of Kitasato.[7]

Briefly, then, plague presents itself as a very severe disease, more or less sudden in onset, attacking both men and lower animals, having a high fatality, and in man specially characterised by bubonic swellings and hæmorrhages.

Now, these are the symptoms which have been associated with plague from time immemorial. In comparing the features of the present epidemic with those described by the earliest writers, one cannot fail to be struck by the remarkable fixity of type displayed by plague throughout past ages. The definition of plague given in the last edition of Quain's Dictionary of Medicine, published in 1894, refers to it as a "specific fever, attended by bubo of the inguinal glands, and occasionally by carbuncle." A description written in 280 B.C. refers to it as "pestilentes bubones, maxime lethales et acuti."[8] Intervening between these two, there is a whole series of accounts in which there is always the same record of a serious epidemic disease, with buboes and sometimes hæmorrhages. The writings of Defoe,[9] Sydenham,[10] and Thomas Lodge[11] have made us familiar with the disease as it appeared in the 17th century. From the 14th century come the numerous records of the terrible "Black Death," which is said to have killed 25 millions of people,[12] and of which Giovanni Boccaccio has left us so excellent a description in his introduction to the Decameron. From the 6th century we have many accounts of the epidemic which overran the Roman Empire in Justinian's time,[13] and which is said to have "depopulated towns, turned the country into desert, and made the habitations of men to become the haunts of wild beasts."[14] The frequent use of such terms as "pestis inguinaria," "glandularia," "clades inguinaria," "pestis bubonica," in the chronicles of the period, sufficiently indicate the nature of the disease. This 6th century epidemic appears to mark the first visitation of the disease to Europe, but plague was known to exist in Egypt long before this time. Through the writings of Oribasius, physician to the Emperor Julian, there has come down to us a description of plague by Rufus of Ephesus, based on observations made by two physicians, Dioscorides and Poseidonius, who lived during the 1st century A.D. Rufus also mentions that the disease was well known to the contemporaries of Dionysius, as existent in Egypt, Lybia, and Syria in their times, the 3rd century B.C.[15]

According to Hirsch, the distinguished epidemiologist, there are no certain references to plague in earlier medical or historical writings. He considers the mention of buboes by Hippocrates as too indefinite to merit reliance. It may be taken, therefore, that the authentic record of plague has not been traced back beyond 300 years before the time of Christ.

The passages in the Scripture, mentioned at the outset of this paper, appear to signalise a still earlier epidemic. They occur in the First Book of Samuel (ch. iv. to vi.), and narrate the defeat of the Israelites and the capture of the Ark of the Covenant by the Philistines at Ebenezer in about the year 1141 B.C., and they tell of the events that occurred subsequent to the arrival of the Ark in Philistia. The Ark was first taken to Ashdod, and to its presence in that city was attributed the severe illness that broke out amongst the people:

"The hand of the Lord was heavy upon them of Ashdod, and He destroyed them and smote them with emerods, even Ashdod and the coasts thereof" (ch. v., 6). Terrified by this disaster, the people of Ashdod sent the Ark to Gath, and here also the disease appeared : "The hand of the Lord was against the city with a very great destruction, and He smote the men of the city, both great and small, and they have emerods in their secret parts" (v. 9). The Ark seems to have been afterwards carried from place to place until it finally reached Ekron, and here again we are told : "There was a deadly destruction throughout all the city, . . . and the men that died not were smitten with the emerods, and the cry of the city went up to heaven" (ch. v., 11, 12). After seven months' experience of "destruction" and "emerods," the Philistines decided to send away the Ark, to the presence of which amongst them they attributed their illness. Accordingly the services of "priests" and "diviners" were requisitioned to decide as to the best means of restoring the Ark to the Israelites. They advised that, in addition to the Ark, a trespass offering should be sent, to consist of "images of your emerods and of your mice that mar the land." This advice was followed, and the Ark, together with the images, went back to Israel on a new cart drawn by two milch kine. The Philistines accompanied the Ark on its journey as far as the borders of Bethshemish, where it arrived at the time when "they of Bethshemish were reaping their wheat harvest" (ch. vi., 13). The Israelites took down the Ark and the coffer, and "clave the wood of the cart and offered the kine as a burnt offering unto the Lord" (ch. vi., 14). Amongst the people of Bethshemish there followed a severe disease of which many died, but no particulars of its nature are given. In response to a message, men came from the neighbouring city of Kirjath Jearim, and conveyed the Ark to the house of one Abinadab, where it was kept under special guardianship for twenty years.

The accounts of these events in the LXX. (Septuagint) and in the Authorised and Revised versions are practically identical, but in the revised version the word "tumour" is used instead of "emerods," and a marginal note adds "or plague boils, as read by the Jews 'emerods.'" Josephus[16] refers to the epidemic amongst the Philistines as "a very destructive disease," "a sore distemper that brought death upon them suddenly," for the people "died of dysentery and flux," and before death "brought up their entrails and vomited up what they had eaten and what was entirely corrupted by the disease." Thomas Lodge, in his translation of "Josephus,"[17] says, "The people being suddenly taken with the flux died in great torment, and some of them vomited up their bowels being corrupted and corroded with the disease." Both translations refer to the mice as destroying the crops, and to the images sent with the Ark to Bethshemish.

The medical evidence furnished by these accounts indicates that the Philistines suffered from an outbreak of epidemic disease. This disease was of sudden onset and marked severity, for it "smote" men with "a very great destruction." It was of wide and rapid spread, for in seven months (four according to Josephus) it became so disseminated as to create public alarm. A special assembly of priests and diviners was convened to deal with it, and the Philistines were induced to part with a war trophy—the Ark—on which they must have set a very high value. The physical symptoms—(i.) emerods in secret parts, (ii.) vomiting of foul material, and (iii.) dysentery or flux—need some discussion.

(i.) The word "emerod" has usually been taken to mean hæmorrhoids—that is, piles—but this has been contested. It is to be noted that the original connotation of the Greek word is not "piles," but "a flow of blood," just, indeed, what Josephus may be supposed to mean by "dysentery or flux." It is significant that Josephus makes no mention of emerods, but only of dysentery—that is, he gives the word its correct Greek interpretation. Nevertheless, emerods must mean tumours or swellings, since the Philistines were able to "make images of their emerods." Dr. William Smith[18] states that the word has affinities with Hebrew roots signifying "anus" or "nates" (secret parts), and in his discussion of the affliction of the Philistines he comes to the conclusion that "some morbid swelling seems to be the most probable nature of the disease." In default of any better explanation, he appears to have accepted the conjecture that "hæmorrhoidal tumours" are meant. He mentions that Wundebar refers to a bloodless kind of emerod distinguished by the Talmudists as very dangerous, and this kind he (Wundebar) supposed to be referred to in the description of the disease in the First Book of Samuel. But the fact is the word "emerod" is exactly comparable to our own word "tumour" in that it has no exact significance. It has already been mentioned that the revised version has tumour or plague boil instead of emerod. The Rev. W. G. Maconochie, M.A.,[32] informs us that the Hebrew word occurring in the description in Samuel is "Ophel," which originally meant a hillock, but has acquired a wider sense, and has amongst others been used to designate hernia Consequently it might well have been applied to bubonic swelling in the groin, which resembles hernia in appearance. The remarks of Dr. William Smith concerning the affinities of the word with others signifying nates lend every support to this view. The "secret parts," therefore, were probably the nates or groins, the commonest sites for plague buboes. From these considerations we think it may fairly be inferred that the "emerods" were really the glandular buboes of plague, as suggested by Professor Kirkpatrick, of Cambridge.[19] Further, Professor George Adam Smith, D.D., of Glasgow, states that the word used means swellings or boils. In recent instances of the pestilence the tumours have been single and inguinal. In India a local name for the plague is "the boil."[20] Professor Cosh[30] writes, "The Hebrew words used to designate the disease which broke out among the Philistines after they had taken the Ark of Jehovah are *ŏphel*, and its plural form *ŏphālîm*." In the singular *ŏphel* means something swollen; in the plural it is used as the designation of the disease in question (I. Sam., v. 6, 9, 12; vi., 4-5; Deut. xxviii., 27), and is understood to denote a swelling of some sort, a tumour or boil, or tumours or boils. In later Hebrew this word (*ŏphālîm*) seems to have been applied only to hæmorrhoids, and to have become a vulgar word; and so the Massoretes directed that in reading the passages where it occurred the word *tehōrîm* should be substituted for it. As there is no other word extant connected with this stem, it is impossible to say exactly what it meant. Cognate words in cognate dialects indicate that it conveyed the idea of a burning sensation, or of a pressing with violent effort, as in tenesmus. Whatever the specific nature of the disease may have been, evidently it was (*a*) a loathsome disease, for it is classed with other diseases of a loathsome kind (Deut. xxviii., 27); (*b*) a virulent disease, for "there was deadly destruction throughout the city," and very many suffered from it (I. Sam. v., 11-12); and (*c*) it had some noteworthy symptom, for they made golden images of it. Professor H. P. Smith says: "We can hardly go astray in seeing in it a description of the bubonic plague" (Intern. Com. on Sam., p. 40). Professor Beecher says: "The evidence indicates some form of dysenteric or typhoid disease in which a loathsome rectal protrusion was a prominent symptom" (Dict. of the Bible, vol. i., p. 700).

(ii.) The vomiting of "corrupted or corroded matter" denotes hæmatemesis. The vomit of plague contains altered blood ("black vomit"), and is said to have an extremely foul odour, so that it might justly be referred to as "corrupted matter," this expression being in common use amongst older writers to indicate hæmatemesis.

(iii.) By "dysentery or flux" is meant the passage of blood by the bowels. The descriptions inform us, therefore, that hæmorrhage occurred both per os and per anum, a condition of affairs characteristic of plague.

A still further point in favour of plague is *the season of the year* at which the epidemic occurred. This is fixed by the mention of the arrival of the Ark at Bethshemish at the time of the wheat harvest, which in Palestine is long, and in Philistia is from April to June.[21] The disease had prevailed amongst the Philistines for the previous six or seven months—that is, from November or December till May or June. The plague season in Egypt, Syria, and Asia Minor generally is from November to June or July.[22] Consequently the epidemic in question was incident on the Philistines at the time of the regular plague season.

An interesting point is the mention of mice in relation to the epidemic. There is only one such reference—viz, that which occurs suddenly and unexpectedly in connection with the images to be sent with the Ark. The expression is "mice that mar the land" ("earth," as Principal Kinross[51] would also translate), and it has usually been assumed that this indicates the destruction of crops, fruit, &c., by mice in large numbers.[23] We know that mice can so destroy crops, but it has been questioned whether this is what is meant by the reference. Professor Cosh[20] writes: "With reference to the 'mice,' the word (^{c}ak-l^{e}re) is always translated 'mice,' and never 'rats.' Etymologically, it denotes a 'burrowing animal.' The kind of mouse specially denoted by it is the 'field mouse.' (i.) In the description given of the *plague* itself in the Hebrew Bible no mention is made of *mice.* It is only when we come to the question as to the offering to be made in expiation of the wrong done to Jehovah by the detention of the Ark that mention is made of mice. The offering prescribed was five golden *ŏphālîm* and five golden 'mice.' If the Hebrew text represents the correct form of the text, then the inference is that there was only *one* plague—viz., that of the *ŏphālîm*—and that there was a double offering: one (the mice) as the general emblem of a pestilence destroying the land, and the other (the *ŏphālîm*) as the representation of the particular form in which this particular pestilence had manifested itself. This is the opinion of Wellhausen, and it is favoured by Driver and H. P. Smith. (ii.) In the description given of the plague in the LXX. the mention of mice in vi., 4, is preceded by two notices describing a plague of mice in the country (v., 6, vi., 1); and similarly in v., 4, 5, in the LXX. separate mention is made of the golden *ŏphālîm* and the golden mice. If these additions of the LXX. represent the correct text, then the inference is that there were two plagues occurring simultaneously—the *ŏphālîm* in the city, and the mice in the country; that both were manifestations of the Divine hand against the people for one and the same cause; and that the twofold offering had reference to both. But some eminent scholars think that the additions of the LXX. are interpolations of a later hand. There is nothing in the terms used to indicate that 'their land' meant the 'country' as distinguished from the 'city.' It is not at all probable that it meant the 'ground' or 'soil.'" Amongst the many alternative suggestions, that of Professor Kirkpatrick seems the most reasonable—viz., that the mention of mice is merely *symbolic* of the destructive disease attacking the Philistines, since the mouse was the Egyptian symbol of destruction. But he admits the probability that there may have been a destruction of crops by mice. In view of all considerations we venture to submit another interpretation of the reference to mice. May it not be that the mice did not "mar the land" by destroying crops, but by their dead bodies and attendant putrefaction; that they did not constitute a plague, but were killed by *one* in numbers such as to excite comment? The significance of this comment not being understood by the earlier scribes and translators was wrongly interpreted, and the mistake handed down through subsequent ages. The now well-known association of great fatality amongst rats and mice in plague epidemics suggests this idea as a feasible one. Muir and Ritchie[24] state that mice are susceptible to plague inoculation, and die in from one to five days. They also state that mice can be infected by feeding upon pieces of organs from cases of the disease, and animals infected by inoculation can transmit the disease to healthy animals kept along with them. The same bacillus is found in mice dead of plague as is found in man dying from the same cause. Manson[25] quotes several authorities who record that plague affects many of the lower animals, especially rats. The association of images of the mice with images of the diseased parts in men induces the inference that they were fellow-sufferers; and so golden images were made for both after the manner that in subsequent times the Greeks made "anathemata" or "dora" and the Romans made "donaria"[33 34] in similar national or individual extremities and deliverances. Against the idea of destruction of crops there is the fact that there was a harvest to reap in Bethshemish, situated a comparatively short distance off,[26] for it is scarcely likely that had the crops in Philistia been destroyed by mice those of the adjoining Bethshemishites would have escaped. However, we do not wish to press the point of the significance of the mice; it has only been mentioned as a matter of interest in relation to the subject.

On the evidence presented, it may be said in brief that the Philistian epidemic presents the usual features of plague. It was widely and rapidly disseminated, of sudden onset, great severity, high fatality, and characterised by buboes and hæmorrhages. The correspondence with other descriptions seem to show clearly and convincingly that the epidemic described in I. Samuel was true bubonic plague.

As to its source, nothing is or can be known, but the location near endemic centres, the great trade routes through, and commercial importance of, Philistia were such as might easily have led to its importation. It is not difficult to understand that it would spread, but how is uncertain. Even apart from the superstition of the period, the Ark or some of its draperies, or some of the persons who accompanied it on its journeyings—in short, soldiers returning with spoils of war from the campaign, travellers, &c.—may have been the means of conveyance of the infection. But it may have been spread in other ways of which no notice would have been taken at the time, and indeed we cannot even now boast of being informed as to all the ways in which plague is transported from place to place. There is no record as to its termination; the Philistines drop out of the narrative on the restoration of the Ark to Israel. That the disease, though introduced, did not spread amongst the Israelites is not surprising. The Bethshemishites practised, albeit inadvertently, the best possible means of disinfection when they burnt the cart and oxen. The dread of pollution from a corpse[27] would render them most careful in dealing with the bodies, clothing, &c., of those who died. The Ark itself was put into seclusion (isolated) for twenty years at least,[28] and was no doubt handled most circumspectly by the men who conveyed it to Kirjath-Jearim. These circumstances may have saved the Israelites from the disease, but in any case the end of the plague season was near, and the disease would tend to subside naturally. Its cessation is only what might have been anticipated.

The assigned date of the epidemic, according to the figures made use of by the authorised version, is 1141 B.C., and there is every reason for believing that this date is at least approximately accurate. Consequently, there appears to be contained in the few chapters of I. Samuel an account of an epidemic of bubonic plague that occurred over 3,000 years ago, or over 800 years previous to the hitherto accepted earliest record.

Before concluding, we may call attention to a still earlier mention of emerods in the Book of Deuteronomy (ch. xxviii., 27), where it is included by Moses amongst the curses for disobedience. The verse reads: "The Lord will smite thee with the botch of Egypt, and with the emerods, and with the scab, and with the itch, whereof thou canst not be healed." This language indicates that the "emerods" was a disease already well known in the time of

Moses, and that it was a very serious disease, and to be dreaded as bubonic plague has always been dreaded. Hence it is possible, though it cannot be certainly stated, that emerods here also means bubonic plague. If it be so, then the history can be carried back to 4,000 years ago, at which time even it was sufficiently well known to be made use of by Moses as a menace, the nature of which could be understood by the unlearned populace. The disease may have been a familiar one in Egypt before this, and research may yet reveal still earlier records of it. Some of the celebrated "plagues of Egypt" may possibly have been due to the *bacillus pestis bubonicæ.*

However, the present paper claims to deal only with the epidemic of 1141 B.C., and to set forth the reasons which indicate that it was bubonic plague. The evidence to this effect is at least as good and valuable as much of that on which the history has been traced to 300 B.C. There is nothing, in either case, which can be subjected to the rigid test of observation, which alone is certain. Nevertheless, we believe that the conclusion formed is justifiable, feeling with Sir William Gowers that "Where we have no certainty, we must be content with probability, or relinquish all attempts to know."[29]

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SOME NOTES ON EMPYEMA.

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To discuss at all fully such a large subject as empyema in the short space at one's disposal here is impossible, so I propose to draw attention principally to one rather unusual feature which has occurred in some of my patients.

The first case was that of a boy of about thirteen, who five years ago appeared amongst my out-patients at the Melbourne Hospital, and who apparently had had for some weeks an empyema, occupying about half of the right pleural cavity. I at once told his mother that he should be operated upon, but she strenuously objected, saying that they had told her the same thing at the Children's Hospital, and at the same time asking for a tonic. I gave him some mild tonic, impressing upon her the absolute necessity of operation before long. In less than a week she reappeared with the boy, who was evidently much better, while she was radiant with pleasure. She said that after the first dose of my medicine he had a violent fit of coughing, and brought up about half-a-pint of matter, after which he was very much better and had continued to improve since. Needless to say, she thought the medicine marvellous. Recovery was afterwards quite uninterrupted.

The next case was that of a man, aet. thirty-five. He had been in the hospital under the care of a colleague for three weeks or so before I took charge. When doing so, I was told that, a couple of days before, he had, after a violent fit of coughing, brought up about a pint of highly offensive pus. The physical signs were those of a fairly large encysted empyema in the upper and anterior part of the left chest. For about a month afterwards he continued to expectorate more or less foetid pus, in gradually diminishing quantities, at first about a pint a day, latterly a few ounces. Despite the intense fœtor, his general condition was good, temperature and pulse practically normal, and he ate and slept well, so gangrene of the lung could be fairly eliminated. When he left the hospital he was practically well.

The third case was that of a little friend of mine, aet. four, who, at the end of November, 1898, contracted a croupous pneumonia of the right base. In about ten days, temperature fell from 103° to 101°, but did not go any lower; the dulness persisted and increased, and vocal resonance and fremitus and the respiratory murmurs all became markedly diminished. For ten days or so I considered the effusion a simple serous one; and as the dyspnœa was not great, and the dulness not higher than the fourth rib, and the patient's general condition fairly good, I did not even suggest the use of the exploring needle; then, as there was no sign of diminution of the effusion, and as he began to have suspicious perspirations and be generally worse, I began to seriously suspect pus, and wished to use an exploring needle; but the mere suggestion almost sent his mother, who was intensely neurotic and was acting as nurse, into a fit. I did not insist, and, as it happened, no harm was done; but, all the same, it was not a wise course to pursue. After the date, when I first broached the subject of tapping, about ten days elapsed, during which his condition was that of a patient with a moderately large empyema. Then, after a violent fit of coughing, he brought up about 4 oz. of pus. After that he was somewhat better, and for a fortnight he expectorated daily between 1 oz. and 2 oz. of pus. His general condition gradually improved, but to my mind it was hardly satisfactory. I thought that drainage from below by an operation was indicated to hasten matters, but Dr. Stawell, who then saw him in consultation with me, thought it as well to let him alone, so nothing surgical was done, and recovery slowly went on for another month. Then he went away for a change, and improved rapidly. Now he is, as far as appearances go, perfectly well; I fancy that the right pleura, however, is a good deal thickened.

The fourth case was that of a female teacher, aet. twenty-one, who was admitted on 5th May, 1899. She had been in bed for a fortnight, feverish, and suffering from pain in the left side; no shivers, but cough and expectoration of blood-stained mucus, with a few clots. Temperature was 102°, and respirations 30, but pulse was 125. The physical signs were those of pleuro-pneumonia of the left side; the vertical dulness nowhere exceeded 3 inches, and the heart was not displaced. No tubercle bacilli were found on repeated examination of sputum.

During the next five days temperature kept about 102°, dropping to subnormal twice, but there were no sweats, and pulse got a little better. On the fourth day after admission it was noted, however, that the respirations were 60, while the pulse was 120. With that exception, the pulse-respiration ratio was practically normal all through. On 10th May, five days after admission, she coughed up 30 oz. of pus. There was no blood mixed with it, and no signs of hydatid. Owing to some misunderstanding, no bacteriological examination was made, although I had given special instructions on the subject. The subsequent course of the case is uneventful. On only one occasion after the perforation did the temperature rise to 101°. After that, recovery was uninterrupted, and she is now perfectly well.

The principal characteristic of these cases is the fact that the pus was evacuated through the bronchi.

But in addition there is the fact that in not one was there the pain and distress (characteristic of pyo-pneumothorax) present either at or after the time of the perforation.

Paget, referring to perforating cases, says: "These *disasters* will become less common in proportion as our hospital patients or their parents become less neglectful of the early stages of the disease, and in proportion as the early diagnosis and treatment of empyema become universal." Well, there was so little of the disastrous element in the results of these cases that if we could always guarantee like results there would be no operations for empyema. But, of course, we cannot guarantee anything of the sort, and so, generally speaking, it is not wise to wait for perforation.

It may be noted that these were all acute empyemata, and there were no very alarming symptoms; in fact, just the class of cases in which one, at any rate, would hesitate about urging a serious operation, such as excision of rib and opening of pleural cavity implies. Personally, I think that a good many of the empyemata of young children can be cured by aspiration alone. Of this I had a very good example recently in a little boy of about five, who had such marked signs and symptoms that I urged surgical interference, at the same time suggesting, on account of his low condition, a preliminary aspiration, to be followed in a few days (if no improvement resulted) by excision of the rib. About 8 oz. of pus were evacuated, and rapid recovery followed.

In that case alone did I succeed in having an accurate bacteriological examination made, and then the only micro-organisms found were staphylococci.

With reference to absence of pyo-pneumothorax, Paget suggests that there are adhesions at the point of communuication, or the lung tissue is soaked like a sponge with the pus, and this saturation of the patch around the opening is increased by the patient's coughing, so that the small bronchi are compressed.

THE CLINICAL VALUE OF THE SERUM REACTION IN TYPHOID, TOGETHER WITH SOME NOTES ON THE USE OF DEAD CULTURES AND ON ANTI-TYPHOID INOCULATIONS.

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As this paper was written for a joint meeting of the Sections of Pathology and Medicine, we would beg the permission of the President to discuss not only the clinical value of the Widal reaction in the diagnosis of typhoid, but we would add a few sentences on its probable future clinical value in ways that are but recently developed, and some of which are hardly beyond the stage of laboratory speculation and experiment.

After an almost constant daily use for nearly two years of the serum test for typhoid, one is driven to the conclusion that it is not only a most valuable aid in the diagnosis, but the most trustworthy of all indications, and of far more definite value than any one clinical sign. To some this may appear to be placing too high a value upon its virtues, but it is not higher than we have been led by our own personal experience to think it deserves. In the majority of cases it, to use our worthy President's simile, is the coping stone of the diagnosis, but in a few cases it is, we venture to say, the very foundation stone itself.

The best are not perfect, and we would not claim perfection for the test. None are better acquainted with its faults than those who use it frequently. But its faults are few and far between, and they gradually become fewer and fewer the more experience and practice the experimenter has, and we must also remember that in using the test we are translating a new language whose every nice distinction is not yet understood. After allowing for all errors, there are no doubt a few cases left over in which the reaction does appear to fall short, but these are not more than 3 per cent. How often are the classical clinical signs of enteric wanting? The spleen is not always enlarged, and if it be enlarged the increase is frequently, as seen in *post-mortem* examination, of comparative insignificance. Or it may be that the patient may come, as one actually did, complaining of malarial attacks, and then how little reliance can be put on the size of the spleen. He had come from New Guinea and the adjoining islands, and his statement was that he had been suffering from malarial attacks for some weeks. On admission the temperature was under 100°, and the patient appeared to be just coming out of a malarial attack. For six days the temperature remained normal, and then rose in forty-eight hours to 104°. The spleen had been no guide, and now the *temperature* was a guide in the wrong direction, for, if such an authority as Wunderlich is to be believed, any fever which reaches 104° by the second day is not enteric, whatever else it be. Yet this patient did have enteric, and the typhoid bacilli indicated by their clumping, &c., that they, if nobody else, were well aware of the true nature of the infection. The patient doubtless had malaria *and* typhoid, and the two coming together confused the clinical phenomena. This case is of especial interest, because the two diseases have been so frequently confused in the diagnosis. Would it not be a difficult task under our clinical methods alone to diagnose, not malaria *or* typhoid, but the two together? Cabot, writing quite recently in connection with the late war in Cuba, says:—"At the hospital in Porto Rico, I found several hundred fever cases, almost all of whom were receiving large doses of quinine, because the diagnosis between typhoid and malaria had not been clearly established, owing to the entire lack of any materials for performing the serum reaction for typhoid. I examined 177 cases at this hospital, and found not a *single* case of malaria among them, while 90 per cent. of those examined showed prompt and intense serum reaction for typhoid." This evidence was, together with that previously obtained by the pathologist, sufficient to check the wholesale administration of quinine, and indicate which of the cases needed typhoid diet.

And such a case as we have given *in extenso* is but typical of many another. It would be an easy matter to select other cases of typhoid, or of cases resembling typhoid and yet not typhoid—it would be an easy matter, we maintain, to show how the ordinary clinical signs were wanting, or, worse still, misleading. But a recital of so much detail would be wearisome, and it is, perhaps, better for that reason to generalise. It could be shown how the test had actually proved of the greatest value in cases of general tuberculosis, and although the Widal reaction will not save the life of a person suffering from a general tubercular dissemination, yet it may prevent the medical attendant from making a favourable prognosis, and it may save the latter much time and many an anxious thought by settling the diagnosis.

We would but mention, and only mention, a few cases in which the test was very valuable :—

Appendicitis complicated by a pyaemic abscess in the liver. The diagnosis was enteric, but for four weeks the Widal test gave no reaction.

Acute Beri-Beri.—The patient was a Chinaman and unable to give any history, and at first sight looked like an enteric.

Acute Cerebro-Spinal Meningitis in an hospital attendant who might reasonably be supposed to be exposed to infection from enteric dejecta.

Another case of malarial infection, in whom the malarial organism was found, but for some unknown reason the patient passed some blood by the bowel.

A drunken Sydney Domain loafer treated for alcoholic gastritis. The patient cut short his troubles with a good dose of arsenic. The bowel showed that he was really convalescing from enteric.

An ordinary cut throat, upon whom all the stigma of suicide was thrown, but who was really suffering from typhoid delirium.

Add to all the ordinary difficulties of a case of enteric the fact that, after all, the typhoid germ may not be the only organism causing symptoms. It may be, as mentioned, a malarial parasite complicating matters, or, more difficult still, the germ of influenza. Of none of these difficulties does the Widal reaction take any apparent cognisance. If there be a malarial parasite attacking a red corpuscle, still the surrounding serum will cause the clumping of the bacilli, and although there may be evidence of a consolidation of the lung and the patient is struggling against a pneumonia, yet, behind it all, the typhoid germs declare by their clumping reaction that their own brethren are at the bottom of all the trouble, and causing a typhoid pneumonia, or are working for evil in partnership with Fränkel's pneumococcus. Conditions which require a keener sense of discernment than we are endowed with, however accurate observers we may be, to a typhoid bacillus may be his very alphabet.

After all, should not a typhoid bacillus be a most excellent connoisseur of the blood of a typhoid patient, and who should be a keener observer than he whose very existence and increase depend wholly on some occult qualities in the blood, which faculties largely determine whether he or the patient is to get the upper hand. The test is but the application of a very general principle : where man's powers of perception cease he devises means of seeing beyond—the microscope and camera to see what the eye cannot, and the magnet to feel what is intangible.

There are certainly some cases in which the reaction appears to fail, but, with a more perfect manipulation of the test, the exceptions are gradually being reduced. In a delicate physical or chemical quantitative experiment, even where the finest balances are used, allowance must be made for experimental error, and surely the same must be allowed to the bacteriologist. Perhaps, after all, experimental error is allowed, for there may be a residuum left, which can only be accounted for by supposing that there are anomalous cases. As in a delicate quantitative analysis we require a perfect balance protected from dust and draughts, so we reduce our experimental error in Widal work : we require as a *sine quâ non* a good culture—a culture which is exceedingly sensitive to the agglutinating and clumping quality produced in the blood of various enteric cases.

The other points to which the serum reaction points in clinical work are many and important, but would come more suitably under the head of the pathological section, as at present they are too experimental. Not least important amongst these would be the interesting subject of post-typhoid reaction. Another most important point is the extension of the Widal reaction to the examination of the blood of patients who have been inoculated against typhoid. To this we have referred more fully later on. Yet another indication of the serum reaction may be its use in prognosis. But this also is quite experimental. However, it may be worth mentioning that cases are on record where relapse has occurred again and again (four times), and it was not till the blood gave a lasting positive Widal reaction that the relapse ceased to appear.

The test may be of use in giving a retrospective diagnosis. E. J., a patient, has had an ill-defined disease which was undiagnosed; later he developes bone lesions or some other pathological condition which might readily be put down to Tubercle or Syphilis. A Widal reaction would point to the almost certain conclusion that the lesion was a post-typhoidal one, and if the pus were examined the typhoid organism might be found. These few fragmentary remarks are in no way complete, and do not touch on many of the more important points which could be more suitably brought before a purely bacteriological Section.

The Widal reaction is as yet in its infancy, and a few years hence it will have a much more extended field—for the principle on which it is based is general and coming into prominence in other forms of disease.

THE EMPLOYMENT OF DEAD BACTERIA IN THE SERUM DIAGNOSIS OF TYPHOID FEVER.

Soon after the appearance of the contribution on this subject by Professor A. E. Wright, of Netley, in 1897, one of us exhibited examples of the method before the Medical Section of the Royal Society of New South Wales.

Widal showed in 1896 that the agglomeration, which takes place when the diluted serum of a patient suffering from typhoid fever is brought in contact with a living culture of typhoid bacilli, is obtained when the serum of the patient is mixed with a culture of bacilli which has been killed by exposing it to a temperature of 60° C.

The culture was prepared as follows:—Fresh agar cultures in test tubes were emulsified, or the culture was washed off the surface of the agar with sterilised normal salt solution, and small glass capsules were filled with the emulsion and sealed, and then exposed to a temperature of 60° C. for ten minutes in a water bath.

The cultures thus killed by heating do not differ very much when examined with the microscope from the living cultures. The movements of the bacilli are much the same, except that those in the cultures, subjected to a temperature of 60° C., have no progressive movement and do not advance across the field. The heated cultures will not grow, nor does agglutination of the bacilli take place spontaneously. The cultures prepared in this way were used for the purpose of diagnosis exactly as the live cultures. We now prepare the dead cultures by cultivating on bouillon and heating to 60° C. for ten minutes before filling the glass capsules, which are afterwards exposed to the same temperature. This we have done to prevent the possible dissemination of living bacilli.

The results obtained both by the agglomeration observed with the microscope in the usual way, and by the use of the capillary sero-sedimentation tubes, do not differ from those observed when the live culture was employed. Before use it is always advisable to thoroughly shake the dead culture, as on standing for a long time clumps form, but these are very readily broken up by the shaking. The advantages connected with the use of dead cultures for diagnostic purposes are portability and absence of risk of infection. The small glass capsules containing the cultures can be carried about, and occupy but a small space, and, should they get broken, there is no risk of spreading the disease.

Living cultures require to be freshly prepared, and sometimes it is difficult to keep up a succession of fresh cultures. The dead cultures, when enclosed in the capsules, will keep for a long period. We have found them reliable when two years old.

VACCINATION AGAINST TYPHOID FEVER.

Vaccination was first performed by Professor Wright, at Netley, in 1896, when he vaccinated himself and seventeen military surgeons. The vaccine is a culture of typhoid bacilli in bouillon, which have been grown for twenty-four hours at blood heat. It is then put into capsules and subjected to a temperature of 60° C. for five minutes The sterility of the vaccine is ascertained by allowing a drop of the contents of the capsules to flow out on the surface of an agar tube, which is then incubated. The dose of the vaccine which kills guinea pigs of 100 grammes weight is the dose for a man. It is difficult to kill a guinea pig with a culture of *bacillus typhosus*, and it is necessary to increase the virulence of the culture by passing it through a series of guinea pigs, which must all weigh from 250 to 300 grammes each.

All of the eighteen cases first vaccinated by Wright gave the characteristic sedimentation when the diluted serum of their blood was examined. The sedimentation took place in all cases where the serum was diluted fifty-fold, and in many cases where it had been diluted one hundred-fold. In other words, their blood reacted as if they were suffering from typhoid fever, or had recovered from an attack of that disease.

One of the cases amongst the army medical officers vaccinated by Wright in 1896, two months afterwards, when his blood possessed a sedimentation power of twenty units, suffered no inconveniences when inoculated with a living virulent culture of typhoid bacilli.

During the epidemic of typhoid fever at the Maidstone Asylum, of the 200 attendants, ninety-five were vaccinated, of whom none contracted the fever during the epidemic, while nineteen of the remainder who refused vaccination suffered from the disease.

At Khartoum six out of eight young subalterns were vaccinated and did not contract the fever; both of those who decided to take their chance and were not vaccinated caught typhoid, and one of them died. About 4,000 British soldiers have been vaccinated, and more than half of these are known not to have contracted the disease up to the present time. How many, if any, have suffered from it is not yet known.

The site for the injection of the vaccine is the loin. The patient generally has a feeling of chilliness two or three hours afterwards. There is a slight rise of temperature and restlessness at night; symptoms pass away in twenty-four hours. Sometimes the local tenderness is very great and extends to the armpits and groins, where the glands may be tender and enlarged, with a large patch of congestion around site of inoculation. There are usually constitutional symptoms, some degree of faintness and collapse coming on in three or four hours, sometimes nausea and vomiting.

We have ourselves quite recently experimented with anti-typhoid vaccine, which was made as previously described. The culture used was one of the ordinary laboratory cultures of *bacillus typhosus*. We injected into one of ourselves a certain quantity of the vaccine, and this was followed in a short time by an intense local reaction. By the end of the third hour the site of inoculation had become intensely red, and the area of redness gradually spread until 3 to 4 inches in diameter. The injection was made in the loin, and the lymphatics draining into the axilla and to a less extent into the groin became reddened. There was most marked tenderness at the site of injection, and along the course of the lymphatics and in the glands. The constitutional symptoms were not so marked as the local—a certain amount of discomfort and but very slight temperature. These effects passed off completely in four days. The blood of the experiment did not give a positive Widal reaction, and so the experiment was repeated at the end of the week. The effects of the second injection were the same as those of the first, but still we did not get a clumping of bacilli on examining the blood. Wright apparently accepts the positive Widal reaction as evidence of immunity. If so, then we have failed to procure immunity in ourselves. We do not say that the agglutinating substance in a typhoid patient's blood itself confers immunity, and we do not touch on the difficult question as to what is the nature of the agglutinating substance, but apparently the clumping phenomena and the immunity go together, although possibly the result of different agents. This is suggested by Wright's extensive experiments. In our own case we produced intense local reaction, and a much modified constitutional reaction and no clumping reaction; hence probably no immunity. The negative result was half anticipated, for Dr. Sidney Martin and others have shown that laboratory cultures, as usually obtained from a *post-mortem*, are not, as a rule, especially virulent, and to produce clumping and immunity, in all probability, a high amount of virulence is necessary. Such properties would be possessed by such a culture as has lately been kindly sent to us by Professor Wright, of Netley. It is a culture whose virulence has been enormously raised by its passage through a long series of guinea pigs.

The question of inoculation for typhoid in the near future will, no doubt, assume an important aspect. Why should those exposed to typhoid infection not undergo inoculation, just as most of us are vaccinated against smallpox? We are far more exposed to the former than to the latter. Medical men, nurses, and all hospital attendants could be readily protected by preventive inoculation, and these are the ones who usually receive a large dose of the infection.

DISCUSSION ON THE TREATMENT OF TYPHOID FEVER.

Dr. Hare (Brisbane): There is only one branch of this subject upon which I am at all specially qualified to speak—viz., the treatment of the disease by systematic cold bathing; and even here I have only old facts to produce and old arguments to bring forward. I will, accordingly, be brief, and confine myself to emphasising a few of what seem to me the more important points in this method of treating typhoid. It must be freely admitted that at present no complete theory as to the *rationale* of the treatment is available. My own belief is that it is beneficial in very numerous and diverse directions. I still believe that the reduction of the temperature *per se* is an important item, for many of the symptoms ebb and flow in direct proportion as the temperature falls and rises. That it exerts a tonic and stimulating influence upon the brain and nervous system generally, independent of the reduction of temperature, is patent from the fact that symptoms such as stupor and delirium not infrequently clear up while the temperature remains stationary, or even at times during the period that it is actually rising; that it acts as a powerful brake upon the progressively increasing feebleness of the heart and circulation cannot be disputed by any who have a personal experience of the subject. Recently, the experiments of Roque and Weil seem to make it certain that systematic bathing very greatly increases the rapidity of toxin excretion. Besides the above, there are many other subsidiary actions of the bath, all fortunately tending to the welfare of the typhoid patient, but which would require too much time for their enumeration here. While, however, I believe that all of the above-mentioned actions of the bath go far to explain its beneficial influence, still such belief is by no means essential for the adoption of the system. The case for cold bathing undoubtedly rests upon its results; in the words of Osler, "Brand's treatment has forced its way to recognition by the overwhelming array of figures which can be brought in its favour." During the five years preceding the adoption of cold bathing in the Brisbane Hospital, 1,828 cases of the disease were treated. Of these, 291 died, a death rate of 14·8 per cent. During the succeeding decade, 1,902 were submitted to the cold-bath treatment; of these, 143 died, a death rate of 7·5 per cent. The mortality, therefore, has been practically halved, or, in other words, 138 lives that would previously have been lost have been saved during the ten years included between 1887 and 1896. It has been suggested that the fall in mortality is due to the cessation of immigration into Queensland. Immigration, however, did not cease until the end of 1889, and its influence upon the death rate, assuming such to exist, would naturally persist for some time longer. By that time, however, the reduction of the death rate to 7 per cent. had been already attained, and no further material fall has since occurred. Plainly, therefore, the cessation of immigration as a factor in lessening the case mortality of typhoid in Brisbane must be abandoned. It may still be maintained that, from some unknown causes, the virulence of the disease has abated of late years. This argument, of course, is really a begging of the question, and I can only state that the abatement in question occurred with remarkable suddenness—in point of fact, just about the middle of the year 1887, exactly at the time when the bath method of treatment had been brought up to its full state of efficiency. Again, were there anything in the contention that the type of the disease had become milder, we might expect all the causes of death to have become less frequent. But this is not so. Perforation and hæmorrhage still continue to carry off practically the same number of patients as they did in the old days at the Brisbane Hospital, and as they did in the London Fever Hospital under Murchison. Only those modes of death—viz., the pyrexial, which the bath treatment was designed to avert—have been rendered conspicuously infrequent. One point about Brand's treatment can hardly be too frequently repeated. Its keynote is *prophylaxis;* it is to be regarded as a prophylactic against the consequences of protracted pyrexia. Hence, to obtain the greatest possible benefit from it, it is above all things necessary that it be commenced at the earliest possible period of the fever, before signs of danger have appeared; and in direct proportion as this indication is responded to, so is the success of the treatment. In this respect it differs fundamentally from expectancy, where active interference begins only when special or abnormal symptoms arise. Expectancy, indeed, may be taken to denote all that is most essential to avoid. Those who are not fully imbued with the prophylactic value of the bath, will almost certainly be disappointed with their results. Over and over again physicians have commenced tentatively, bathing their first cases at the later stages of the fever, when most of the damage has become an accomplished fact, with the result of bringing discredit upon the treatment, if not upon themselves. This method of proceeding is as illogical as it would be to limit the use of antitoxin to cases of diphtheria in a state of advanced toxæmia. No attempt at selection was ever made at the Brisbane Hospital. All cases that were admitted on and after the 1st of January, 1887, were submitted to the operation of Brand's rule, and the result was an immediate alteration in the aspect of the wards, so conspicuous as to elicit unsolicited remark from all, even the most inexperienced, who were employed therein. Finally—

(1) The treatment under the most ordinary supervision is absolutely free from risk. During the last two and a-half years no accident, so far as I know, has ever occurred in the administration of the baths, although 40,000 or upwards must have been given.

(2) The treatment is easily carried out both in the hospitals and in the houses of the well-to-do. This in spite of all that has been said to the contrary. In Brisbane, at any rate, there is a considerable number of nurses fully trained in the system and fully competent to carry it out in ordinary cases, with one daily visit from the medical attendant.

(3) The treatment is economical, although in hospitals some extra male attendance is required, yet the slight increase in salaries is more than counterbalanced by the saving in drugs, alcohol, and funeral expenses.

Dr. Pain (Prince Alfred Hospital, Sydney) : Dr. Hare, both in his admirable monograph and the few remarks he has just made, fully justifies the adoption of the cold-bath treatment for typhoid as it is practised in the Brisbane Hospital. The expense of applying the system to all cases of typhoid must not, however, be overlooked; and, where the number of hospital wardsmen is very limited, it would seem that a modification of the system might with advantage be adopted, only those patients whose temperatures resist simpler hydro-therapeutic measures being subjected to the cold bath. In the Prince Alfred Hospital, last year, the death rate was 6·6 per cent., 150 cases being treated. The bath was not used, but cold or tepid sponging was employed. There were only a few cases in which, in my opinion, the bath might have been used with advantage. Drugs of any kind were very rarely used, the only antipyretics being quinine and phenacetin, so that the drug bill was very small, though the cost of the alcohol used was certainly an item for consideration. At one time, an extensive trial was given to Burney Yeo's chlorine mixture, without any beneficial results.

Dr. Nihill (Melbourne) : It has been a great advantage to be afforded such an opportunity as we have had of witnessing the details of the cold-bath treatment in actual operation. Not having had any experience of the system, I am not in a position to express any definite opinion in regard to it. Statistics certainly show that the cold-bath method of treating typhoid has considerably decreased the mortality, and consequently it would appear to be desirable that the system should be given a fair trial in hospitals where it has not yet been adopted. At the Melbourne Hospital the treatment had not yet been tried, and, although I have not come furnished with the statistics, my impression is that the mortality is low, and that those cases that have died would not have received benefit from any system of treatment. An initial dose of calomel I believe to be beneficial, for there is usually a predisposition to constipation. A mistake is sometimes made in not regarding the quantity, as well as the quality, of the food given to typhoid patients, the error being frequently to give too much. Though milk is generally recognised as the most suitable diet, it is not an ideal food. In Melbourne, Dr. Springthorpe introduced sterilised malt extract, which, though it has a marked soothing effect, and induces sleep, causes patients to lose flesh to a very noticeable extent, and makes them more debilitated than is the case where milk diet is adhered to. It is much to be questioned whether very large doses of alcohol are of much value.

Dr. Hirschfeld (Brisbane) : After paying a tribute to the work done by Dr. Hare in introducing systematic cold bathing for the treatment of the fever patients of the Brisbane Hospital, he remarked that he was led to substitute the tepid for the cold bath, not by theoretical considerations, but by his clinical experience, which now embraced between 500 and 600 cases. The first bath was generally given at a temperature of 85° F., and, having found out the individual reaction of the patient, it was either continued at 85° F., or lowered to 80° or increased to 90°; but in rare instances it was found necessary to go below 80°. The duration of the bath was twenty minutes. The effect of the tepid bath upon skin, nervous system, respiratory organs, was very favourable; delirious conditions, except immediately after admission, being conspicuous by their absence. However, he did not intend to deal with this side of the question on the present occasion. The main point of contention was the antipyretic action of the cold and tepid baths respectively. In dealing with this question, it was one-sided to regard only the loss of heat effected by the bathing. The temperature of the body is the result of the production of heat and the dissipation of heat. While admitting that the loss of heat is greater the colder the water employed for bathing, on the other hand, the reaction following upon the bath brings about an increased production of heat, which, according to Kubner's researches, is the greater the colder the water. He quoted the following figures:—

Temp. of Water.	Excess of Production.	Increased Metabolism expressed in Fat.
59°	407 calorics	43 grammes
68°	297 ,,	31 ,,
77°	167 ,,	18 ,,
86°	77 ,,	8 ,,
95°	7 ,,	0·7 ,,

This means that the reactive production of heat after a bath of 59° F. is, roughly speaking, five times greater than after one at 86° F., and consequently the waste of bodily tissue is five times as great. Now, increased production of heat is the very thing to be avoided in the treatment of fever. Correspondingly he had frequently found that a bath of 85° F. brought about a greater fall than one of 80° F. in the temperature of the patient simply because the patient having come out of the warmer bath did not exhibit the same reactive production of heat. Everything depended upon the individual reaction. As the result of many thousands of baths, he was in a position to state that the average reduction of temperature achieved by tepid baths was between 1·5° and 2·0° F. The antipyretic effect of the baths was most marked in the later stages of the illness. In weak people, children, and the aged he advised to start bathing at 90° F. The principal advantages of the tepid bath were—

(1) It is more easily applied, as the patient does not raise as much objection against it as against the cold bath—a matter of very great importance in private practice, where very few patients could be persuaded to submit to systematic cold bathing. The dread of the bath, which, in a few cases, becomes so great as to form an absolute contra-indication against bathing, is much less frequently met with when tepid baths are being employed.

(2) The appearance of untoward symptoms like shivering and cyanosis following after a cold bath is not so often observed after using tepid baths; shivering raises the temperature of the patient again by lessening the skin circulation on the one hand, and stimulating the production of heat on the other.

(3) Tepid baths are alone permissible in patients whose heart has had considerable wear and tear, as we find in patients who are admitted at the end of the second or beginning of the third week, and in children and old people whose hearts are naturally less resistant. All these people are not in a position to support severe refrigeration, as effected by the cold bath.

In conclusion, he wished to sum up his experience by stating that, as long as we had no specific treatment, baths were the most valuable remedial agent employed in the treatment of typhoid fever on account of their favourable effects upon pyrexia, circulation, respiration, and nervous symptoms.

Dr. Wolfhagen (Hobart): Having had a personal experience of a severe attack of typhoid, I can speak from the patient's point of view as well as that of the medical practitioner. My experience and observation convince me that it is of the utmost importance to treat such cases early. I am doubtful as to the advisability of administering calomel, as it has a very irritating effect on the lower bowel of some patients. In certain cases of typhoid, alcohol is undoubtedly an important aid in treatment, but in cases with temperatures not reaching 103°, and especially in young patients, it can well be dispensed with, and milk diet, with a judicious allowance of eggs, can be relied upon. Typhoid, in the Australian colonies, presents much difference to the disease as known in the old country. I cannot speak from experience of the cold-bath method of treatment, but it appears to me to be unsuitable for some of the colonies whose climate is colder than that of Queensland. It has been tried in Tasmania, with bad results. I am also of opinion that it is better to avoid chemical antipyretics. If they had asked me, when suffering from typhoid, to enter a cold or even a tepid bath, I should have cursed the day on which I was born.

Dr. Hunt (Warwick) expressed the opinion that, in the treatment of typhoid, the keeping of the bowels open with mild purgatives during the early stage of the disease was of the very utmost importance. If there were a timely and strict observance of that precaution, he was convinced that the dangers of the disease in its later stages would be considerably lessened.

Dr. Crowther (Hobart) said he thoroughly concurred with Dr. Wolfhagen's opinion that typhoid in the colonies was a different disease to that in the old country. He did not hesitate to say that, so far as Tasmania was concerned, the cold-bath treatment was unsuitable and unnecessary. He certainly agreed with those who advocated the initial dose of calomel.

Dr. Halford (Clermont, Queensland): Whatever Brand did for the cold-bath treatment, Dr. Hare has placed it on a scientific basis. No one has studied it so carefully from the point of view of its advantages and its disadvantages. Having had three years' experience of the method in the Brisbane Hospital, I can emphatically state, without going into details, that the patients received most benefit from the treatment when that was carried out most rigorously. I may state that at first I adopted the method of classification of Dr. Hare, by diagnosing as typhoid those cases that ran a course of ten days or over. Later the method of Widal was used to supplement the diagnosis. With regard to the figures quoted by Dr. Hirschfeld concerning the consumption of calorific material, I must say that they are entirely theoretical in their application. In my opinion Dr. Hare has settled the matter practically. It is admitted that the treatment should be instituted in all hospitals for obstinate pyrexial cases. That expense is practically sufficient to allow of the extension of it to all cases. Calomel is certainly useful in clearing up the diagnosis, and I have not seen it do harm. I favour the employment of antiseptics in the routine treatment of cases, but recommend that the drug should be varied frequently during the course of the disease.

Dr. Anderson (Melbourne) strongly supported the cold-bath system of treatment. Wherever the cold bath had been judiciously used, it had reduced the mortality. Before it was adopted in Brisbane, the mortality was nearly double what it had been since. He saw little difficulty in carrying out the treatment in hospitals, and whatever extra trouble it might give was compensated for by the advantage to the doctor and the benefit to the patient.

Dr. Power (Sydney) considered it advisable to commence treatment with a lukewarm bath, after which the patient was put into bed with a cradle-like contrivance to keep the bedclothes from the body and at the same time provide for the admission of cool air. In winter, it was difficult to persuade the patient to enter any kind of bath. He doubted the advantage of the routine administration of calomel, but regarded alcohol as an indispensable aid in the treatment.

Dr. Scot-Skirving, in summing up the views of the various speakers, said: If we look closely, we will find that, after all, the speakers are pretty well agreed upon most of the points in the treatment of this disease. It is in the matter of hydrotherapy in which some divergence of opinion obtains.

I would like to plead for a somewhat more liberal diet scale in typhoid fever. I have elsewhere published the practice of the Prince Alfred Hospital in this respect. So far as I know, no intestinal accidents or other evils have followed this wider range of diet. Some of the speakers referred to the greater prevalence of constipation in enteric in Australia than in Europe. In Sydney we give an enema in such cases every alternate day. Meteorismus is by no means always due to improper feeding, for my experience leads me to the conclusion that it was not seldom an indication of deep ulceration. It must indeed be rare that any case required a larger dose of alcohol in twenty-four hours than 6 oz. Because a man had fever it was no reason why he should be made intoxicated. As to antipyretic drugs, at Prince Alfred Hospital they were used as seldom as possible—only for an occasional emergency. I have, however, never classed quinine in the same depressant black-list with antifebrin. As to Burney Yeo's mixture of chlorine and quinine, I introduced it into the Prince Alfred Hospital, and have published a series of nearly 200 cases, in which it has been used; a comparison at the same time being made with cases treated on an acid mixture. I came to the conclusion that the quinine and chlorine mixture did no harm, but that it exercised apparently no influence on the progress and result of the case. The possible intestinal antiseptic action was probably not so well obtained as by salol. Turning now to the bath treatment, I must pay a high compliment to Dr. Hare's industry, his fairness, and to the amount he has taught us all on the subject. Almost does Dr. Hare persuade me to become a "bathist." Dr. Hare, I take it, believes that by bathing every case indiscriminately he forestalls and prevents a large or continuous pyrexia, and so obviates the many evils which follow that condition, and he and other speakers point to the greatly diminished mortality under its use. They also say it is simple, easily carried out, not particularly expensive, free of risk, and not very objectionable to the patients. As to the relative merits of cold bathing and the tepid bath, I shall say nothing here. Dr. Hare and Dr. Hirschfeld are apparently at issue on this point. Now, one of the pieces of knowledge a visit to Brisbane is likely to give us is some definite information on this vexed question. Dr. Hare's results are admirable, but I wish to point out that our results in the Prince Alfred are practically as good, without the routine treatment by baths. Dr. Hirschfeld's demonstration at the hospital, for which we all thank him, has removed much of our dislike of the treatment on the score of mechanical difficulties. The business was done smartly, cleanly, and like clockwork; but, in spite of all that is said to the contrary, it must entail a terrible amount of labour to bathe many patients every three hours; 102·2° F. being the height which in Brisbane practice calls for a bath; nor can one get out of the fact that the treatment requires extra assistance, an item of expense which looms as large in hospital as in private practice. Moreover, with our own excellent results, I find it difficult to think that so general a use of the treatment in every case is necessary. The moving into the bath, deftly as it is done here, cannot be quite good for a restless patient with deep ulceration. Nor is the shivering, which I myself witnessed, altogether a comforting sensation. Another point which the outside world, ever ready to find spots in the medical sun, would object to, is the apparent necessity, in order to save labour, of using the same bath-water for a succession of patients. I need not add that this argument ought not to have any weight in balancing the *pros* and *cons* of the matter. It seems, then, to me that one may conclude that taking it all round, in competent hands, bathing is a method of infinite value, but that apparently as good results may be gained by those practising the less robust method of hydrotherapy, *e.g.*, sponging, etc. If one could be sure that in any particular case a long and dangerously high pyrexia was to be the feature of the case, then, in that case, I believe the bath treatment would forestall or control the pyrexia better than milder measures, and I would gladly adopt it in such a case as the routine treatment throughout, but, happily, all enterics are not so situated. Their temperatures are amenable to discipline of a less troublesome and exacting character. These less dangerous cases form the bulk—except in grave epidemics—and they seem to be as well treated by sponging, packing, and the like. I have often thought, and occasionally seen it suggested, that the application of dry cold air circulating under a cradle might produce all the benefits of even bathing, with fewer of its drawbacks. I cannot, therefore, as yet go back to Sydney frankly converted to the bath-treatment. But this I can say: I have now learned the practical application of the method, and I see that most of the mechanical difficulties are easily overcome. I take with me more exact, and at the same time a broader view of the subject than I had before, and a willingness to apply it earlier, and to think more cases likely to be benefited by it than I formerly held, and these views I shall carry into practice. I feel now that I only voice the meeting when I express our hearty thanks to Dr. Hare and our other Queensland colleagues who have so ably and clearly presented the subject to us to-day.

Dr. Hare, in reply, said: Dr. Pain has contended that in ordinary cases the practice of sponging would reduce the temperature sufficiently, would economise labour, and save expense. I cannot agree to this. It is a fact that in the great majority of cases half or three-quarters of an hour will not reduce the internal temperature of the patient—*i.e.*, the rectal temperature—so much as a bath lasting fifteen minutes; and any nurse who has experience of cold bathing will bear me out when I say that the administration of a bath is less tedious and troublesome for the staff, if not less fatiguing for the patient, than sponging. I do not agree with Dr. Hirschfeld's practice of giving frequent doses of antipyretics. Dr. Hirschfeld would not have found these necessary, except in very rare cases, had he not limited himself to the use of tepid water. Tepid baths will not appreciably reduce the rectal temperature in cases of intense pyrexia. In one of my cases, the duration of the bath (75° F.) was gradually prolonged up to sixty minutes, and still the rectal temperature was *higher* after immersion than before. It was not until the water had been reduced to 57° F. that the rectal temperature yielded. I cannot see any reason to fear an increased production of heat, if such occurs. What is important, clinically, is to reduce the internal temperature of the patient's body, for then most of the symptoms of pyrexia subside and do not reappear until the temperature has attained its previous height. As regards perforation, facts must override theories, here as elsewhere. No difference in the perforation rate has occurred after ten years of the bath treatment, although it can hardly be contended that the average temperature of the patients during this period has not been reduced. The reduction of the typhoid mortality at the Brisbane Hospital cannot be accounted for, as one speaker very kindly suggested, by my own long experience in the disease. My personal connection with the fever wards ceased after the first three years of the bath treatment. Since then the wards have been under a long succession of resident medical officers and several honorary physicians; and the results have been almost identical. There is no special virtue in the Queensland climate which renders it specially suitable for cold bathing. The treatment originated in Germany, and, on my recent trip through Canada, I found the physicians at the Royal Victorian Hospital, Montreal, uncompromising cold bathers, and their results were better than those of the Brisbane Hospital. Dr. Pearce's extremely low death rate at the Coast Hospital, Little Bay, can hardly be taken as a standard for comparison. That institution is a long way from town; severe cases are not therefore sent, but go to swell the mortality at the city hospitals. I hardly think that, with our present knowledge, the hospital mortality of typhoid can fall much below 5 per cent. Perforation and hæmorrhage continue to carry off 4 in about every 100 admitted, and there are always other rarer and unavoidable complications which are liable to prove fatal. I have to thank the meeting most cordially for the kindly reference to my work, and for the patience they have exhibited in listening to so much that they have heard frequently before.

SECTION OF SURGERY.

AN ADDRESS DELIVERED TO THE SECTION OF SURGERY.

By Alexander MacCormick, M.D. Edin., M.R.C.S.,
PRESIDENT OF THE SECTION,
Lecturer on Surgery, University of Sydney; Hon. Surgeon, Sydney Hospital.

Gentlemen,—

I am deeply sensible of the honour which you have conferred upon me in electing me to the position of President of the Surgical Section of this Congress; and I am overwhelmingly conscious of my inability to do justice to the position, which has, on former occasions, been filled by such men as Sir Thomas Naghten Fitzgerald and Mr. O'Hara, of Melbourne, and Drs. Stirling and Gardner, of Adelaide.

Unfortunately for me, and perhaps for you, one of my first duties was to prepare an address; and when I turned to the fulfilment of my task, the first difficulty which met me, at the threshold, was the selection of a subject upon which to speak to an assembly so important.

This being the last meeting of the Congress due to be held before the passing away of the century, it occurred to me that it might not be out of place to review, briefly, some of the principal causes which have most contributed to that advance of our Science and Art which has been so conspicuous a feature of the century, and more especially of that part of it which it is now customary to call the "Victorian Era."

To give a detailed account of the advance in surgery during the period under review would be as impossible in the limited time at my disposal as it would probably be unnecessary in this place, but, to understand aright the position of surgery at the beginning of this century, a few preliminary leading facts must be stated. Before this, much had no doubt been done in the study of anatomy by "the great masters." John Hunter, "the father of scientific surgery," had died in 1793. In the same year was published his treatise on the blood and inflammation. He established surgery on the only sure basis of observation and experience, and he made, as Malgaigne said, a *science* of what Paré and Petit had left but an *art*. Before Hunter's time, surgery was practised empirically, so that the science of surgery has been almost entirely a growth of the present century, and its growth has not been a continuous and steady one. It has been rather by fits and starts, and the greater part of it has been evolved during the last fifty years or so.

The most distinguishing surgical features of the century itself have undoubtedly been the introduction of anæsthetics and the use of antiseptics. They are both epoch-making events. Perhaps the only other discovery of equal moment was the introduction of the ligature by Paré. When I remind you that before Paré's time it was the practice to bathe a wound with hot pitch, or apply the actual cautery, as the means for stopping hæmorrhage, we can appreciate the great advance that was made in surgery by the Huguenot army surgeon.

The introduction of anæsthetics has been one of the greatest of blessings to mankind. In 1800 Sir Humphrey Davy discovered the anæsthetic properties of nitrous oxide or laughing gas, and suggested that it might be used in surgical operations. The first operation was performed upon Dr. Horace Wells, a dentist, who had a tooth extracted while insensible from the effects of the gas. He intended to introduce it into his practice, but met with a failure in Boston, and this led him to give it up.

The first discovery of the anæsthetic properties of ether was made by Faraday in 1818, but it was left to Morton, in America, to work out its practical application. The first operation performed under its influence was the pulling of a tooth in October, 1846. The news reached England on the 17th December, and on the 19th a dentist pulled out a tooth, and Liston did an operation under its influence After that most operations were done under ether, until Simpson introduced the use of chloroform. He was the first to apply anæsthesia to midwifery, but he was not satisfied with ether—possibly owing to its inflammable properties, which rendered it dangerous for midwifery use—and tried many substances to act as substitutes. Chloroform was first suggested to him by a Mr. Waldie, a chemist of Liverpool, who sent him some to try. The story goes that he was not impressed with the dull, heavy-looking liquid, which was not at all like his ideal anæsthetic, and he let it stand untested for some days. One night at dinner, he spoke of it to his friends (Keith and Matthews Duncan) after the ladies had withdrawn, and suggested that they should all try it. They did so by putting some of it into their wine-glasses and inhaling it, " when we were all under the table in a minute or two" to quote Simpson's own graphic words. By and by the ladies wondered why they did not come to the drawing-room, and sent a message. The servant, a decent Presbyterian, and possibly not quite ignorant of one of the traditions of old Edinburgh, where a boy was kept for unloosing the cravats of the guests as they succumbed, was scandalised to find her master and all his friends in a hopeless state of slumber under the table. After that the future of chloroform was assured, and it became the only anæsthetic used in midwifery, and perhaps the most important in surgery.

The introduction of chloroform met with fierce opposition, especially in midwifery practice. People of ultra-religious views raised many objections to it, such, for instance, as that it was not mentioned in the Bible and also because it was mentioned that women *should* bring forth in pain and labour. It was also said that, as pain was a punishment for sin, it was flying in the face of Providence to abolish it. Simpson waged a great warfare against these arguments, carrying on a correspondence in half the journals—both lay and medical—in the country, and quoting the Scripture against his opponents as vigorously as they did against him. Amongst other arguments he pointed out that the first operation ever performed—when Adam's rib was taken to form Eve—was done when Adam was asleep, and suggested that, with such a precedent, one could not go very far wrong in using anæsthetics.

Ether continued to be used in the States of America, but chloroform for a long time was almost the exclusive anæsthetic in Europe. When I was in Edinburgh, in the seventies, I never saw ether administered at all, but, on the other hand, I gave chloroform in the theatre at the end of my second year as a student. Then for some reason, which I cannot understand, the mortality from chloroform seemed to increase from year to year, and, as a consequence, ether came into more general use.

Ether and chloroform are still the only general anæsthetics that surgeons rely upon, and we must admit that the use of both the one and the other is attended with some danger, as one would naturally expect with agents so powerful as to destroy all consciousness. Of the two, ether is undoubtedly the safer; indeed, I heard some time ago of a medical witness at a coroner's inquest remarking that in case of death from chloroform the administrator should be tried for culpable homicide. Of course such a statement was unwarrantable, and calculated to do much harm. I think that now most surgeons prefer chloroform for young children and old men, for cases of phthisis and bronchial affections, and for all operations about the regions of the head and neck.

Local anæsthesia was introduced comparatively recently. Dr. Richardson induced local anæsthesia by a fine ether spray, which produced such a degree of cold as to cause the part to freeze. Of local anæsthetics, probably the best is now cocaine, or eucaine, which can be applied in solution to a mucous surface, or injected into the tissues by the infiltration method.

It will probably be unnecessary to speak further of the countless benefits conferred upon man by anæsthetics; of the absence of all anticipatory fear of suffering; of the diminution of shock; of the suffering prevented, and of the happy, subsequent oblivion of all the details of the operation. One great barrier which hindered the development of our art was broken down, and the vast change in surgical practice now became apparent. "The lion's heart" was no longer a requisite of the surgeon, and the type of man who could, and did, practise surgery was altered, although, perhaps, in some cases not for the better. Before the use of anæsthetics, the known "niggler" was carefully avoided by patients. A man like Liston had it all his own way. There is a tradition at the Edinburgh School that this surgeon amputated through the thigh, with only one assistant to hold the leg. He cut the flaps with one hand, and controlled the bleeding with the other.

After the introduction of anæsthetics, operations multiplied enormously, for many people, before anæsthetics, could not be prevailed upon to submit to an operation on account of the dread of pain, and many patients with a stone in the bladder, for instance, preferred death to an operation for its removal. There is a story told of a man who was being cut for stone by a surgeon who had two assistants. One of them received a cheque for 100 guineas soon afterwards, but the other merely got his fee; and when he inquired why this difference had been made, he got the following answer: "Sir, when we were in the midst of the operation, I noticed you turn round and brush a piece of blood clot from your white trousers. A man who could think of his trousers while I was suffering such agony must have been lost to all sense of human kindness and be not worthy of any extra consideration." The thought which occurs to *me* is that patients in those days were, apparently, more appreciative of the surgeon's services than they have ever been in my time.

The next epoch-making event in surgery was the advent of antiseptics. Inspired by the discoveries of Pasteur, and the experiments of Tyndall on floating matters in the air, Lister founded his "System," and, although the methods and details of this system have undergone many changes, there has been no deviation from the main principle. Like the use of anæsthetics, Listerism met with a great deal of opposition and criticism at first, but it is now firmly established, and has led to practical results, which have, in an extraordinary degree, widened the domain of our art, diminished suffering, avoided deformity, and saved innumerable lives.

There are two great principles upon which the system is founded: First, that the healthy unexposed tissues are sterile; second, that suppuration and infective conditions of wounds are due to microbes introduced from without.

The main changes in this method of treatment owe their origin to changing views as to the sources from which infection might be expected to come. Under the guidance of Tyndall's experiments, we had, first, sealed wounds. Later came the spray, but, as the belief strengthened that the air as a source of contamination could be ignored, the spray was gradually put aside, for it came to be admitted that the surgeon's hands, the skin of the patient, the instruments, ligatures, sutures, and sponges were more often at fault.

There are two plans at present for carrying out Lister's system: "One is that which was originally introduced by Lister, and has undergone many modifications. It consists in preliminary disinfection of the skin, hands, instruments, &c., in the use of antiseptic substances during the operation and in the subsequent application to the wound of dressings containing antiseptics. In the other method (sometimes termed the aseptic method), while in the preliminary stages the procedures are the same as in the other, the use of antiseptic substances during the course of the operation is avoided." The hands of the operator and the skin of the patient are prepared as in the first plan, the instruments and other accessories are sterilised by heat, and an aseptic dressing alone is applied.

A surgeon generally works out a plan for himself according to the class of cases he has to deal with. Personally, I use both plans combined. For instance, in an open wound, such as after amputation of the breast, the washing of the wound with an antiseptic lotion, such as weak biniodide or corrosive sublimate solution, will wash away blood clot, and may destroy any organisms that may have fallen upon the surface of such a large gaping wound. This does not interfere with the resistance of the tissues, and it gets rid of dead material, which would be a suitable soil for the growth of organisms. In such wounds, after the most careful hæmostasis, one expects a certain amount of discharge during the first twenty-four hours, and this discharge is best received into an antiseptic dressing. Should the dressing be merely aseptic, and should the discharge come into contact with the atmosphere or with a non-sterile substance, fermentation will take place in it and will quickly reach the wound. On the other hand, in abdominal surgery, I carefully avoid any antiseptic unless sepsis is already present, for it is of more importance to leave the peritoneal membrane uninjured than to destroy a few organisms with the use of an irritating antiseptic lotion, and an aseptic dry dressing is all that is required, for discharges are not abundant—they dry, and therefore will not ferment. I go on the principle of excluding organisms as far as possible, but, at the same time, consider it of equal importance to give as much play as possible to the resistance of the tissues, by avoiding injury to the parts from bruising and tearing, or from tight sutures, by getting as perfect hæmostasis as possible, by accurate coaptation, and by securing rest. The invention of a hæmostatic which would stop oozing of blood, without in any way interfering with the resistance of the tissues, would be a great boon to the surgeon in the treatment of wounds. On the improved methods of hæmostasis, both during and after operation, I will not dwell.

Pathological anatomy and physiology have been a creation of the century, and, with their advance, many diseases which were looked upon as incurable came to be eradicated by the knife. An accurate knowledge of anatomy is of the utmost importance to the surgeon of the present day, not such a knowledge as is obtained from books, but such as is gained from a prolonged study in the dissecting and *post-mortem* rooms. The training obtained in teaching in the dissecting-room is of the greatest value, for it gives a surgeon a familiarity with the parts not otherwise attainable. From having to help students with tangled dissections, he gains valuable experience in recognising structures under altered relations.

This more accurate knowledge of anatomy has become necessary, since the nature of the operations undertaken by surgeons has, in so many instances, become entirely altered. For instance, think how many operations nowadays are, after all, but elaborate processes of dissecting out diseased structures from amongst healthy and vital parts. This cannot be done quickly and effectively unless the field of operation be kept dry, and that is possible only when the surgeon so knows his anatomy that he can, where desirable, so to speak, turn off the tap, by securing the principal vessels of the part.

Amongst the many recent advances in surgery, the one that has interested me personally the most, is the "complete" operation for the removal of cancer of the breast, the desirability of which has been so effectively pointed out by Halstead, Watson-Cheyne, Stiles, and others; and, as it has been my custom to treat cancer about the mouth and tongue on the same principles, it might not be out of place to describe the method I follow in some of these cases.

In operations in malignant disease of the tongue, surgeons frequently amputated the whole tongue; it did not matter whether the disease was on one side only or on the floor of the mouth: the whole tongue was often taken away. In cancer to one side of the middle line this seemed to me unnecessary, especially as, in cases of cancer at the reflection of the mucous membrane on to the fauces, the breadth of healthy tissue to one side of the disease when removed was very limited indeed—the disease was not in the centre of the area removed, as should generally be the case. Having occasion to treat a patient with extensive epithelioma of tongue, fauces, tonsil, and soft palate on one side, I found that, by removing the lower jaw on the affected side and splitting the tongue in the middle line, I could still leave half the tongue. This became a useful organ, much more freely movable, since it was not bound down to the jaw by cicatricial tissue, and the patient could articulate better and was not troubled with salivation. The half-tongue was so useful to the patient that I have adopted this procedure in cases where the disease is clearly to one side of the middle line, and the method has this additional advantage: that one can also perform a "complete" operation by removing all infected glands at the same sitting. I am satisfied that the results justify the adoption of this procedure, and, to get anything like permanently good results, the removal of the lymphatic area leading from the disease is more urgently called for in this disease than in any other form of cancer, for gland infection is present generally from the first, and often when the primary disease is but limited. Furthermore, whenever there is any chance of the epithelial structures in the floor of the mouth being infiltrated—and when the cancerous growth has come into contact with them, this is certain to happen—they, too, are removed.

I will now describe, in detail, the operation I perform.

As preparatory treatment, every care should be taken to cleanse the mouth and teeth by the use of a brush with some disinfectant, such as Sanitas or Condy. The operating-room and the patient's bedroom should be kept at an even temperature.

If the disease extends to the fauces, preliminary tracheotomy unquestionably lessens the risk from septic pneumonia, renders the administration of the anæsthetic easier, makes the patient's breathing more satisfactory, shortens the actual operation, and gives the surgeon a better chance of removing the whole of the disease. I use an ordinary Durham's tracheotomy tube with a funnel attached, and plug the pharynx with a sponge. When the disease does not extend to the pillars of the fauces, tracheotomy is unnecessary.

An incision is made from the symphysis menti to the hyoid bone, and then gently curved outwards, with the convexity of the curve downwards, along the side of the neck to an inch below the mastoid process, much the same as in the preliminary part of Kocher's method. This incision should pass at once down to the deep fascia, through skin and platysma. Any bleeding points should then be secured, and large veins crossing the line of incision should be tied between two ligatures. The flap, including skin, superficial fascia, and platysma, should then be reflected beyond the level of the lower border of the jaw, and the facial artery and vein secured where they cross the bone. If there is any doubt as to the infection of the glands accompanying the internal jugular vein at a lower level than the incision, they can be exposed by carrying another incision downwards from the first incision, along the anterior border of the sterno-mastoid, or vertically over that muscle. Through such an incision, one can clear out the whole of the glands on one side of the neck if necessary. All bleeding having been stopped, the deep fascia is opened along the line of the superficial incision, and all glands and areolar tissue removed, commencing from below, upwards. This dissection should be carried out with the field of operation as free of blood as possible. The parts should be cleanly dissected. In other words, the space should be cleaned out, and any structures that are attached to infected glands removed, without cutting into the diseased tissues during the dissection. It may be necessary to remove both bellies of the digastric and the mylo-hyoid muscles, and to ligature the external carotid, but generally it is only necessary to secure the facial artery below the posterior belly of the digastric. In conducting the dissection from the front, care should be taken to remove the sub-mental and supra-hyoid glands, and the anterior belly of the digastric should be dissected bare, or removed, according to necessity; posteriorly, the anterior border of the sterno-mastoid is dissected bare of fascia, and any glands under it removed, taking care to spare the spinal accessory nerve if it is not involved in the disease, and the parotid gland freed.

The internal jugular vein often requires removal. I have removed the common and internal carotids as well as the vagus, but I think that where it is necessary to remove these structures an operation is contra-indicated. At this stage the lingual artery can be easily ligatured by separating the fibres of the hyo-glossus muscle with a pair of aneurism needles. The mylo-hyoid muscle is divided parallel to the middle line, and the dissection is completed as far as possible working outside the mouth. An incision is now carried through the middle of the lower lip to the anterior end of the incision above described. If the relations of the disease will allow of it, the cheek can be quickly separated from the lower jaw by a periosteal elevator; the masseter separated from the ascending ramus, far enough to be able to divide the coronoid process with saw and bone pliers, and to divide the neck of the condyle in a similar manner. The symphysis is now divided a shade to the diseased side of the middle line with saw and bone pliers. A thread is next passed through the tip of the tongue on the sound side, and the tongue fixed with a pair of forceps on the diseased side so as to steady it while it is being split in the middle line right back beyond the disease, as far as the hyoid bone if necessary. Half the tongue, with half the jaw, and with the submaxillary and sublingual salivary glands and the lymphatic area attached, is then removed in one piece, by dividing, with scissors, the genio-glossus and hyo-glossus muscles at the levels that may be deemed necessary. The mucous membrane is divided beyond the disease and the parts removed, after dividing the stylo-glossus and internal pterygoid muscles and the nerves to the jaw and tongue.

All bleeding having been stopped, the mucous membrane is brought together carefully with catgut—the vertical portion of the base of the tongue to the mucous membrane that may be left at the back of the throat; the mucous membrane on the dorsum of the tongue to the mucous membrane of the inside of the cheek; and, lastly, the mucous membrane at the under-surface of the tip to the mucous membrane on the dorsum of the tip. This shutting out the cavity of the mouth I consider is of great importance, and I have been in the habit of practising it in all operations on the tongue in which the mucous membrane is divided. A few buried sutures will help to keep the raw surfaces together. A drainage tube is introduced, the skin incision is closed, a dressing is applied, the line of incision in the mucous membrane is smeared with iodoform on the finger, and the patient is put to bed. I generally romove the tracheotomy tube the same evening and feed the patient by enemata for twenty-four hours. After that he is fed by a Jaques catheter introduced through the mouth or nose.

I claim for this operation that one can remove the disease most thoroughly, and that the tongue as it is left is more useful—the removal of the jaw allowing it more play, and the operation so far has not been any more dangerous than Kocher's—healing generally taking place by the first intention.

I have performed the operation about five times, with one death as the result of the operation. The death occurred from septic pneumonia in the case of a lunatic. Of the surviving cases two are well after three and a-half years, and one after six months. Another lived two years, and afterwards died of a recurrence behind the inner end of the clavicle. In the sixth case the disease was far advanced at the time of operation, and he got a recurrence shortly afterwards and died. This was the only case in which any recurrence took place in the region of the dissection or of the scar in the mouth. With earlier opportunities of treating this class of cases, I hope that the improvement will soon be as great as that claimed in cases of cancer of the breast by the "complete" operation.

I trust, gentlemen, that I have not wearied you, but, in any case, the sectional work proper is so important that I dare not trespass further on our time or on your patience. I conclude, therefore, with a hope that our deliberations may be big with inspiration and instruction to ourselves and with happiness to our patients.

A REVIEW OF HYDATID DISEASE IN AUSTRALASIA DURING THE DECENNIAL PERIOD, 1888-1897.

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Although hydatid disease has been so widely written about, and discussed by Australasian surgeons, there still appears room for further contributions on the subject; not only on account of its local interest to Australasian practitioners, but also on account of the apparent increase in the number of cases coming under treatment.

This also seems an opportune time to review the treatment, and the results obtained during the last ten years; for, during that time—thanks to the impetus given by the late Drs. Davies Thomas and W. Gardner, and other surgeons in South Australia and the neighbouring colonies—the disease may now be said to be robbed of half its terrors, and a radical cure effected, where, ten years ago, in many cases, one of two methods was adopted—viz., aspiration or puncture, with a possible cure, but more probably only a temporary alleviation, and a subsequent refilling of the cyst with a more virulent fluid; or tapping and subsequent drainage through a canula (*canule à demeure*), which was, in my experience as resident surgeon of the Ballarat Hospital, one of the most heartbreaking tasks that can be given to any medical man—a continuous flow of semi-purulent fluid, and, later, of stinking pus, with portions of the decomposing cyst coming through the tube, resulting in a fight worthy of the Titans between the phagocytes and the bacilli of decomposition, with a result, in a very high percentage of cases, favourable to the streptococci, and consequently fatal to the patient.

Since the discussions, "Radical Operations *v.* Tapping," at the Intercolonial Medical Congress held at Melbourne, and later at Sydney, operative procedures have largely superseded the simpler and older methods. Perhaps operators may now be too ready to adopt the more open methods; but I cannot help agreeing with Dr. Gardner, that "what we want is that men should lay on the table their statistics, the number of cases they have tapped, the results of the tapping, the accidents they have had, and the number of years that they have watched over the cases afterwards, before laying their results before us; for I venture to think that the man who undertakes to say that tapping for hydatid cysts is a panacea has a greater amount of work and more prolonged investigation to do than the man who cuts into the lung or liver and removes the cyst." He goes on to say that "his previous experience of tapping in cases of hydatid cysts packed with daughter cysts, which could not be removed in any way, has been confirmed. Those are the cases in which aspiration is perfectly powerless." There are still many debatable points in this connection, and even in the life history of the *Echinococcus* there are some who deny the necessity of an intermediate host in order to complete the cycle of the *Taenia.*

I purpose showing, by statistics and published cases—vital and operative—the position of the disease in each colony up to the beginning of 1898.

I would take this opportunity of thanking all the medical men and the Government Statists of the various colonies who have kindly furnished me with the desired information.

BIOLOGY.

The life history of the *Taenia echinococcus* has been in the main so thoroughly demonstrated, that observers have added but little fresh to the literature already existent. We are indebted to Steenstrup's "Alternation of Generations" theory for the connection between the *Taenia echinococcus* and the hydatid cyst. His theory, further worked out by German investigators, and supplemented by the feeding experiments of Kuchenmeister in 1851, and those of Davies Thomas with human Echinococci at a later date (1883), proved conclusively the close relationship existing between the two forms. Leuckart was also able to demonstrate in the pig that the result of administering the *ova* was the formation of the hydatid cyst, or larval stage of the *Echinococcus.*

Briefly then, what, beyond dispute, appertains to the life history of the *Taenia echinococcus* is as follows:—The scolex, after being ingested by the dog, or allied species, such as the wolf, &c., passes on into the upper part of the small intestine, where it develops into the adult tapeworm. This consists, when mature, of four parts. The first is the head, armed with a double row of hooklets and suckers, behind which is the first segment, about equal in width to the neck; succeeding this is the third segment, in which the organs of generation can be seen in an immature state; while last of all is the terminal segment, which contains the fully developed generative organs and the fully formed eggs, which are enclosed in a hard-shelled or chitinous envelope, in which eggs the hexacanth embryo can often be observed. These eggs are present in great numbers—estimated at 500. This terminal segment, when ripe, is cast off and extruded with the fœcal matter, and so deposited on herbage or soil, or carried by surface water to other resting-places. The softer tissues of the proglottis soon decay, and the ova are then liberated, and find their way into the alimentary canal of one of the domestic animals (sheep, cattle, or pigs) or of man himself, in the bodies of which, as hosts, they go through the further cystic stage of their development. The duration of life of the adult tapeworm is yet uncertain. Thomas, in his feeding experiments, found numbers of worms thirty-two days after feeding, but none were quite mature, nor did any of them contain the ripe hard-shelled eggs.

When introduced into the digestive tract of its host, the protecting envelope is soon dissolved by the solvent action of the digestive fluids, and the hexacanth embryo is then liberated, and begins an active existence. The route by which it reaches its final resting-place is still somewhat uncertain. Leuckart has found the embryo in the portal vessels, and the generally accepted opinion is that it bores its way through the intestinal wall until it reaches the portal circulation, in which it is carried to the liver, which from its frequency of infection seems to be the site of election. Other observers suggest that the embryos* "push their way amongst the actual or potential spaces of the connective tissue elements of the body, or (though there is no proof) that they may travel in the lymphatic spaces or vessels. Indeed, the deficiencies in our records of hydatids affecting lymphatic glands and closed lymphatic vessels is a remarkable fact which must be taken into account when we seek to explain the distribution of the bladders by a mere passive migration." But however impelled, or by whatever route it travels, the wandering embryo eventually finds a suitable nidus. Having found a suitable resting-place, certain changes then take place in the embryo. Originally somewhat less than a red-blood corpuscle (0·01 mm., Leuckart), it gradually enlarges until it may contain many gallons of fluid. At the earliest stage at which it has been observed (Leuckart) it forms a solid, spherical body, having an elastic cuticle or ectocyst, enclosing granular contents. An internal membrane then appears —the germinal layer or endocyst—while the contents become more transparent owing to fluid beginning to form. The cuticle also becomes thicker, and laminated. At the same time as the embryo is undergoing these changes, certain changes are also taking place in the organ in which it is resting. These consist of the formation of an outer fibrous layer, which surrounds the ectocyst, and which supplies the cyst or bladder with nourishment. This has been variously called the capsule, adventitia, or *sac*, and is often wrongly denominated "ectocyst." It is not in any way a part of the parasite, but is formed by a gradual absorption of the surrounding structures causing a condensation or stratification of the fibrous tissue. The formation of the "sac" proceeds, probably, without causing irritation, and this view seems reasonable from the fact that † "cysts may attain to very great size without contracting adhesions on the peritoneal aspect of the adventitious sac; nor does the sac become adherent to the hydatid cyst itself, which is in close apposition to it."

‡ "The function of the 'sac' is probably two-fold—(1) supplying the hydatid cyst with nourishment; and (2) removing effete products from its interior. This is carried on by a process of osmosis between the fluid in the cyst and the blood contained in the arterial capillaries, which are distributed on the inner aspect of the adventitious sac, the dialysing membrane consisting of the thin walls of the capillaries and the hydatid cyst, which is in direct apposition to them. That such osmosis does occur is proved by the fact that the fluid of an unruptured hydatid cyst may be tinted with green bile pigment (in a cyst of the liver)."

As regards the cyst itself, the views put forward by German authorities appear to be generally accepted. By a process of invagination of the parenchyma and proliferation of its cells, the brood capsules are formed, in the walls of which the "heads" or scolices first appear as a discoidal thickening. This at first is a hollow cone, which gradually becomes fused and permanently inverted into the brood capsule. Hooklets make their appearance, and the "head" is almost the same as that of the adult tapeworm. Here there is a difference of opinion expressed. Some writers (Leuckart) insist that the "heads" always grow from the outside of the brood capsule; while others (Naunyn) hold that they arise from the interior, and as solid bodies.

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* Prof. Watson, *A. M. Gazette*, April 1895.

† Dr. Lendon—"Clinical Lectures on Hydatid Disease."

‡ Dr. Lendon—"Clinical Lectures."

The formation of daughter cysts is intimately connected with the brood capsules. Some cysts are monocystic, or acephalocystic, but others—and these are the majority—contain a larger or smaller number of daughter cysts swimming freely in the fluid. These are formed either endogenously, or exogenously, in relation to the mother cyst. The endogenous variety is produced either (1) wholly from brood capsules without the intervention of the contained scolices; or (2) by the disintegration of the scolices themselves, which spread out over the inside of the cuticle, forming an investment layer; or (3) by an infolding of the parenchymatous layer. The young cysts loosen themselves from their attachments to the mother cyst, become filled with fluid, and, except for the remains of the hooklets, which persist for some time, are in no respect different from the mother cyst, in the fluid of which they now float freely. These in turn may produce a second generation, and in like manner even a third generation may be formed—the "grand-daughter" and "great grand-daughter" cysts.

The life history of the "hydatid" has now been briefly sketched from the ingestion of the "scolex" into the alimentary canal of the dog; its development there into the adult taenia; the extrusion of the ripe proglottides with their contained ova. Further, how these ova are taken into the digestive tract of man or one of the domestic animals, and there go through the resting or larval condition, resulting in the formation of the hydatid cyst or bladder, in the wall of which the *scolex* develops.

It thus seems beyond dispute that the dog or some allied species must of necessity act as "host" for the parasite in its cestoid phase, and that man or one of the domestic animals must fill the like office for the embryo during its cystic phase.

It appears that some observers are, however, still sceptical about the necessity of any intermediate host, asserting that the transformation from scolex to adult *Taenia* through the cystic stage can be carried on in man without requiring the assistance of the dog. This statement has been made by a surgeon of experience and repute, and it is now mentioned to show what divergent views are still held on the subject of hydatid disease, even by Australian surgeons. It is to be hoped that such a theory may ever remain as a theory; were it true, hydatid disease would become so prevalent that immunity from it would be the exception amongst Australasians. The danger of infection from ingestion of scolices is, so far as is known, absolutely *nil*.

Here it may be stated that daughter cysts which may escape from the mother cyst owing to rupture, or during the course of an operation, may find a suitable nidus, and so continue their cystic life, even giving rise to further generations. This is given as one of the probable causes of multiple hydatid cysts of the omentum and peritoneum; and Graham holds this is so probable that he considers operators should take every possible care to prevent the escape of fluid, which may contain daughter cysts, into the serous cavity, where daughter cysts may find a suitable resting-place and continue to grow.

As a practical point in this connection, it becomes the duty of every medical man to point out the danger of infection attaching to the indiscriminate use of *raw* vegetables such as lettuce, cress, tomatoes, &c. These are often grown in gardens to which dogs have access, or the plants either grow or are nourished by water which may contain the ripe proglottides. Dr. Springthorpe, at the Intercolonial Medical Congress held in Melbourne, urged the necessity of greater preventive measures, such as the inspection of dogs in the more infected areas, &c.

Much has been done, but much still remains to be done, especially in country districts. In many places the number of dogs is far in excess of the rates paid per head. The laxity of ordinary precautions was forcibly brought under my personal observation on one occasion during a visit to the country. The district was a highly infected one. A bullock was killed, and the liver and omentum were the seats of an immense number of hydatid cysts; one in the liver must have contained gallons of fluid. These were given, along with the offal, to the dogs, of which there were about a dozen. Although the drinking water was free from any danger of contamination, being stored rain water, still the vegetables were all watered from a dam that was the usual source from which the dogs were supplied. On the other hand, I was much surprised on being told by a butcher, in answer to my inquiry, that he had for years slaughtered, on an average, 350 sheep and 80 cattle per week in Geelong and Ballarat, and had never seen an hydatid, and did not know what it was like; and yet Ballarat and district show a very considerable percentage of persons affected.

GEOGRAPHICAL DISTRIBUTION.

It is impossible to arrive at a correct estimate of the actual number of cases of hydatid disease occurring in Australasia, but an analysis of the statistics of reported deaths caused by the parasite gives some interesting particulars. These statistics have been courteously forwarded to me by the Government Statists of the various colonies.

VICTORIA.

Population in 1896:—1,174,944, and a mean death rate of 15·3 for the nine years ending 1896. Total number of deaths from hydatid disease during that period: Males, 281; females, 229; total, 510.

DEATHS FROM HYDATIDS AT VARIOUS AGES IN VICTORIA, 1888 TO 1896.

AGES.	MALES.								
	1888.	1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.
Under 5 years	...	...	...	...	...	...	...	...	1
5 to 10 ,,	2	1	...	3	...	1	2	1	...
10 to 15 ,,	3	1	2	...	...	4	2	2	4
15 to 25 ,,	2	9	8	4	4	9	6	5	5
25 to 35 ,,	5	8	7	6	6	6	8	4	5
35 to 45 ,,	3	3	2	2	3	2	1	4	3
45 to 55 ,,	4	4	4	8	5	4	1	2	3
55 to 60 ,,	7	2	4	4	2	7	1	2	2
60 years and upwards	5	4	6	5	9	8	8	5	6
Total	31	32	33	32	29	41	29	25	29
	FEMALES.								
Under 5 years	...	...	...	...	...	...	1	...	...
5 to 10 ,,	1	2	2	1	...	1	...	2	1
10 to 15 ,,	...	1	...	2	...	2	1	...	1
15 to 25 ,,	3	6	6	3	8	5	5	3	2
25 to 35 ,,	8	7	5	10	11	7	4	9	11
35 to 45 ,,	3	5	1	1	6	6	1	3	2
45 to 55 ,,	3	4	3	4	5	2	2	5	6
55 to 60 ,,	1	1	2	1	1	...	...	...	3
60 years and upwards	3	3	1	4	3	1	8	1	3
Total	22	29	20	26	34	24	22	23	29

DEATHS FROM HYDATIDS IN VICTORIA, 1888 TO 1896.

YEAR.	URBAN DISTRICTS.				RURAL DISTRICTS.		TOTAL IN VICTORIA.		
	MELBOURNE AND SUBURBS.		ALL OTHERS.						
	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.	Total.
1888	12	9	9	7	10	6	31	22	53
1889	11	14	15	8	6	7	32	29	61
1890	12	9	14	6	7	5	33	20	53
1891	12	15	10	9	10	2	32	26	58
1892	9	14	11	11	9	9	29	34	63
			Males.		Females.				
1893	12	11	29		13		41	24	65
1894	11	10	18		12		29	22	51
1895	10	13	15		10		25	23	48
1896	8	12	21		17		29	29	58

The four decennial periods from 15 to 55 show the greatest number of deaths. The yearly number of deaths does not show any increase. The sexes are affected nearly equally, the males being somewhat in excess. In response to a circular sent out to the various hospitals, I received replies from ten, giving authenticated cases admitted during the decennial period. These cases number 300: Males, 137; females, 163; total, 300. A curious fact in this connection is that, as will be seen in the accompanying table, in every hospital but one the females outnumbered the males :—

Name of Hospital.	Males.	Females.	Total.
Alfred	45	49	94
Children's	16	24	40
Ballarat	32	41	73
Bendigo	31	33	64
Echuca	5	8	13
Clunes	4	4	8
Heathcote	4	3	7
Maldon	...	1	1
Portland	...	...	...
Sale (no records kept till 1896)	...	...	...
	137	163	300

I regret having been unable to obtain full particulars from the Melbourne Hospital for this period. From the Statistical Register the average number treated each year during this period was about thirty-eight, of which the males outnumbered the females by two-thirds.

The following table shows the number of patients treated each year in the eight hospitals:—

Hospital.	1888.		1889.		1890.		1891.		1892.		1893.		1894.		1895.		1896.		1897.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Alfred	3	3	1	3	2	2	4	4	4	5	8	9	8	9	2	5	5	5	8	4
Children's	...	3	1	3	...	...	1	3	...	...	2	3	1	4	4	4	2	...	4	6
Ballarat	8	2	3	3	6	3	2	7	1	6	6	5	3	2	1	1	3	6	2	3
Bendigo	1	7	2	3	1	2	...	1	4	4	9	1	2	1	4	4	4	7	4	3
Echuca	2	...	...	...	...	...	...	...	...	2	1	1	...	1	...	2	...	2	2	...
Clunes	...	...	1	...	1	...	...	...	...	...	...	..	2	...	...	3	...	1	...	...
Heathcote	...	...	...	...	1	...	...	1	...	...	1	2	1	...	1	...	...	...	...	...
Maldon	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	..

From this it is seen that at both the Alfred and Children's Hospitals the number of cases under treatment has much increased. The larger country hospitals do not show such increase, although the number of patients admitted during each year has not decreased.

The following table shows the proportion in which the various organs of the body were affected:—

Organs.	Males.	Females.	Organs.	Males.	Females.
Liver	58	114	Orbit	1	2
Lungs	23	21	"Abdomen"	6	4
Omentum	16	10	Breast	..	1
Kidney	1	2	Lungs and Liver	1	1
Brain	...	1	Muscles, &c.	12	3
Pleura	4	...	Long Bones	3	1
Spleen	2	2	Organs not stated	8	
Testicle	1	...			
Ovary	...	1	Totals	137	163
Spinal Cord	1	...			

Although the number of cases collected is comparatively small, the proportion may fairly be taken as proving the affinity the parasite exhibits for the liver and lungs. No estimate can be given as to the much greater prevalence of the disease in any one part of Victoria. The majority of cases treated at the Alfred and Children's Hospitals are entered as residing in Melbourne or its suburbs, but it does not follow that the disease was contracted in that locality. Two facts can specially be noted: (1) That the disease has steadily increased during the last ten years in Victoria, but (and this is a gratifying feature) the vital statistics do *not* show an increased mortality; (2) That no portion of the colony can be said to be immune from the parasite. This is no doubt due to the fact that the colony is small compared with the other Australian colonies, and that the population is more evenly distributed.

NEW SOUTH WALES.

Population: 1,297,640. According to the returns from the statist there were 370 deaths recorded in the ten years ending 1896 from hydatid disease. Of these there were: Males, 199; females, 171; total, 370.

DEATHS FROM HYDATID DISEASE IN NEW SOUTH WALES, 1887 TO 1896.

AGES.	MALES.									
	1887.	1888.	1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.
Under 1 year	...	...	...	...	...	...	...	. .	...	..
1 and under 2 years ...	...	...	...	...	...	...	...	...	...	..
2 ,, 3 ,, ...	...	...	...	...	...	1	...	...	...	...
3 ,, 4 ,, ...	...	...	...	1	...	1	...	...	...	...
4 ,, 5 ,, ...	...	...	...	...	...	...	...	...	...	...
5 ,, 10 ,, ...	...	...	...	...	5	2	3	1	3	3
10 ,, 15 ,, ...	1	...	3	...	2	1	1	2	1	1
15 ,, 20 ,, ...	...	1	...	1	1	...	1	..	2	2
20 ,, 25 ,, ...	3	...	...	...	...	1	...	1	2	3
25 ,, 30 ,, ...	...	1	4	1	2	1	1	2	1	...
30 ,, 35 ,, ...	2	...	...	2	1	2	1	4	4	2
35 ,, 40 ,, ...	1	1	1	2	2	3	7	2	2	5
40 ,, 45 ,, ...	2	1	...	4	1	...	2	3	1	3
45 ,, 50 ,, ...	1	...	2	2	1	1	2	2	2	3
50 ,, 55 ,, ...	...	1	1	...	1	2	2	2	2	2
55 ,, 60 ,, ...	1	4	...	...	...	1	4	2	1	1
60 ,, 65 ,, ...	1	1	3	1	1	...	...	3	1	..
65 ,, 70 ,, ...	...	...	...	1	...	2	5	...	1	1
70 ,, 75 ,, ...	...	...	...	...	...	...	1	1	2	1
75 years and upwards ...	...	1	1	...	...	1	1	...	1	...
Age not stated	...	1	...	...	...	...	...	...	...	...
Total	12	12	15	15	17	19	31	25	26	27

DEATHS FROM HYDATID DISEASE IN NEW SOUTH WALES, 1887 TO 1896—*continued.*

AGES.	FEMALES.									
	1887.	1888.	1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.
Under 1 year	...	...	...	...	...	...	...	...	...	...
1 and under 2 years ...	...	...	...	...	...	...	...	...	...	...
2 ,, 3 ,, ...	...	...	...	...	...	...	...	...	1	...
3 ,, 4 ,, ...	...	...	...	...	...	...	...	...	...	..
4 ,, 5 ,, ...	...	...	...	...	...	...	...	...	...	...
5 ,, 10 ,, ...	1	1	1	1	...	...	1	...	2	4
10 ,, 15 ,, ...	1	...	...	3	2	1	2	1	1	3
15 ,, 20 ,, ...	1	1	2	...	1	1	4	3	2	1
20 ,, 25 ,, ...	1	1	3	1	...	2	..	2	5	5
25 ,, 30 ,, ...	3	2	...	2	3	2	3	1	4	6
30 ,, 35 ,, ...	4	2	2	3	1	1	3	1	1	2
35 ,, 40 ,, ...	1	1	...	3	3	1	1	2	...	4
40 ,, 45 ,, ...	1	...	...	1	3	1	1	1	...	...
45 ,, 50 ,, ...	...	..	...	3	3	3	...	1	1	2
50 ,, 55 ,, ...	...	...	1	...	...	1	...	4	...	1
55 ,, 60 ,, ...	...	1	...	1	...	...	1	...	2	1
60 ,, 65 ,, ...	...	1	...	2	...	1	1	1	2	...
65 ,, 70 ,, ...	1	...	...	...	...	...	...	1	...	...
70 ,, 75 ,, ...	1	...	...	...	. .	...	1	...	...	...
75 years and upwards ...	...	...	...	...	...	...	...	1	...	...
Age not stated	...	...	...	...	...	...	...	..	...	...
Total	15	10	9	20	16	14	18	19	21	29

The foregoing table shows a gradual increase in the number of deaths recorded, from 27 in 1887 to 56 in 1896. Thomas gives the total number of deaths, covering a period of seven years—from 1875 to 1881, *in*clusive—as 56, with a population of 781,265. Thus in 1896 there were as many deaths recorded as in the seven years preceding 1881.

Sex.—As in Victoria, the total number of males is somewhat in excess of the females.

Age.—This table also shows the number of deaths to be greatest during middle life.

DEATHS FROM HYDATID DISEASE IN NEW SOUTH WALES. 1887 TO 1896.

DISTRICTS.	1887.	1888.	1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.
I. Metropolis	9	8	9	14	16	14	19	13	15	23
II. Central (outside Metropolis)	18	...	1	1	...	...	1	1	...	...
III. North Coast		...	1	1	1	...	...	...	...	1
IV. South Coast		...	...	...	...	...	...	2	...	...
V. New England		2	...	3	4	2	4	3	2	6
VI. The Hunter		1	1	2	4	2	2	5	2	...
VII. The Hawkesbury and Nepean		1	...	1	...	...	1	...	...	4
VIII. Bathurst		...	3	4	1	2	1	1	1	6
IX. Mudgee		...	1	...	2	1	2	1	2	2
X. Argyle		1	...	...	...	...	1	...	1	1
XI. Monaro		2	...	...	1	2	1	1	1	1
XII. The Namoi and Gwydir...		2	...	...	...	1	...	2	2	1
XIII. The Bogan		1	1	4	...	2	2	3	2	1
XIV. Young		1	2	...	1	2	1	2	4	3
XV. Gundagai		...	2	...	...	...	1	1	...	2
XVI. The Murrumbidgee		2	2	4	2	1	2	2	10	...
XVII. The Murray		1	...	1	...	2	6	3	3	3
XVIII. Western		...	1	...	1	2	5	3	2	2
XIX. Lord Howe Island		...	...	...	...	...	...	1	...	...
Total	27	22	24	35	33	33	49	44	47	56

The above table is interesting as showing the number of deaths in the colony in the various districts. Leaving the metropolis out of account—as cases naturally converge to the largest centre from all districts—the most of the cases come from the southern and eastern districts. These are, of course, the most populous, which would account for this to some extent, but they also have the heaviest rainfall.

In response to a circular sent to the various hospitals, replies were received from ten.

I regret being unable to furnish full reports from the two large Sydney hospitals; these, however, were not forthcoming. The average number of cases treated annually at the Sydney hospital is about 25.

The following table shows the number of patients treated each year in five hospitals:—

Hospital.	1888.		1889.		1890.		1891.		1892.		1893.		1894.		1895.		1896.		1897.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Lewisham	...	...	2	...	1	...	2	...	1	3	...	2	...	2	...	3	...	3	...	3
Hay	2	...	...	...	1	2	3	...	4	1	...	...	1	1	2	1	2	1	..	...
Little Bay	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	. .	1	...
Gulgong	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	1	...	2	2
Macleay	...	...	...	...	...	...	...	..	...	...	...	...	1	...	...	...	...	..	1	...

Replies were also received from Goodooga, Brewarrina, Clarence River, and Murrurundi hospitals, stating that no cases had been treated, except in the Murrurundi hospital, where three cases had been diagnosed in the last seventeen years; also from the Albury hospital, stating that owing to absence of clinical records replies could not be furnished. The medical officers of the Brewarrina, Goodooga, and Clarence River hospitals also inform me that their districts seem immune from the parasite, even amongst the sheep and cattle, although very large sheep and cattle districts.

PROPORTIONS IN WHICH THE VARIOUS ORGANS ARE AFFECTED.

Organs.	Males.	Females.	Remarks.
Liver	15	26	The large proportion of females is due to that sex being more specially treated at the Lewisham Hospital.
Lungs	3	2	
Abdomen	5	3	
Brain	1	...	
Broad Ligament	...	1	
Mesentery	...	1	
Omentum	1	...	
Kidney	1	...	
Total	26	33	

This table shows the special affinity the parasite has for the liver.

SOUTH AUSTRALIA.

Population: 336,220. During the ten years commencing with 1888 and concluding with the end of 1897, 114 persons died of hydatid disease. Of these there were: Males, 60; females, 54; total, 114.

Dr. Davies Thomas gives the mortality from hydatid disease in the seventeen years, 1866 to 1882. The total number of deaths was 46: males, 27; females, 19. The population of the colony in 1881 was 279,865, or only 56,355 less than the latest census, whereas the deaths from hydatid disease have increased from 46 in the seventeen years ending 1882 to 114 in the ten years ending 1897.

DEATHS FROM HYDATID DISEASE IN SOUTH AUSTRALIA, 1888 TO 1897.

AGES.	MALES.									
	1888.	1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.
Under 3 years	...	...	...	...	...	...	...	...	...	...
3 and under 4 years ...	...	...	...	...	1	...	...	...	1	...
4 ,, 5 ,, ..	1	1	...	...	...	...	...	...	...	...
5 ,, 10 ,, ...	1	...	...	...	3	...	1	...	...	...
10 ,, 15 ,, ...	...	...	...	...	1	...	...	...	...	...
15 ,, 20 ,, ...	2	1	1	2	...	...	1	...	...	...
20 ,, 25 ,, ...	..	1	1	...	...	1	...	1	3	1
25 ,, 30 ,, ...	...	...	...	1	...	1	1	1	...	3
30 ,, 35 ,, ...	...	...	1	...	...	...	1	1	...	1
35 ,, 40 ,, ...	2	...	...	...	...	2	...	1	...	2
40 ,, 45 ,, ...	1	1	...	...	1	...	...	...	1	...
45 ,, 50 ,, ...	1	...	...	...	1	...	1	...	...	...
50 ,, 65 ,, ...	...	...	...	...	1	...	...	1	...	...
55 ,, 60 ,, ...	...	1	...	...	...	...	...	1	...	...
60 ,, 65 ,, ...	1	...	...	...	2	...	...	...	...	1
65 ,, 70 ,, ...	...	...	...	...	1	...	...	...	1	...
Total	9	5	3	3	11	4	5	6	6	8
	FEMALES.									
Under 3 years	...	...	...	...	...	...	...	...	...	...
3 and under 4 years ...	...	...	...	...	...	...	...	...	...	...
4 ,, 5 ,, ...	...	...	...	...	...	...	...	...	...	...
5 ,, 10 ,, ...	...	...	1	1	...	...	...	1	2	...
10 ,, 15 ,, ...	...	...	...	...	...	...	...	2	...	1
15 ,, 20 ,, ...	...	1	1	2	...	1	1	1	...	...
20 ,, 25 ,, ...	1	3	2	...	1	...	1	1	...	...
25 ,, 30 ,, ...	1	...	1	...	...	...	1	1	...	...
30 ,, 35 ,, ...	...	...	...	3	...	2	1	1	1	...
35 ,, 40 ,, ..	1	1	...	2	...	...	...	...	...	...
40 ,, 45 ,, ...	...	...	...	...	...	...	1	...	...	1
45 ,, 50 ,, ...	1	1	...	...	...	2	1	1	1	...
50 ,, 55 ,, ...	...	...	...	1	...	2	...	...	...	...
55 ,, 60 ,, ...	...	...	...	...	...	...	...	1	...	...
60 ,, 65 ,, ...	...	...	...	...	...	...	...	...	...	...
65 ,, 70 ,, ...	...	...	...	...	...	...	...	...	...	1
Total	4	6	5	9	1	7	6	9	4	3

From this table it will be seen that the death rate remains nearly stationary, 13 deaths having been recorded in 1888, and 11 in 1897.

Sex.—Males again show a slight preponderance.

Age.—The middle decades, from 20 to 50, supply the largest number of deaths.

REGISTRATION DISTRICTS IN WHICH DEATHS OCCURRED.

Districts.	1888.		1889.		1890.		1891.		1892.		1893.		1894.		1895.		1896.		1897.	
	M	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Adelaide	6	3	4	3	1	2	2	3	8	...	2	4	4	1	2	3	2	1	4	2
Norwood	1	...	1	1	1	...	...	...	...	...	1	...	...	...	1	...	2	...	...	...
Frome	1	1	...	...	...	...	...	1	1	...	...	...	...	...	...	2	...	...	...	...
Clare	1	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1	...	1	...	...
Strathalbyn	...	...	...	1	...	...	...	1	...	...	...	...	...	...	..	1	...	...	...	...
Grey	...	...	...	1	...	2	...	...	1	1	...	3	...	3	...	...	2	2	...	1
Burra	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	1	..	...	2	...
Mount Barker	...	...	...	...	...	1	...	...	...	...	...	...	...	..	...	...	...	...	...	...
Kapunda	...	...	...	...	...	...	1	1	...	...	...	...	...	...	...	...	...	...	...	..
Talunga	...	...	...	...	...	...	...	1	...	..	...	...	...	...	...	...	...	...	...	...
Daly	...	...	...	...	...	...	...	1	...	...	...	...	...	1	2	1	...	...	1	...
Wellington	...	...	...	...	..	...	...	1	1	...	...	...	...	...	...	..	...	..	...	...
Flinders	...	...	...	...	...	...	...	...	...	...	1		...	...	...	...	...	...	...	...
Barossa	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...
Nairne	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...
Port Adelaide	...	...	...	...	...	...	..	...	...	...	...	...	...	...	...	...	...	...	1	...
Total	9	4	5	6	3	5	3	9	11	1	4	7	5	6	6	9	6	4	8	3.

This table shows that most of the fatal cases came from the southern and eastern parts of the colony. In this connection it is interesting to note the statement of Dr. Stirling, that *"it is a remarkable fact that no aboriginal admitted into the Adelaide hospital during the last eleven years has died in the institution from any other cause than tuberculosis, *hydatid disease*, or accident."

Of the hospitals whose records are forthcoming, the Port Augusta admitted five cases, of which two were readmissions.

The secretary of the Wallaroo hospital writes stating that the records of cases admitted are too brief to be of any value.

The Adelaide and Mount Gambier hospitals treat the great bulk of cases.

ADELAIDE HOSPITAL.

1888.		1889.		1890.		1891.		1892.		1893.		1894.		1895.		1896.		1897.	
M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
8	9	6	19	7	7	8	8	16	8	19	19	14	13	10	15	9	11	10	9

Sex.—The females are in a small majority, 118 cases having been treated, and 107 males.

Proportion in which the various organs are affected:—

Organs.	Males.	Females.	Organs.	Males.	Females.
Liver	59	71	Pelvis	2	3
Lungs	25	19	Spine	2	0
Muscles	1	5	Thigh	0	8
Iliac Fossa	1	0	Abdomen	9	7
Pleura	3	3	Brain	3	2
Omentum	0	6	Tibia	0	1
Kidney	3	2	Broad Ligament	0	1
Spleen	3	1			
Mamma	0	1	Totals	111	125

From the records of cases obtainable, this table shows how the parasite favours the liver and lung as sites for its cystic growth. In South Australia the disease appears to be increasing steadily. In 1888, 17 cases were treated at the Adelaide hospital, the population of the colony being then 311,491; and, in 1896, with a population of 366,220, there were 20 cases under treatment. In 1893 the number of cases admitted was 38, and 27 in 1894.

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*The Horn Expedition, 1896—Anthropology, page 127.

QUEENSLAND.

Population: 472,179. During the ten years commencing with 1888 and ending with 1897, 32 persons died of hydatid disease. Of these there were: Males, 19; females, 13; total, 32.

DEATHS FROM HYDATID DISEASE IN QUEENSLAND, 1888 TO 1897.

AGES.	MALES.									
	1888.	1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.
4 and under 5 years ...	...	...	...	.	...	...	...	...	...	...
5 ,, 10 ,, ...	...	...	...	...	...	...	..	...	...	...
15 ,, 20 ,, ...	...	...	...	...	...	...	...	...	...	1
20 ,, 25 ,, ...	...	1	...	...	...	...	2	1	...	...
25 ,, 30 ,, ...	...	...	...	...	...	...	...	...	...	...
30 ,, 35 ,, ...	...	1	...	1	...	...	...	1	...	...
35 ,, 40 ,, ...	...	1	...	...	...	...	...	...	1	...
40 ,, 45 ,, ...	...	...	1	...	...	...	...	...	...	...
45 ,, 50 ,, ...	...	...	...	1	...	...	...	1	...	...
50 ,, 55 ,, ..	...	...	...	...	...	...	...	1	...	...
55 ,, 60 ,, ...	...	...	...	...	...	...	1	...	2	...
65 ,, 70 ,, ...	...	...	...	...	...	...	...	...	1	1
Total	...	3	1	2	...	...	3	4	4	2
	FEMALES.									
4 and under 5 years ...	...	...	...	...	...	...	...	1	...	...
5 ,, 10 ,, ...	...	...	...	...	...	...	...	...	...	...
15 ,, 20 ,, ...	...	...	...	...	...	...	1	...	...	1
20 ,, 25 ,, ...	...	...	...	...	...	...	..	...	...	..
25 ,, 30 ,, ...	...	...	...	1	...	...	1	1	1	...
30 ,, 35 ,, ...	...	...	...	...	...	...	...	...	1	...
35 ,, 40 ,, ...	...	...	...	...	...	...	...	...	...	..
40 ,, 45 ,, ...	...	...	1	...	...	1	1	1	...	...
45 ,, 50 ,, ...	...	...	...	...	...	...	...	...	...	...
50 ,, 55 ,, ...	...	...	...	...	...	...	...	...	...	...
55 ,, 60 ,, ...	...	...	...	...	...	...	...	...	...	...
65 ,, 70 ,, ...	...	...	...	...	...	1	...	...	...	...
Total	...	...	1	1	...	2	3	3	2	1

Hydatid disease is not making very great headway in Queensland. Thomas gives the total number of deaths covering a period or four years (from 1878 to 1881) as five, with a population of 226,968.

Sex.—Males form the largest proportion of fatal cases.

Age.—The middle decade of life provides the larger number.

Year.	District in which the Deaths Occurred.
1889 ...	Maryborough, Rockhampton, Warwick.
1890 ...	1 male in Cairns; 1 female in Enoggera.
1891 ...	2 males in Brisbane and Rockhampton; 1 female in Leichhardt.
1893 ...	Bundaberg, Somerset.
1894 ...	3 males in Bundaberg, Nundah, Warwick; 3 females in Brisbane, Cloncurry, Springsure.
1895 ...	4 males in Balonne, Brisbane, Burke, Stanthorpe; 3 females in Brisbane, Bundaberg, Warwick.
1896 ...	4 males in Toowoomba, Moreton E., Toowong, Townsville; 2 females in Diamantina, Toowoomba.
1897 ..	2 males in Brisbane, Moreton E.; 1 female in Brisbane.

The fatal cases are fairly well distributed over the inhabited parts of the colony.

Circulars were sent to forty-nine hospitals, and returns were received from thirteen.

In the following hospitals there were no cases of hydatid disease:—Gladstone, Kennedy, Aramac, Cooktown, Normanton, Taroom, St. George, Palmer River District, and Boulia.

The following table shows the number of patients treated each year in four hospitals:—

Hospital.	1888.		1889.		1890.		1891.		1892.		1893.		1894.		1895.		1896.		1897.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Toowoomba ...	1	...	...	...	...	...	...	...	...	...	1	...	...	2	1	1	3	1	2	...
Thargomindah...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...
Barcaldine ...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...
Ipswich ...	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...

In the Brisbane hospital complete records cannot be given. During 1897 only one case was treated.

Organs Affected.—In every case recorded the liver was the seat of the disease.

The records show that Queensland is singularly free from hydatid disease as compared with South Australia. The latter colony has a population of 366,220, while Queensland has over 100,000 more, with a total mortality of 32 from hydatid disease, as against 114 for the same period in South Australia. This is probably due to the more northern latitude of Queensland. The increase for the last ten years is quite sufficient to show that the colony is not immune, and that as population increases, bringing in its train the facilities for the propagation and transmission of the disease, so will hydatids become a factor to be watched and guarded against.

TASMANIA.

Population: 166,133. In the ten years commencing with 1888 and concluding with the end of 1897, forty-four persons died of hydatid disease. Of these there were: Males, 27; females, 17; total, 44.

DEATHS FROM HYDATID DISEASE IN TASMANIA, 1888 TO 1897.

AGES.	MALES.									
	1888.	1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.
Under 4 years	...	...	...	...	...	...	...	...	...	...
4 and under 5 years ...	...	...	...	...	...	...	...	...	...	...
5 ,, 10 ,, ...	...	...	...	...	...	...	...	1	...	...
10 ,, 15 ,, ...	...	1	...	...	...	...	...	...	...	...
15 ,, 20 ,, ...	1	...	...	...	...	1	...	...	...	...
20 ,, 25 ,, ...	...	...	...	1	...	1	...	1	...	...
25 ,, 30 ,, ...	...	...	...	...	1	...	...	...	...	...
30 ,, 35 ,, ...	...	...	...	...	3	...	1	2	...	...
35 ,, 40 ,, ...	...	1	...	...	...	1	1	1	...	1
40 ,, 45 ,, ...	...	...	...	1	...	...	...	...	...	...
45 ,, 50 ,, ...	...	...	...	...	...	1	1	...	...	...
50 ,, 55 ,, ...	...	...	...	...	...	...	...	...	...	...
55 ,, 60 ,, ...	...	...	...	1	1	...	1	2	...	...
75 ,, 80 ,, ...	...	...	...	...	...	...	...	...	...	...
Total	1	2	...	3	5	4	4	7	...	1

Deaths from Hydatid Disease in Tasmania, 1888 to 1897—*continued.*

Ages.	Females.									
	1888.	1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.
Under 4 years	...	...	...	...	...	...	...	...	...	...
4 and under 5 years ...	...	...	...	...	...	...	...	1	...	...
5 ,, 10 ,, ...	...	1	...	...	...	...	...	...	...	...
10 ,, 15 ,, ...	...	...	...	...	...	...	...	...	...	...
15 ,, 20 ,, ...	1	...	...	...	...	1	...	...	...	...
20 ,, 25 ,, ...	...	...	...	...	...	...	1	...	...	...
25 ,, 30 ,, ...	...	1	...	...	1	...	1	1	2	1
30 ,, 35 ,, ...	...	...	...	...	...	...	1	...	...	...
35 ,, 40 ,, ...	...	...	...	...	1	...	...	...	...	1
40 ,, 45 ,, ...	...	...	...	...	...	...	...	...	...	...
45 ,, 50 ,, ...	...	...	...	...	1	...	...	...	...	...
50 ,, 55 ,, ...	...	...	...	...	...	...	...	...	...	...
55 ,, 60 ,, ...	...	...	...	...	...	...	...	...	...	...
75 ,, 80 ,, ...	...	1	...	...	...	...	...	...	...	...
Total	1	3	...	...	3	1	3	2	2	2

The above table does not show any marked increase in the mortality during the decennial period, but between 1878 and 1881, as recorded by Thomas, only six deaths occurred, with a population of 118,923.

Sex.—The males decidedly predominate.

Age.—The middle decades provide the highest mortality.

The distribution of the deaths from hydatid disease during the decennial period shows a large percentage in favour of the urban districts, which include Hobart and Launceston :—

Year.	District in which the Deaths occurred.
1888 ...	Launceston, 1 ; South-eastern Division, 1.
1889 ...	Hobart, 3 ; Launceston, 1 ; North-eastern Division, 1.
1891 ...	Hobart, 1 ; Launceston, 1 ; Midland Division, 1.
1892 ...	Hobart, 2 ; Launceston, 4 ; North-eastern Division, 2.
1893 ...	Hobart, 2 ; Launceston, 2 ; Midland Division, 1.
1894 ...	Hobart, 3 ; Launceston, 3 ; North-western Division, 1.
1895 ...	Hobart, 2 ; Launceston, 5 ; North-western Division, 1 ; North-eastern Division, 1.
1896 ...	South-eastern Division, 2.
1897 ...	Midland Division, 2 ; South-eastern Division, 1.

No deaths were recorded during 1890, and no deaths from hydatids occurred in the south-western division of the colony during this period.

Thirty out of the 44 fatal cases were in Hobart and Launceston.

From circulars directed to six hospitals, returns were received from two :—

Hospital.	1888.		1889.		1890.		1891.		1892.		1893.		1894.		1895.		1896.		1897.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Hobart	...	...	...	...	...	...	...	...	...	...	2	...	1	3	3	3	4	3	3	3
Zeehan	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1

Dr. Sprott writes that no definite information can be given prior to 1893.

The organs affected in the detailed cases were as follow :—

Organs.	Males.	Females.	Organs.	Males.	Females.
Liver	8	6	Jaw	0	1
Lung	4	3	Zygomatic Fossa ...	0	1
Abdomen	0	2	Axilla	0	1

NEW ZEALAND.

Population: 714,612. In the ten years, 71 persons died of hydatid disease. Of these there were: Males, 38; females, 33; total, 71.

The table courteously furnished from the Registrar-General's Department is reproduced *in extenso*, as it shows not only the number of deaths, with sex and age, but also the organs affected in each case.

DEATHS FROM HYDATID DISEASE IN NEW ZEALAND, 1888 TO 1897.

Year.	Place.	Sex.	Age.	Organs Affected.
1888	Gabriels ...	M.	11	Brain and Liver.
	Oamaru ...	F.	21	(Not stated).
	Auckland ...	M.	70	Liver.
	Dunedin ...	M.	52	,,
	Invercargill	M.	49	,,
1889	Dunedin ...	M.	20	Lung.
	,, ...	M.	36	,,
1890	Wairau ...	M.	52	Liver.
	Dunedin ...	M.	53	Lung.
	Wellington...	F.	7	Brain.
1891	Dunedin ...	F.	14	Liver.
	,, ...	M.	11	Lungs.
	,, ...	M.	4	Liver and Lungs.
1892	Wairau ...	M.	23	Liver.
	Waimate ...	M.	20	Brain.
	Ashburton ...	M.	36	Pleura.
	Timaru ...	M.	48	Lung.
	Dunedin ...	M.	5	Brain.
	Invercargill	M.	38	Liver.
	Auckland ...	F.	7	,,
	Wellington...	F.	35	,,
	Dunedin ...	F.	7	,,
	,, ...	F.	20	Lung.
	,, ...	F.	20	Kidney.
1892	,, ...	F.	24	Liver.
	,, ...	F.	26	,,
1893	Auckland ...	M.	37	,,
	Dunedin ...	M.	49	,,
	Wellington	F.	17	,,
	Dunedin ...	M.	3	,,
	Invercargill	F.	10	Lung.
	Clutha ...	F.	48	Liver.
	Waikonaite	M.	22	Brain.
	Winton ...	M.	27	Liver.
1894	Wellington	M.	48	Brain.
	Waimate ...	M.	7	Lung.
	Gore ...	M.	26	Liver.
1894	Gabriels ...	M.	40	Abdomen.
	Waitahuna	M.	45	Lung.
	Port Chalmers	M.	79	Liver.
	Dunedin ...	F.	28	Spleen.
	Oamaru ...	F.	39	(Not stated).
	Dunedin ...	F.	58	Liver.
1895	Napier ...	F.	12	Liver and Spleen.
	Wellington	F.	59	Liver.
	Huntly ...	M.	53	,,
	Waimate ...	M.	17	Brain.
	Dunedin ...	M.	64	Pleura.
	,, ...	F.	25	Liver and Pleural Cavity.
	Hutt ...	F.	$1\frac{2}{3}$	Brain.
	Otaki ..	M.	76	Liver.
	Blenheim ...	M.	14	,,
	Christchurch	M.	22	Brain.
	,, ...	F.	70	Liver and Stomach.
	Blackstone ...	M.	57	Lungs.
	Auckland ...	F.	41	(Not stated).
1896	Wellington	M.	47	Pleura.
	Auckland ...	F.	32	Liver.
	,, ...	F.	49	Uterus.
	Invercargill	M.	25	Liver and Lungs.
	Dunedin ..	F.	28	Liver.
	Wellington	F.	41	Brain.
	,, ...	F.	25	Liver.
1897	Auckland ...	F.	10	Brain.
	Invercargill	F.	34	(Not stated).
	Greytown ...	M.	50	,,
	Wellington	F.	11	Liver.
	Dunedin ...	F.	41	,,
	Kaiapoi ...	F.	52	(Not stated).
	Dunedin ...	M.	9	Lungs.
	Oamaru ...	F.	29	(Not stated).

Sex.—Males are slightly in excess over females.

Age.—The greatest mortality is during the middle decades.

The returns show a marked *increase* in the mortality since 1892. The localities affected are spread fairly equally over the whole colony, principally along the east coast.

The organs affected are principally the liver and lungs, but the number of cases of hydatid of the *brain*, either single or in conjunction with another organ, is remarkable, being 11 out of a total of 71 deaths.

The following hospitals report no cases of hydatid disease having been treated during the decennial period:—Thames, Wallace, Masterton, Reefton, and Picton.

Place.	1888.		1889.		1890.		1891.		1892.		1893.		1894.		1895.		1896.		1897.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Christchurch ...	...	..	2	1	3	1	1	1	...	1	...	...	1	1	...	1	1	1	2	4
Auckland ...	1	...	1	1	...	...	...	...	...	...	1	...	...	..	...	1	...	3	1	1
Waimate ...	...	...	...	...	1	...	...	...	...	...	...	...	1	...	2	2	...	...	...	...
Nelson ...	...	...	...	...	1	...	...	...	...	...	...	..	...	...	...	...	...	...	...	1
Tuapeka ...	...	...	...	...	...	...	..	...	...	...	...	...	2	...	...	...	...	..	1	...
Totara ...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...

No returns have been received from Wellington (160 beds) and Dunedin (120 beds), but this cannot alter the conclusion that hydatid disease is still comparatively rare in New Zealand.

WESTERN AUSTRALIA.

Population: 137,946. During the decennial period ending 31st December, 1897, nine persons died of hydatid disease.

Year.	District.	Sex.	Age.	Organs Affected.
1888	York	M.	53	(Not stated).
	Busselton	M.	18	Liver.
1893	Geraldton	F.	20	(Not stated).
1897	Fremantle	M.	21	,,
	Coolgardie	M.	43	,,
	Northam	M.	46	,,
	Katanning	M.	59	,,
	Perth	M.	22	,,
	Perth	F.	28	,,

This table is furnished by the courtesy of the Colonial Secretary.

Of the number, only two deaths occurred in hospitals—one in the Fremantle and one in the Colonial Hospital.

Males decidedly predominate, and the ages are all in the middle decades.

The abnormal increase in 1897 is probably to be accounted for by the great influx due to the opening up of the goldfields. The population increased from 58,694 in 1892 to 137,946 in 1896. It is therefore impossible to form a correct estimate from the statistics, as doubtless a number of those who died from the disease were affected prior to residence in the colony.

The only hospital from which a reply has been received was Southern Cross, in which no cases were admitted.

OPERATIVE TREATMENT.

The biology of the *Taenia*, and its geographical distribution in the Australasian colonies, together with the vital statistics of the disease in each colony, have thus been sketched. Let us now turn to the therapeutical aspect, and endeavour to trace the advance, if any, made towards the cure of hydatid disease during the decennial period ending with 1897.

I do not purpose entering into the pathological changes that may occur during the cystic stage in the life history of the parasite. Suffice it to say that after a longer or shorter period—this being indefinite, and often lasting for years—certain changes occur in the cyst.

1. Spontaneous cure by extrusion of the contents of the cyst *per vias naturales*, by bursting into bowel, ureter, bladder, bronchial tubes, &c.

2. (*a*) Continued growth or discomfort to the patient from pressure symptoms; or (*b*) Death of the parasite and subsequent degenerative changes —both of which conditions necessitate operative procedures.

The method of procedure generally adopted, almost up to the date of the Intercolonial Medical Congress held at Melbourne in January, 1889, was tapping, either by aspiration or puncture. Owing to the papers read at that Congress by the late Drs. Davies Thomas and William Gardner, and what may be called the "Adelaide school," and the brilliant results shown by them from the more radical methods of operation, the death-knell was rung of tapping as a curative method. Dr. Gardner gives the percentage of deaths by the various methods:—Récamier's method, 29·76 per cent. in 84 cases; retained canula, 26·66 per cent. in 90 cases; radical operations, 17·39 per cent. in 46 cases. He also tabulates 32 cases of section occurring in his own practice, showing 100 per cent. of recoveries in the thoracic cases, and a mortality of 6·25 per cent. in the 32 sections.

As regards tapping and electrolysis, Dr. Gardner remarks that *"Reliable statistics are impossible to obtain, on account of the fact that in the great majority of cases the surgeon loses sight of the patient, and possibly also because, in a vast number of cases, the sufferer, finding that his cyst has refilled, seeks other advice." He goes on to say that it is not free from danger, and though in some cases really successful, he has quite given it up, except as a "diagnostic aid at the time of performing a radical operation. Refilling of the cyst has been known to take place even as long as ten years after the patient was believed to be cured; indeed, it is almost impossible to fix a time at which an hydatid cyst treated by puncture may safely be said to be cured."

There is no doubt that tapping, even with a hypodermic needle, for exploratory purposes, is not unattended with danger in hydatid disease. I inserted a hypodermic needle into the chest wall of a healthy adult in a case of pulmonary hydatid, and most urgent dyspnœa and collapse followed almost immediately. A medical practitioner also read a paper at a meeting of the Ballarat District Branch of the British Medical Association, in which he enumerated three cases where most alarming symptoms, and in one case death in a few hours, followed exploratory puncture with a hypodermic needle.

Of two other methods—viz., (*a*) electrolysis, (*b*) medicated injections into the sac—†Dr Thomas concludes that the successes claimed by these methods are simply due to the *puncture*.

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* GARDNER—"The Surgical Treatment of Hydatid Disease," Melb. Congress.

† THOMAS—"The Operative Treatment of Echinococcus Cysts," Melb. Congress.

Medicated injections have been largely employed, either for their parasiticide action on the *Echinococcus*, or for the solvent action on the cyst itself. Towards this end, iodine, carbolic acid, Condy's fluid, and latterly corrosive sublimate, have all been tried. My greatest experience has been with Condy's fluid, which was injected through a retained canula every day. This was the regular treatment of hydatid cysts at the Ballarat Hospital when I was appointed resident surgeon in 1886. The routine treatment was to inject through the canula as much Condy's fluid (two teaspoonsful to the pint of water) as the cyst would hold, then pass a long probe into the cyst and extract, by a screwing or rotatory movement, as much of the retained cyst wall or other solid contents as your patience would admit; the same treatment to be carried on from day to day or week to week, until the dead cyst was all evacuated and the sac gradually contracted, or—and this was only too frequent—the patient succumbed from exhaustion or septicæmia. May this treatment for ever be consigned to the limbo of the surgery of the dark ages!

Bacelli's method of injecting a solution of perchloride of mercury into the cyst was advocated at the Sydney Intercolonial Medical Congress in two papers—one recording three successful cases, but the cures appear to have been effected in all cases after somewhat prolonged and unsatisfactory convalescence; the other paper, by Dr. Tremearne, giving a list of sixty-five cases, of which sixty were cured, three not benefited, and two died. So far no surgeon has published further results of this treatment. Personally, I have not attempted it, and, though it might be applicable in selected cases of simple or mono-cysts, I fail to see how corrosive sublimate, given in such quantities as would not be inimical to life, could produce such a "shrivelling up" of the cysts as to effect a cure in cases where the mother cyst is filled with daughter and grand-daughter cysts, or where degenerative changes have set in owing to the death of the parasite.

It is interesting to note the discussion on hydatid disease which took place during the sitting of the Intercolonial Medical Congress at Melbourne. Sir T. N. Fitzgerald and Dr. Bird, of Melbourne, and Dr. Sydney Jones, of Sydney, who took part, all condemned, in more or less measured terms, the more radical treatment. Dr. Bird, whose experience dated as far back as 1861, stated that he had tapped many cases with perfect success with the ordinary fine trocar and canula. He considered the aspirator was distinctly dangerous. In old suppurating cases he introduced a trocar, left it *in situ*, and after a few days, if necessary, enlarged the wound, when he invariably was able to remove the cyst, without further trouble or danger.

Following on this discussion, Drs. Sydney Jones and Scot-Skirving brought forward a report on the "Results of Tapping Hydatid Cysts" at the Congress held at Sydney in 1892. They collected 146 cases, but full statistics of these could not be obtained. Of 82 cases treated at the Prince Alfred Hospital, 13 were cured, and 25 required incision, the result of the rest being doubtful. They advocate tapping in all presumably simple abdominal cases, and a fair trial given before proceeding to more radical measures. A small needle, 1-16th inch, should be used, and, owing to the danger of collapse, this treatment should not be resorted to without due precautions.

Such, then, is the position as regards tapping and its allies. Perhaps the swing of the pendulum has gone too far in the direction of "radical" operations; but it is a curious fact that in all published records from hospital and other sources, since 1892, I cannot find a single case of hydatid disease treated by any form of tapping or aspiration where the case is reported as cured. In several cases tapping has been resorted to, but invariably the patient is reported as being readmitted for further treatment.

As regards the radical operations—

1. CANULE À DEMEURE
2. VOLKMANN'S
3. RÉCAMIER'S
4. LINDEMAN'S
5. BOND'S
6. RUSSELL'S—

the first three have very justly fallen into disrepute. In the reports of all cases that can be found during the last five years, only one case was treated by *Canule à Demeure*, five cases by Volkmann's, and not a single case by Récamier's method.

In discussing the remaining radical operations, I purpose leaving out of account all cases in which the serous cavities are not opened, so that all cases occurring in the musculature or bones are left out of account, as these would be certainly dealt with either by incision or excision. Hydatids of the kidney and cases of retro-peritoneal hydatid disease are, however, included, being so closely allied to intra-peritoneal affections.

The radical operations to be discussed are—(1) Lindeman's, (2) Bond's, (3) Russell's (modification of Bond's).

1. *Lindeman's* operation has for the last twenty-seven years, and more so of later years, been deemed the operation *facile princeps;* especially so in cases of suppurating cysts, or cysts which may be still growing, and are filled with daughter cysts. The details of the operation are so well known that little needs to be said. The "Adelaide school" favours stitching the serous covering of the sac to the parietal serous membrane prior to incising the cyst, then drawing off a small quantity of fluid, enlarging the incision to admit the finger, by which the whole cyst is hooked up to the parietes, any escape of fluid or daughter cysts into the serous cavity being prevented by the circle of stitches and sponges, which are suitably placed round the incision. The ectocyst and the whole contents of the sac are then, if possible, withdrawn, the sac itself carefully irrigated, sponged, and "anchored" to the parietal wall; a drainage tube is passed to the bottom of the collapsed sac, and sterile gauze dressing is applied.

The modifications that I prefer to adopt are: After the cyst is exposed, numbers of sponges are packed round the site of the proposed incision. A small quantity of fluid is then drawn off, and the sac hooked up, cleared of its contents, and washed out; a sponge is then placed inside the collapsed sac, to prevent leakage; the sponges are removed from the serous cavity, and the serous surfaces are then united by a continuous fine catgut suture. Four or six "anchoring" silkworm gut sutures are then introduced through the parietes and through the wall of the sac, between the edge of the incision and the continuous suture. By this method it seems to me easier to introduce the continuous suture, and to afford greater protection against leakage into the serous cavity than in the method adopted by the Adelaide surgeons. I also prefer to use a long strip of sterile gauze passed to the bottom of the collapsed cyst—enough to act as a drain, but not in such quantity as to "plug" the sac, and cause "pocketing" of any discharge. The question of including the skin in the "anchoring" sutures is still an open one. The objection raised is the dragging and incurving of the skin, caused by the retraction of the collapsing and shrinking sac. (*See* Case 1.) The inclusion of the skin gives a firmer hold, and I have never seen any permanent ill-effects from doing so.

Irrigation of the sac at subsequent dressings, if done at all, should only be adopted when there is any suspicion that remnants of the sac contents have been left behind, or any sign of septic trouble. It should then be conducted with great gentleness, to avoid any tearing of the sac from its "anchors."

The following table gives a summary of operations for hydatids at the Adelaide Hospital, from 1st January, 1888, to 31st December, 1897*:—

Position of Parasite.	Operation.	'88.	'89.	'90.	'91.	'92.	'93.	'94.	'95.	'96.	'97.	Result.		Total.	Per cent. of Deaths.
												Cure.	Death.		
Pleura	Thoracic Section ...	1	1	1	...	1	1	4	...	...	...	9	...	9	8
Lung	,, ...	3	3	3	3	...	1	...	...	1	2	14	2	16	
Liver	Thoracic Abdom. Section	4	3	2	1	7	4	7	6	2	...	29	7	36	19·4
Liver	Abdominal Section	4	13	7	10	4	9	12	12	7	10	74	14	88	26·1
Spleen	,, ...	...	1	...	...	1	...	...	...	...	...	...	2	2	
Kidney	,, ...	...	...	...	1	...	...	...	...	...	...	1	...	1	
Omentum ...	,, ...	2	1	...	1	...	2	...	..	1	...	6	1	7	
Pelvis	,, ...	1	1	1	...	...	..	...	2	4	...	8	1	9	
Kidney	Lumbar Section ...	..	...	...	...	1	...	...	...	...	...	...	1	1	
Liver	Aspiration ...	1	...	...	...	...	...	...	...	...	...	1	...	1	
Broad Ligament	Abdominal Section	...	...	...	...	...	1	...	...	...	...	1	...	1	
		16	23	14	16	14	18	23	20	15	12	143	28	171	
Extra-Visceral—Mamma, Pectoralis Major, Temporal Muscles, Loin, Groin, Thoracic Wall, Buttock, Back (Excised)														9	
														180	

Up to 30th June, 1892, Dr. Stirling, in his table, shows a percentage in thoracic section of 6·6 for 15 cases. In thoracic abdominal section the percentage is 30·7 for 13 cases; and in abdominal section 17·0 for 47 cases.

The results of cases treated in the hospitals of Victoria from which reports have been received are shown in the following table. They include all operations involving the serous cavities, from 1888 to 1897:—

Organ.	Operation.	Results.		Total.	Per cent. of Deaths.
		Cure.	Death.		
Liver	Lindeman's	96	29	125	23·2
Lung	Resection	16	6	22	27·2
Omentum	Lindeman's	12	3	15	20·0
Omentum	Excision	5	...	5	
Pleura	Thoracic Section	3	2	5	
Abdomen	Lindeman's	4	2	6	
Ovary	Abdominal Section	...	1	1	
Broad Ligament	Abdominal Section	1	...	1	
Spleen	Lindeman's	2	1	3	
Kidney	Lindeman's	3	...	3	
Brain	Trephining, &c.	...	1	1	
Spinal Cord	Trephining, &c.	..	1	1	
Totals	...	142	46	188	

Probably, if full returns from the Melbourne Hospital had been obtainable, the percentages would have been lower. In most of the cases the deaths occurred in multiple suppurating hydatids—cases that could not have been treated by any less radical procedure, and which yield a very high mortality by whatever measures they are treated.

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* The first four years are taken from Dr. Stirling's paper, Intercolonial Medical Congress, 1892.

By grouping all the "abdominal sections" together, as in the Adelaide Hospital table, the percentage is 22·9 in 159 cases.

The other colonies show better results in the small number of cases of which I have authentic reports:—

New South Wales.—27 cases of abdominal section, and 2 deaths, or a percentage of 7·4; and 8 cases of resection, with 3 deaths.

Queensland.—9 successful cases of abdominal section, and 3 deaths.

New Zealand.—30 cases operated on, with 4 deaths, of which 1 was an hydatid of the brain, and 1 in which the "abdomen was full of hydatids," and the patient lived for two months after operation.

Tasmania.—14 cases were operated on, with 3 deaths.

Coming now to the more recent radical operations—

2. The operation known as *Bond's* was first advocated by C. J. Bond, of Leicester (*British Medical Journal*, 1891, Vol. I., p. 795). The procedure followed is known as the intra-peritoneal method. The cyst, after being exposed by the necessary incision, is opened, the contents evacuated, the sac thoroughly irrigated and sponged dry, a few fine catgut sutures inserted into the inverted edges of the collapsed sac, "to prevent the subsequent entrance of a coil of intestine," and the sac then returned into the serous cavity, without any drainage. The parietal incision is completely closed without any drainage. Bond sees "no reason why we should not deal with cysts of the liver and other organs in the same way" (his paper specially dealt with cysts of the omentum), "provided, of course, that suppuration had not occurred."

3. *Russell's* method, as described in the *Intercolonial Medical Journal*, 1st February, 1895, has this important distinction from Bond's · that after clearing out the cystic contents, and carefully sponging the sac, he returns the collapsed sac without introducing any sutures, and closes the parietal incision.

This intra-peritoneal treatment is a great advance in the surgery of hydatid disease; and Dr. Charles Ryan (*Australian Medical Journal*, August, 1895) says that the "whole aspect of hydatid disease of the abdomen is changed by it," and he is confident that "all other methods in dealing with *simple uncomplicated cysts* will rapidly fall into permanent disuse." I have taken some trouble to examine the reports of published cases that have been treated by this method or by Bond's, and the ideal successes have invariably been in cases of simple cysts, mostly situated in the mesentery, and a few in the liver, lungs, and kidney. I have collected twenty-eight cases, with three deaths. The liver was the seat of the disease in all the fatal cases, the cause of death being apparently septic peritonitis. Amongst the cases that recovered, however, union by first intention was not the rule, a number requiring a partial reopening of the wound to let out serous or turbid fluid, and in some cases, pus.

The best results have been attained by Russell's method, which is admirably adapted for simple multiple cysts attached to the mesocolon or mesentery, which cannot be excised, and which before the introduction of his method were attached to the parietes, if possible, or the case was abandoned owing to the hopelessness of evacuating and fixing such numbers of cysts as are at times encountered.

When situated in the *liver*, this method should only be adopted when the cyst is so deeply placed that Lindeman's operation is impracticable. In these cases a serous exudation from the sac is not the danger to be feared—that would be readily absorbed by the peritoneum; but in my experience it is the rule rather than the exception for bile to be poured out into the sac, owing to the difference in tension between the liver itself and the collapsed sac.

Should this occur into the general peritoneal cavity, a fatal result must be looked for. Amongst the published cases there are a few in which a perfect result has been attained in hepatic cysts. In these, probably, the cyst has either formed more on the surface than in the substance of the organ, or the size of the cyst is such that the vascular tension is not materially altered.

The theory put forward by Dr. Moore, that the edges of the incision in the sac always lie in contact with the parietal wound, and that this is a safeguard against any general infection, does not, unfortunately, hold good in practice, for a fatal case published in the *Australian Medical Journal*, September, 1895, showed *post-mortem* that "the sac cavity lay at the back of the abdomen; the edges of the incision showed no signs of union, either to each other or to other structures; the incision communicated with the general peritoneal cavity, and was not near the parietal wall, but in apposition with intestine."

In pulmonary hydatids successful cases have been reported by Russell's method, in which the cyst was incised and evacuated, the parietal incision closed, and sponge pressure applied. In these cases I would certainly be inclined to modify the operation by stitching the two pleural surfaces together, so that, should the cyst refill in a few hours, there would be union between the serous surfaces, and the contents of the sac would then find their way to the surface, or could be readily allowed to escape by freeing one of the sutures, without incurring the risk of a septic pleurisy.

What is, then, the present position of surgical procedure in the treatment of hydatid disease in connection with organs where one of the serous cavities have to be opened?

Brain and Spinal Cord.—In these cases the mortality is deplorably high. *Verco, in an able paper, fully discusses the question of cerebral hydatids, and records seventy cases, with one recovery, and that one probably grew from the arachnoid, and pushed the brain before it (Clubbe and Graham's case). Verco considers that death in these cases after operation is due to draining of the cerebro-spinal fluid, owing to communication with the ventricles. He recommends Bond's method after opening the skull with a small trephine (1 inch), and then applying firm pressure over the scalp.

Pulmonary Hydatids.—In many cases spontaneous cure results from the cyst bursting into a bronchus, and the contents being coughed up. The question arises, Should the surgeon interfere in such a case, where there are no urgent symptoms? To this I would unhesitatingly reply, "No" (Case 2), as nature often effects a cure, and the patient experiences no further trouble. But in some cases spontaneous evacuation is going on, but urgent symptoms are also present—hæmoptysis, great emaciation, or a serious hectic condition from a suppurating cyst (*see* Case 3). In such cases Lindeman's operation, with resection, should be performed.

In small simple cysts, tapping does, in a great many cases, effect a permanent cure; in fact, Dr. Bird, of Melbourne, whose experience of this form of treatment has been extremely large, would not recommend any other. But in large cysts, seriously involving the lung, I cannot see how the presence of such a foreign body as an old and thickened ectocyst could be tolerated after the fluid contents had been withdrawn; therefore the example of the Adelaide surgeons should be here followed—viz., resection, with removal of the ectocyst.

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* Intercolonial Congress, Sydney, 1892.

Hydatids situated in the *abdominal cavity* may be ranged into three classes:—

1. Those cases in which the *convex surface of the liver* is affected. Owing to the greater risk attending operative procedure, a preliminary tapping may be undertaken. Should this fail, a rib should be resected over the area of greatest dulness, and the cyst opened and drained, after carefully suturing the two pleural surfaces, and, if necessary, the two peritoneal surfaces, together (Case 1), so as to guard against any leakage into the serous cavities.
2. Old and large cysts, which have absorbed a large amount of liver substance; or suppurating cysts situated anywhere in the abdominal cavity. These cases should be dealt with by Lindeman's operation.
3. Simple cysts which are connected with the mesocolon, omentum, &c., or which are so deeply situated that fixation to the parietes is impossible. These are the cases to which Bond's or Russell's "intra-peritoneal" method is admirably adapted.

Small cysts that are freely movable—such as some omental cysts—may preferably be treated by complete excision. This method would naturally be preferred when possible.

Hydatid cysts situated in the muscles, bones, or other tissues should be treated on the ordinary surgical principles of incision, with or without drainage; or, where possible, complete excision of the fibrous sac.

CASE 1.—*Multiple hydatid of the liver.*

E.D., æt. seventeen, female, admitted to the Ballarat Hospital on 6th September, 1896. Large area of dulness, extending right across the hypochondrium—almost to the nipple—on the right, and running into the cardiac area of dulness on the left. As the tumour appeared most prominent on the left side, I resected 1½ inches of ninth rib, opened into pleural cavity, stitched the two pleural surfaces together, then cut through the diaphragm, when I was able to seize the cyst wall, and draw it up towards the surface. After stitching the peritoneal surfaces, the sac was incised, and a large amount of clear hydatid fluid escaped. The ectocyst was delivered, and the adventitia was stitched to the parietes, including the skin. The bottom of the cyst could not be reached with the finger. Iodoform gauze strip used as a drain. Next day the skin was puckered in to such an extent by the retraction of the sac that I feared it would give way; this, however, gave no further trouble, and I believe, had the skin *not* been included, the "anchoring" sutures would have torn through. As the temperature still kept up, and the dulness on the right side did not diminish, I explored, and drew off 2 oz. of clear fluid, when the flow stopped. On introducing the needle a second time, I drew off *pus.*

On the 28th I resected 1½ inches of a rib on the right side, and opened into a large suppurating hydatid, also of the liver, which was filled with daughter cysts, some collapsed, and others still containing clear fluid. Lindeman's method was adopted, and she made a good recovery. Is now married, and in perfect health.

CASE 2.—*Pulmonary hydatid.*—Spontaneous cure.

R.M., æt. twenty-one, miner. On being called to see him, I found him suffering from all the signs of an acute pneumonia of the right lung: Dulness to angle of the scapula, rusty sputum, and temperature 103·4°; history of a chill a few days previously; had lately come back from Western Australia, where he was mining; ten months previously in Ballarat had a severe hæmoptysis when at work below ground; one brother had died of phthisis. Two days after my first visit he had a severe hæmoptysis, and this was followed by a gush of fluid; most of it was thrown out, but some had been expectorated shortly before my visit, and consisted of turbid, bile-stained fluid, with pieces of cyst membrane, which showed the characteristic lamination under the microscope. I should say, there was not now, nor had there ever been, any hepatic enlargement, jaundice, pain, or any other sign of liver trouble. From this out the case progressed favourably. The sputum was frothy and profuse, and always bile-stained, sometimes almost pure bile. This condition lasted for about a fortnight, when the sputum became clear, and gradually ceased. At the present time (eight months after tho attack) he is strong and well. The interesting feature here is the spontaneous cure of what must have been an hydatid cyst of the convex surface of the liver, brought about by perforation of the diaphragm and rupture of the cyst into one of the bronchi.

CASE 3.—*Pulmonary hydatid*, involving one of the larger pulmonary vessels and bronchus.—Death from hæmorrhage.*

H. S., æt. twenty-three, single, engaged in pastoral pursuits. First complained of severe pain over the base of the right lung, accompanied by fever and cough, in October, 1891. This attack followed immediately on a visit to Melbourne. and ran the course of an acute pneumonia with pleurisy, from which he partially recovered, but did not regain his former health, and had a persistent cough, accompanied by slight attacks of hæmoptysis, and in the intervals blood-stained sputum.

He first came under my observation in April, 1892. At that time my notes state that there was marked dulness at the right apex, with prolonged expiration and a dull area over the right base. Coarse mucous râles were present all over the right lung. Left lung was healthy ; tongue somewhat furred. Patient's friends state that every night he coughs during sleep, and expectorates a considerable amount of blood, the bed-clothes being stained in the morning. The patient appears to have become used to the cough, so that he sleeps through it. Physically he is well-built, but spare.

He was put on a mixture containing ext. ergot. liq. and syr. prun. virg., and when I next saw him, about one month later, the physical signs were much improved, and the bleeding had quite ceased, though the dulness was still present. His appetite had improved, and altogether he stated that he felt much stronger, and was able to do his work.

This condition persisted for some months until September, 1892, when he had a relapse. I did not see him at this time, but the cough appears to have been very troublesome, and the sputum deeply tinged with blood, and he had one or two severe attacks of hæmoptysis. He rallied, however, and when I saw him in November his condition was much the same as at the last examination in May.

In December he had a severe attack of coughing, and, as he described it to me, there was a sudden rush of fluid, and he felt as if he were going to suffocate, when he finally expectorated a "skin" about the size of a large peach. I then asked him very particularly whether he had ever had any such attack previously, or had expectorated any "skins" similar, and he said he had had a similar attack shortly after he was first laid up (in October, 1891). He also said that about three months before his first illness two pigs had been killed on the station, and that they were both full of hydatids, and that the "skin" he had expectorated was just like one of these. I told him to forward me the next "skin" he brought up, which he did about a fortnight later. I showed this to Dr. Ochiltree, who agreed with me that it was undoubtedly hydatid.

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* Reprinted from the *A. M. Gazette.*

In January I again saw him, and then persuaded him to come to Ballarat and put himself under treatment, as he was losing weight, and still had the persistent and hacking cough, with frequent attacks of hæmoptysis more or less severe.

He came to Ballarat on the 24th January, 1893. On the 25th Drs. Ochiltree, Morrison, and myself held a consultation, and we decided to make an exploratory aspiration. Chloroform was administered, and a Potain's needle introduced in the seventh interspace, and in a line with the angle of the scapula, when a small quantity of thick, stinking pus with shreddy membrane was withdrawn. This slight operation set up most violent coughing, with hæmoptysis, which became most alarming, about a pint and a-half of pure arterial blood being expectorated. Ergot was given, and the bleeding gradually stopped. Two days, and again four days later, he had a return of the hæmoptysis, in each attack about a pint of blood being expectorated. I should state that at this time the hydatid fœtor was markedly present.

Absolute rest in the horizontal posture was strictly enforced, with light, non-stimulating diet. The physical signs showed some improvement, becoming more vesicular in character, but expiration at apex and over the seat of aspiration was prolonged, and in the latter position the auscultatory sounds were cavernous over an area which could be covered by a crown piece.

During this period he expectorated small pieces of shreddy membrane, which, under the microscope, presented the characteristic laminated appearance. The sputa and breath had the fœtor which is almost pathognomonic of hydatid. No hooklets were now, nor at any later period, found, after careful and repeated examinations.

Patient progressed satisfactorily until the 17th February, when, without any known cause, a most alarming hæmorrhage came on, the bleeding being bright and arterial, and which only stopped when a condition of syncope was induced, leaving him to all appearances moribund. He rallied, however, and for the next ten days improved very much; appetite good; cough not troublesome; pulse regular; temperature varying from 98° in the morning to 99° or 99·4° at night; slept well.

Treatment during this period was free depletion of the bowels with saline purges; light, non-stimulating diet and hæmostatics by the mouth, with hypodermic injections of $\frac{1}{150}$ gr. ergotinin every four hours.

On the 26th February the sputum was streaked with blood. During the next three days slight hæmorrhages occurred, and on the 2nd March he had an attack as alarming as the one fourteen days before, and which only ceased when the blood pressure was lowered to such a degree as to induce a condition of syncope. The patient's condition was so critical that we requested further advice.

Sir T. N. Fitzgerald saw the case with us on the 5th March, and he considered the radical cure by resection the only course to pursue. At the same time the extreme danger of the operation in this case was pointed out clearly to the patient, and he was ready to take the risk.

On the 12th March Sir T. N. Fitzgerald operated, assisted by Drs. Ochiltree, Morrison, and myself. Chloroform was administered; $\frac{1}{3}$ gr. morph. and $\frac{1}{150}$ gr. ergotinin was given hypodermically. The incision was made over the seat of the former aspiration, and the operation proceeded on the usual lines until the pulmonary pleura was clearly in view. A fine exploratory needle was then introduced, and when found to be in the cyst was withdrawn and a Weiss' dilator substituted. Almost instantaneously the patient expelled by a cough the decomposing remains of the cyst, but this was followed by a rush of blood both from the wound and the mouth. An attempt was made to control the bleeding by plugging, &c., but unavailingly, and the gush of blood was so severe that the patient was *in articulo mortis* in a very few seconds.

By permission of the friends an examination was subsequently made, and it clearly showed the cause of the bleeding, and how death from bleeding must have ensued sooner or later. The fibrous sac was found to be incorporated with one of the pulmonary arteries, and in this way the vessel was kept patulous. About a quarter of an inch from the artery a large bronchial tube opened, and was incorporated with the sac in like manner to the blood-vessel. This condition then clearly explained the repeated and serious hæmoptyses, which had been such a feature of the case.

There was also a second hydatid cyst at the upper and back part of the lung. The physical signs over the dull area in this portion of the crest wall were such as to load us to the conclusion that the dulness was due to consolidation following on the initial attack of pneumonia. The flattening under the clavicle and the auscultatory sounds led us to this conclusion.

No doubt anything causing a disturbance of the cyst wall or its contents set up bleeding from the artery, which was only checked by clot filling up the cyst, combined with the resistance of the chest wall. As soon as this was removed by the resection and expulsion of the cyst contents, uncontrollable hæmorrhage occurred. Sir T. N. Fitzgerald stated that he had operated in over 100 cases of pulmonary hydatids, but had never experienced a case similar where a large blood-vessel opened directly into the cyst, and was kept from retracting by the fibrous membrane. Cases have been recorded in which death has occurred from hæmoptysis caused by degeneration of the vessels on its walls, but in this case attacks of hæmoptysis had occurred comparatively early, and certainly before any suppuration was present. Dr. Davies Thomas, in a paper on "Operative Treatment of Echinococcus Cysts," says: "I am convinced that patients suffering from moderately-sized or large pulmonary hydatids are most safely treated by free openings into the sac and immediate removal of the parasite. . . . I am disposed to operate upon any case of pulmonary hydatid in which there is proof positive of the nature of the disease and its precise locality, with one proviso—viz., that there shall be a distinct, even though limited, area of dulness on the chest wall; when, on the other hand, the alteration of the percussion note is to the production of a tympanitic note, which seems to me to indicate compression of lung which still contains air, I should feel disposed to puncture with the aspiratory needle."

Speaking of puncture, Dr. Thomas says: "The results of aspiratory puncture show only about half the ratio of deaths as compared with puncture with an ordinary trocar. The mortality rises with the number of tappings required. The best results are met with in living juvenile echinococci of moderate size; the worst in old, degenerated, or suppurated cysts."

This paragraph sums up the position shortly. No doubt we have all met with juvenile living echinococci which have undoubtedly been cured by a single aspiration, but when suppuration is present I am confident no treatment will do any good except free incision.

In the present case Drs. Ochiltree, Morrison, and myself were deterred from operating owing to the exceptionally grave symptoms present, in which opinion Sir T. N. Fitzgerald quite concurred, but, as he said, "if left alone death is certain; the operation will give him a chance of recovery." This case appears to me one of very great interest in the history of hydatid tumours. The history of the attack beginning suddenly and running the course of an acute pneumonia, together with the absence of any characteristic expectoration (with the exception of the one occasion which he said nothing about till months later), all led one to place the symptoms as due to consolidation following on the acute inflammation.

Even had the diagnosis been clear the result of any operative treatment earlier could not have been otherwise than fatal, for the cyst appears to have formed in, or at least involved, the artery from its inception ; and in this way attacks of hæmoptysis occurred at intervals all through the illness.

A case such as this brings home to one the gravity of pulmonary hydatids, and when one considers the vascularity of the lungs it is a matter of surprise how the larger blood-vessels are not more often involved by such an octopus growth as the *Echinococcus.*

TREATMENT OF HYDATID BY A MODIFICATION OF THE CLOSED METHOD, WITH ILLUSTRATIVE CASES.

By John Ramsay, M.B., Ch.B. Melb.
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The treatment of hydatid by the closed method after complete removal of the cyst or cysts in non-suppurating cases is now supplanting the older method of incision and drainage according to the method of Lindemann. The methods of Bond or Hamilton Russell are at our disposal, if we are to adopt the more modern treatment. But the question arises: Are the ultimate results that are attained by the closed method as at present practised better than those obtained by the open method ? Carefully compiled statistics might help us to decide this question, but it is doubtful whether the number of cases so far treated by the closed method is sufficient for us to base any deductions upon. The very successful cases treated by the newer method find their way into the journals, but those followed by a fatal result are not often tabulated, in spite of the fact that they do occur. Suppuration will occur in a certain proportion of cases, and, although in the majority the abscess will ultimately point in the wound, cases have occurred where such was not the case, and a fatal result has followed such an unfortunate complication as a general peritonitis consequent on the dropping of the sac back into the abdominal cavity. That this suppuration consequent on operation is not always the fault of the surgeon's technique gains support from the undoubted evidence that we have of the possibility of cavities in close proximity to the bowel becoming infected by such organisms as the *bacillus coli communis*, which have in some way gained access to the part. The traumatism necessarily connected with such an operation as the radical cure of hydatid, the close proximity and possible adhesion to bowel in hydatids of the abdomen and its organs, the collection of blood and other effused fluids about the part, etc., might all tend to some predisposition towards infection. Greater care and accuracy in asepsis diminish the number of these suppurating cases, and lead to more satisfactory results.

The greater frequency of infection of the liver by hydatid also opens the question as to the influence of the bile on this problem. Is the bile likely to prove a source of infection to the cavity of a hydatid ? Researches that have been made are not unanimous as to the absence of micro-organisms from the bile. Naunyn found normal bile* to be sterile in the four cases that he examined. Hunter found it so in two out of three, Gilbert and Girode in six out of eight cases. These observations apply to normal bile. When, however, stagnation occurs from some causes, micro-organisms are commonly found. This statement is strongly supported by Naunyn in his elaborate work in connection with cholelithiasis, the *bacillus coli* being the offendor, and infection occurring by spread from the intestine along the bile ducts.

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* Clifford Allbutt's System of Medicine, vol. iv.

When we consider the intra-cystic pressure exerted by hydatid fluid it seems probable that some stagnation of bile should exist in the vicinity of the swelling, although the part played by the fibrous tissue of the adventitia cannot be left out of consideration. As secretion of bile takes place under low pressure (about 15 mm. Hg. in the dog, Foster), the amount of this pressure is probably less than the intra-cystic pressure of hydatid fluid, estimated by Thomas as equal to about 21 mm. Hg. One would imagine that this should lead to a little jaundice, but how rarely do we see it in non-suppurating hydatid of the liver! Possibly the very gradual growth of an elastic swelling like this is the explanation. When the bile enters the cavity after evacuation of the hydatid, it may possibly then carry with it pyogenetic micro-organisms and cause septic mischief.

Of course in some cases of hydatid of the liver, and especially where it is situate on or close to the surface, bile may not enter the cavity. Experience of Lindemann's operation shows that most cases exhibit a bile-stained discharge within a few days of operation. Again, with regard to hydatids affecting the lung there is always the possibility of infection by means of the respiratory passages. As is well-known, bronchi of varying calibre frequently open by their distal ends into the cavity of an hydatid of this organ. The laboratory experiments of Buchner and others* show the possibility of producing various diseases of the lungs by the inhalation of sprays or powders containing various pathogenic organisms such as the *bacillus anthracis* producing a general infection as well as a pneumonia, or the *pneumococci* producing pneumonia, etc. Hence this may help to explain why certain hydatids of this organ after operation by the closed method may proceed to suppuration.

In all the organs of the body we have the possibility of infection by the transport of micro-organisms in the blood. These, finding some "locus minoris resistentiæ" in the region of the operation, may proceed to multiply in the excellent culture medium provided by the injured tissues, the extravasated blood, and other effused albuminous fluids.* The well-known experiments of Becker, who injected *staphylococci aurei* into the circulation after fracturing the bones of a rabbit, show that suppuration tends to occur in the injured part. Clinical experience also teaches the frequency with which such organisms as the pyogenetic cocci, tubercle bacilli, etc , infect parts that have been the site of some preceding injury.

Having thus shown the possibilities of infection in a proportion of these cases with more or less danger to the patient's life, the question of minimising this risk and carrying out the principle of a closed method presents itself.

We are all conversant with Lindemann's operation, with its usual consequence of more or less suppuration, profuse discharge, abundant dressing, lengthy convalescence, and so on. The more rigidly asepsis is practised the more satisfactory is the result. I am, however, unaware of any extensive statistics of cases where, after a day or two's discharge, union of the edges of the wound occurred, as with strict asepsis should be considered possible. The closed methods of Bond and Hamilton Russell in allowing the sac to drop back after evacuation of the hydatid introduce an element of danger that is unnecessary.

An operation of the nature somewhat of a closed Lindemann's operation I have carried out in seven cases during the last two years, and, as this has proved satisfactory, it deserves a trial.

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* Sternberg's Bacteriology

The operation is carried out as follows in a case of, *e.g.*, hydatid of liver presenting in the abdomen.

After the usual incision the cyst is exposed and sponges are packed around to shut off peritoneal cavity. The cyst is then tapped with a branched ascites trocar. An assistant holds the lax cyst wall with fingers or volsella, whilst two stout silk or silkworm gut sutures are passed transversely through the cyst wall at the upper and lower ends of the wound. These may be passed before tapping the cyst, but less leakage occurs if done later. These are gently drawn on to hold up the cyst, whilst the scissors is used to enlarge the wound, taking care not to cut them. The cyst is then removed in the usual way with fingers, forceps, spoon, stream of lotion from irrigator, etc. An attempt should be made to remove it whole if possible, as there is less danger of anything being left behind. Daughter and grand-daughter cysts, if present, are removed. The cavity is then thoroughly irrigated with some antiseptic or normal saline solution, in order to remove all *débris.* This fluid is thoroughly removed by altering position of patient, and also by small sponges on forceps. Oozing is soon stopped by hot solution. Hæmorrhage, as a rule, does not give any trouble. (This method would not be adopted unless hæmorrhage could be readily controlled.) After drying out the cyst, its cut edges may be approximated by a continuous catgut suture, though this is hardly necessary. The ends of the silk sutures previously passed through the cyst wall are threaded on needles, and passed through the entire abdominal wall on each side of the wound from within outwards, but are not tied until the rest of the sutures have been passed. Any approved method may be used for closing the abdominal wound. Suturing in layers is usually adopted: continuous catgut for peritoneum, interrupted silk for musculo-aponeurotic layer, and intradermic suture of silkworm gut for the skin. The deep tethering sutures are now tied, and the ends left long. The cyst wall is thus held up under, and approximated to, the abdominal wall. The wound is usually dressed with shreds of cotton wool and iodoform collodium. This is advantageous in allowing of the intradermic suture being pulled out without disturbing the collodium dressing.

If the temperature, pulse, or other signs indicate that there is not any severe septic mischief, the deep tethering silk sutures may easily be removed on the second day, and thus give the cyst wall an opportunity of dropping away from the abdominal wall. However, it is difficult to say whether the adhesions, which soon form between peritoneal surfaces, will allow of this happening. If in any doubt at all as to an aseptic result, these stitches may be left for a few days before removal. The long ends of the tethering sutures are useful guides, and access to them is easily gained by picking through the collodium dressing.

It is probable that if the tethering sutures are removed sufficiently early the cyst wall, if no old adhesions are present, will drop away from the parietes. In most of the cases, however, if the sutures are left for more than two or three days, more or less firm adhesion will have occurred, and the chance of its dropping back will be gone. The longer they are left *cœteris paribus*, the firmer will be the adhesions. In all cases of doubt as to the aseptic progress of the cases, they should be left until the doubts are dispelled.

If sepsis occurs they may be removed at the end of a week, the wound and cavity having been opened at a spot between them, and a drainage tube inserted as soon as the condition is diagnosed from the pulse, temperature, progressive distension of the sac, and other septic symptoms.

In all cases after the operation the collapsed cavity undoubtedly becomes more or less distended by effused fluids, and this in the satisfactory cases will progressively diminish until there is no sign of it at the end of a week or two. In the infected cases the sac becomes progressively distended, and the abscess will probably point into the wound if allowed to do so. In the only case in which this infection was evident, the sac was emptied by opening up the wound before this occurred.

The following is a brief *résumé* of the cases thus treated :—

Case I.—A.B., æt. twenty-two, female, single. Hydatid of right lobe of liver, presenting in abdomen. Operation on 4th May, 1898. Soft, filmy adhesions present at the upper part of incision. Crowds of daughter cysts were evacuated. Cavity about six inches in diameter. Sacculations outward of the cavity, in which lay small cysts. These were carefully removed. Cavity thoroughly irrigated and then dried.

The temperature was up and down for ten days; highest point 102° on fourth day; normal after eleventh day; pulse steady the whole time, 80 to 90 per minute; deep sutures removed on the seventh day; was walking about on the thirteenth day; twelve months after the operation she was quite well; no sign of any swelling in region of wound.

Case II.—D.B., æt. four years, female. Small hydatid under left lobe of liver on which it was movable. Well-marked expansile pulsation could be felt in it, when in its usual position, but on pulling it to one side communicated pulsation was still present though not expansile. Abdominal aneurism was difficult to exclude, especially as she dated the swelling from a blow on the epigastrium through falling on the edge of a stool three months before. Unilocular cyst containing about 2 oz. of fluid. Slight adhesion to omentum; none to abdominal wall; operation, 29th November, 1898; no sutures used to close incision into the cyst; abdominal wall not stitched in layers; deep sutures removed on second day, as temperature was only 99° and pulse was not unusually rapid.

Everything went well until her discharge from hospital on 28th December. Has been quite well ever since.

Case III.—R.R., æt. forty-seven, farmer. Admitted 29th November, 1898. Retro-peritoneal hydatids extending down into pelvis; large cyst of right lobe of liver. Four months ago he noticed the swelling of the abdomen. He has got thinner, weaker, and older-looking. Feels full about abdomen, suffers from flatulent indigestion symptoms. Sleeps badly. Urine has been paler and more abundant lately. For two months pains resembling those of sciatica have troubled him. Sight good. Skin was very dry looking and scaly. Urine 1010, acid, no albumen, hyaline casts of large size. Round swelling in right abdomen from rib margin down into right iliac fossa moving with respiration, large irregularly rounded elastic mass in lower abdomen, especially to the left side and extending deeply into pelvis in front and to left of rectum and pressing bladder downwards, pulse weak and rapid, suffering from nausea and vomiting. He was in a very weak condition suffering from renal insufficiency, only 12, 22, 27, 37, and 39 oz. of urine being passed on consecutive days, of low gravity—1008-1012—with occasional trace of albumen and hyaline casts.

As the general condition was not improving after a week, it was decided to evacuate the pelvic hydatid, as it might be producing some of the renal trouble by pressure on ureter. Through a medium abdominal incision the retro-peritoneal hydatid was incised. The sigmoid flexure was stretched across the front of it, and it passed down beneath this to left of rectum. Dozens of small cysts of various sizes were evacuated by flushing, etc. After drying, the wound was treated as before, but silkworm gut sutures were used through whole abdominal wall. Large hydatid was felt below liver adherent to its under surface.

Next day the temperature was 98·8°, pulse 104 fair volume, and urine contained a good deal of albumen. After that he gradually passed into a uræmic condition, temperature usually normal, up to 99·8° on the fifth day after operation. Pulse gradually increasing up to 110-126, and of variable tension and becoming weaker. Skin very dry and harsh, later scaly; eyelids bleared. Tongue getting dry and brown; talking nonsense at times; sleeping very badly, in spite of various sedatives. The wound healed soundly by first intention; no swelling could be detected in the part operated upon, and no abdominal distension. He was treated by diaphoretics, vapour baths, pilocarpine occasionally; and later, diuretics, but all without avail, for he died ten days after the operation, gradually sinking into a worse and worse condition. No *post-mortem* was allowed. It was apparently a case of uræmia, the operation leading to a more acute condition in some way. He had been a heavy drinker in his youth.

Case IV.—R.F., boy, æt. ten years. Hydatid of right lobe of liver containing about a pint, bulging costal margins forward. Operation 9th February, 1899; incision through $\frac{3}{16}$-inch of liver substance. No adhesions present. After irrigating and drying cavity, the hepatic incision was closed; and, after passing the tethering sutures, the abdominal wall was closed in layers. Temperature rose to 101·8° in evening, fell to 99° next day, and gradually reached normal. Tethering sutures removed on the third day. Patient getting about in a fortnight after operation, wearing a bandage. No sign of swelling. No complaint of any kind. Was quite well six months after operation.

Case V.—V.F., girl, æt. eight years, cousin of Case IV. Hydatid of right lobe of liver; of capacity about 1 pint. Temperature 99·8° on admission. Operation, 24th February, 1899. Temperature on day following was 100·4°, and it remained between 99·6° and 100·6° for eleven days, when she was found to have developed scarlet fever. The sac rapidly filled with fluid, and became more and more tense until it was re-opened on the thirteenth day, and a good quantity of bile-stained, yellow, and purulent fluid evacuated. The tethering sutures were removed on the sixth day; a little clear bile-stained fluid leaking from the stitch-holes. The cavity was easily opened, and a rubber drainage tube inserted. Healing gradually occurred, and she was discharged about the middle of May.

This was the only case in which suppuration occurred. The source of infection is an open question. Possibly the onset of the scarlet fever may have had some influence, if it were not due to the more usual causes.

Case VI.—E.S., girl, æt. seven years. Hydatid of left lobe of liver containing about half-a-pint of fluid. Operation, 14th July, 1899. No adhesions present. There was exogenous formation in connection with the hydatid cyst, so that care was necessary to clear out the sacculated pouches. No suture used for the liver incision. Everything well on the second day; tethering sutures removed. No marked distension of cyst cavity was noticed, and child left the hospital on 1st August wearing a bandage as support.

Case VII.—This case of hydatid of right lung was treated by the closed method, but not by the same operation as the preceding ones.

E.D., female, æt. eighteen years. Admitted 16th October, 1897. Twelve months previously she had a hydatid at base of right lung aspirated, as she was suffering from severe dyspnœa, and was not considered strong enough to stand the radical operation. During the aspiration she began to cough up, and nearly suffocated from the filling of the air passages with the clear hydatid fluid. For three days her condition was precarious, pleurisy setting in after a rigor. A small patch at right apex had broncho-cavernous breathing and pectoriloquy; tubercle bacilli were found in her sputum on several occasions.

Her health improved after this, but for past six months she had pain in the right side of chest, and at intervals spat up watery fluid in variable quantity; streaks of blood, and sometimes "grape skins." No night sweats. No loss of flesh. The fulness and deficient expansion of lower part of right chest, relative dulness and diminished vocal fremitus in this region, with marked rubbing, friction, and crepitations over the dull area, led to an exploratory puncture with needle, which proved unsuccessful till the sixth attempt, when clear hydatid fluid was tapped in the fifth interspace, just outside the nipple. Breathing at right apex was broncho-vesicular. No tubercle bacilli were found in her sputum, though examined several times. 16th November, 1897: Under chloroform, about 3 inches of the sixth rib were resected over the spot where the fluid was tapped. Soft pleuritic adhesions, few in number, were found. The lung was fixed by four silk sutures to the deeper parts of the chest wall; the cyst was opened at about a quarter of an inch from the surface of the lung through an incision, the unilocular cyst and contents removed (about three-quarters of a pint of fluid). The cavity was irrigated with 1 in 60 carbolic and dried. A few more sutures were placed to fix lung to chest wall and shut off pleural cavity, the sutures being buried. Skin, subcutaneous tissue, and muscles were approximated by silkworm gut and horsehair; dressed with sublimated wood-wool wadding. On the second day temperature reached 101°; she had very little pain and felt well. The wound healed soundly by first intention; there was no discharge from it. Temperature soon became normal, and she was discharged quite well. Eighteen months later she was still in good health. (The presence of tubercle bacilli in the sputum of cases of hydatid of the lung may tend to mislead one. This is the second case on which I have operated in which tubercle bacilli were undoubtedly present at one time. In the other case—a suppurating hydatid—no tubercle could be found three weeks after the hydatid operation. Both of these cases I have seen eighteen months and two years respectively after the operation; there is no sign of tubercular mischief, and their general health is good.)

Case VIII.—A few days ago I assisted Dr. Pardey in a case of hydatid of the peritoneum in a woman of about forty. It was situate in the lesser peritoneal cavity, and adherent to coils of intestine. As it could not be peeled off the coats of the bowel without danger to the gut, it was evacuated and treated in the method previously described. Temperature kept normal, the pulse natural, and the patient comfortable. The tethering sutures were removed on the third day. The parts appeared well fourteen days after operation, when she was allowed to get up. On the fourth day after operation a slight rise of temperature to 100·4° occurred. This lasted with remissions for a few days, until the bowels were regulated. Dr. Pardey has kindly given me permission to make a note of this case.

The particular classes of non-suppurating hydatids for which I consider this method of operation most suitable are those arising in, or in close connection with, the thoracic and abdominal viscera, particularly the latter, where there is danger of peritoneal infection, should a septic condition result. The usual adhesions present in pulmonary hydatid render the precautions of tethering sutures unnecessary when the pleural cavity is shut off and the adhesion firm.

Where no adhesions, or very incomplete ones, exist, it would be safer to insert sutures to bring visceral to parietal pleura, and thus minimise, if not prevent, the risk of empyema, should suppuration occur in the sac. For subcutaneous, intra-muscular, intra-osseous, and similarly situate hydatid, the ordinary closed method is always available. For free and slightly, adherent peritoneal or subperitoneal cases, and free intracranial hydatids, removal of cyst and adventitia, if any, may be accomplished.

Suppurating hydatids, as a rule, cannot be treated in this way. In some cases, however, cyst and adventitia may be shelled out whole, and union by first intention result. Until, however, we discover some means for the sterilisation of large suppurating sacs, the method of Lindemann will probably be the operation of election in these cases.

One large cyst in a girl aged fifteen, containing 5 pints, and arising in the region of the urachus, was treated by enucleation, like an ovarian cyst. Having been tapped, the adventitious sac was peeled off fairly firmly adherent intestines, until a small thin pedicle was formed at the apex of the bladder, in the region of the shrivelled urachus. It was then ligatured and removed. The ovaries and tubes were seen in position, pressed downwards by the tense cyst. The case did well, the only trouble being caused by suppuration about the ligatures put on the pedicle. This soon came away, and the wound healed. In this case the clear, pellucid cysts containing hydatid fluid, that were floating in the purulent fluid of the mother cyst, rather deceived the operators, as one commonly expects daughter cysts to have opaque or opalescent walls. No hooklets were found either in the fluid of the mother or daughter cysts, in spite of frequent examinations. A section of one of the clear cyst walls, however, revealed the characteristic structure of hydatid membrane.

Of the seven cases of hydatid treated by this modification of the closed method, union by first intention occurred in six, suppuration in one, and that one a case in which scarlatina developed eleven days after the operation, many cases of scarlet fever being at that time present in the city. In one of the cases death occurred ten days after the operation, but as the patient was suffering from a uræmic condition, which became progressively worse after the operation, and as the part operated upon seemed well and without sign of swelling in the region of the cysts, it would, perhaps, hardly be fair to ascribe the fatal result to the operation alone. It is, of course, rather early at the present stage to say that the patients have been cured of their hydatids, as it is little more than fifteen months since the first of these modified operations was done, and it is not impossible that some minute cyst may not have been removed and may develop at a later date. This possibility is not uncommon, whatever form of the closed method be adopted. Of this, a striking case presented itself in a woman of forty-five, who was operated upon in the right side of epigastric region in November, 1897, in Queensland, for a non-suppurating hydatid of the liver. The closed method was adopted, and the wound healed by first intention. Seven months later she was operated upon in Sydney for a suppurating hydatid, the anterior end of the eighth rib on the right side being excised, and the cavity healing after three months. It is quite possible that this second hydatid was quite separate from the previous, but the opinion of the operator was that some minute cyst had been left behind and that suppuration had occurred in it. There has been no trouble since then.

To sum up, we may consider the disadvantages and advantages which this operation possesses.

Disadvantages.

1. It is quite possible, especially where there are crowds of daughter cysts, and especially also where exogenous cyst formation has taken place, that some small colony may be left behind and develope later. With care in thoroughly irrigating and sponging, as well as with careful examination of the wall of the cavity as far as possible, this risk is minimised. However, recurrence may apparently take place through no fault of the surgeon where an adjacent cyst gradually develops and occupies the site of the previous hydatid. This disadvantage is common to all modifications of the closed method, and even Lindemann's operation is not free from it, though the latter gives opportunity for a contiguous hydatid to rupture in the direction of least resistance into the cavity occupied by the drainage tube.

2. The collection of blood and fluid from the blood-vessels in the cavity provides a very suitable culture medium for organisms that may be present in close proximity, as in the case of the bowel, or that may be circulating in the blood-streams, and thus suppuration follow. This is most difficult to avoid, but care during the operation in arresting all hæmorrhage from cut surfaces and from the cavity wall will help. Traumatism should be as far as possible avoided, gentleness in handling the sac and in clearing out the cavity probably leading to less effusion. The use of powerful antiseptics in flushing out the cavity should be avoided as causing unnecessary irritation, normal saline solution or weak antiseptic lotion being used in their stead. A certain amount of effusion is inevitable. The entrance of the secretion of the organ, *e.g.*, the bile, may also be a disturbing element in the progress of the case.

3. At the present time a certain percentage of operation cases for hydatids seems to proceed to suppuration, no matter what operation is performed. The open method can, I think, claim a larger percentage of suppuration cases than can the closed method, but in the latter if not carried out with the modification suggested in this paper, or something similar to it, the immediate risk to life is greater. An interesting case in connection with this point presented itself about three years ago. A man, aged thirty-two, was operated upon for a non-suppurating hydatid of the spleen, Lindemann's method being adopted. The wound healed soundly after two months; he regained his health, and recommenced work. Three months later he got what he called an attack of influenza (?), anorexia, chilliness, fever up to 103°, sweatings, cough, scanty sputum, and pains in the right side of the chest with dyspnœa; he had pain in the pit of the stomach at the onset of the illness, but not since; no vomiting or diarrhœa, no jaundice. He wasted rapidly, and when examined four weeks later he was found to have all the signs of right pleural effusion. An exploring needle having found turbid greenish-yellow fluid, the pleural cavity was drained through an incision at base of right lung. His temperature fell for three days. He then reverted to his old condition with rigors followed by sweatings, big remissions of temperature (temperature, 99·8-104·8°), pains and tenderness over lower half of right chest, and gradually sank in the "typhoidal" condition; he died eleven days after the drainage of the pleural cavity. The *post-mortem* revealed a very interesting condition. In the right pleural cavity there was a little of the turbid, yellowish-green fluid containing numerous pus cells and large spherical nucleated cells, probably from the serous membrane. Fairly recent lymph covered the base of the partly collapsed right lung, and caused adhesion to the upper surface of the diaphragm. The heart was soft and flabby from fatty degeneration. Liver a little larger than natural; on its upper surface thin layers of fairly recent lymph caused adhesion to the under surface of the diaphragm, especially over an area the size of the palm of the hand, which was darker, more congested, and with petechial extravasation in it. On cutting through this patch soft suppurating yellow clots were found in the small branches of the portal vein adherent to the roughened inner coats of the vessels. On opening up the left division of the portal vein a soft yellow purulent clot was found projecting from the mouth of one of the large branches into the lumen of the vein. This large branch supplied the irregularly wedge-shaped congested area, the liver tissue being very soft and friable and pus could be squeezed from it in small quantity. On searching for a cause, the spleen was much enlarged and firmly adherent to the old operation wound, the cavity of the hydatid which had involved the convex surface of the spleen had entirely disappeared, and all that remained was a firm cicatrix. The spleen was also adherent to the stomach (adjacent); and on separating these firm adhesions a cavity containing about 2 oz. of thick, greenish-yellow pus and with ragged walls was opened. No clots could be found in the splenic vein or its branches. This case is of interest because of this uncommon sequel to an operation for splenic hydatid, the probability being

that this deep portion of the hydatid cavity became shut off, the greater and more superficial part having healed. The man meanwhile remained well until this suppurative pyle-phlebitis occurred with a consequent suppurative pleurisy by extension. Probably this pyle-phlebitis resulted from an embolism or bearing of septic organisms from the localised abscess. No bacteriological examination was made.

4. From the pathological condition of more or less adhesion between the site of the cyst and the parietes, it is possible that adhesion of the bowel or some obstruction to the bowel may result. Even in Lindemann's operation it is not very common for intestinal obstruction to occur as the result of the strong adhesions. Still one must consider it a possibility. This possibility, too, may be greater in those cases where the tethering sutures are removed fairly early, as we should then expect that the weak adhesions might be drawn out into long and thin bands as the organ is allowed to drop back into its original position. Of these possibilities I have had no practical experience.

Advantages.

1. The operation is nearer the ideal than is the older method of Lindemann.

2. The use of tethering sutures helps to safeguard the patient against such dangers as peritoneal infection, should suppuration occur in the cavity, and is thus an advantage over the other closed methods which allow the cyst cavity to drop away from the region of the wound, if adhesions are not present.

3. Suppuration is less likely to occur in a closed wound than with the open operation, where every change of dressing is a possible source of infection, especially if asepsis is imperfectly carried out.

4. The quantity of dressing required is small; the period of convalescence is greatly shortened; the comfort of the patient is promoted. Economy in every sense is, therefore, exercised.

In short, the operation combines all the advantages of the closed method (except, perhaps, the slight parietal adhesion) with the safety of the Lindemann's operation.

Note.—I am unable to find the exact counterpart of the above modified operation in the text books or journals at my disposal. Greig Smith ("Abdominal Surgery") mentions Knowsley Thornton as having used the closed Lindemann's operation with success. The other closed methods of Bond and Hamilton Russell are well known.

THE TREATMENT OF HYDATID CYSTS SITUATED LOW IN THE PELVIS.

By W. Moore, M.D., M.S. Melb.

Australian surgeons are well aware of the fact that hydatids may be met with in any part of the body. They are occasionally found low in the pelvis, and the best method of operating in these cases has, perhaps, not yet been decided. In women, no doubt, the exact nature of the disease will not, as a rule, have been determined until it is revealed by an abdominal section. Such was the case in a patient under my care. But in men the diagnosis is commonly made, and the surgeon has to decide what route he will take in reaching the cyst. In these cases the cyst is frequently situated very low, and difficulty in dealing with it is apt to be met with. The question of the best route in women may at once be got rid of, not because it is so easily settled, but because it is really not merely a question of the best route by which to attack a hydatid in the pelvis, but because that small question, very seldom asked, is involved in the much larger one of the vaginal or the abdominal route in gynæcological cases generally.

But these cases of hydatid when met with in men are much more difficult of treatment, and the *dark route* from below is the one generally adopted. These cases are frequently extra-peritoneal in their situation, and as a rule they are placed between the bladder and the rectum. I have found the bladder lifted to a level with the umbilicus by such an hydatid.

Formerly the great object of the operating surgeon was to avoid opening the peritoneum; hence the perinæal route was always selected. It seems to me that the time has now come when this position should be re-examined, and it should be considered whether, in the great majority of these cases, operation through the peritoneum should not be the selected method. The introduction of the Trendelenberg position has made a marvellous difference in the ease and accuracy with which we can deal with the many pathological conditions that are found low in the pelvis in women, and I am sure that Trendelenberg's position is eminently suited to enable the surgeon to deal with precision and accuracy with these hydatids when situated low in the pelvis of men. Again the introduction of the intra-peritoneal method of dealing with hydatid cysts after the removal of their contents enables us to treat in the most satisfactory manner these somewhat difficult and troublesome cases. Thus, by the application and conjunction of these two comparatively recently introduced procedures, a distinct advance is made in the treatment of hydatid cysts situated low in the pelvis in men. Of course it would not be right to say that all cases should be treated in this way; but I do maintain that the majority should be so treated.

The objections to the *perinæal route* are that the space in which to work is very restricted, and that in this limited space are contained important organs and structures, which may readily be seriously injured. Thus the operation through the perinæum is both difficult and dangerous. If the cyst is at all high in its situation, it may be very difficult to reach, and the urethra, the bladder, or the rectum may be injured in the endeavours to do so, for here, much more than in pelvic operations by the vaginal route in women, the surgeon has to work in the dark. Then the cavity cannot be cleansed thoroughly and precisely, and asepsis cannot subsequently be maintained with any degree of certainty. Consequently drainage has to be resorted to, and as sepsis, even of a very mild form, is very apt to occur, the discharge is kept up for some considerable time.

On the other hand, in the Trendelenberg position, especially if a small electric light be used, the cyst can be cleansed with absolute accuracy, and then, either after suturing or without suturing, dropped back into the pelvis and the supra-pubic incision closed, with the same care that the surgeon is in the habit of exercising in closing his abdominal incisions under the most favourable conditions. In this way the operation is performed with greater accuracy and with less risk of injury to important structures, but the main advantage is in the much greater rapidity of healing. It has the further subsidiary, but by no means unimportant, advantage that it enables the operator to detect and deal with other small cysts which are not unlikely to be present.

Whilst in my opinion the abdominal operation performed in the Trendelenberg position is the better practice, since it is more thorough, more precise, and, if done with care and skill, at least as safe as the perinæal method, it must be remembered that the latter is good and effective surgery, and a considerable number of cases have been successfully treated in this way. Also, if there is evidence of suppuration this route should be adopted in preference to the suprapubic, as in either method drainage must be resorted to, and the after-effects of a slowly-healing abdominal incision are infinitely worse than those of a perinæal incision, whilst the danger to life would also probably be greater by the suprapubic method.

I am able to record briefly a case treated by each method :—

Case I.—*Hydatid in lower part of abdomen treated by abdominal section ; large cyst between bladder and rectum treated by perinæal section.* The patient was a man, H.L, æt. thirty-four, who was admitted into the Melbourne Hospital on 31st December, 1897, suffering from an obvious lump in the right inguinal region. The lump had been present for months, but only recently had it become painful. On 4th January, 1898, the abdomen was opened in the mid-line, and the cyst in the inguinal region treated by suturing to the abdominal wall. At the same time a large cyst was discovered lying at the back of the bladder, between it and the rectum. Nine days afterwards, on 13th January, the patient was again operated on. He was placed in the lithotomy position, and a curved incision across the perinæum, as recommended by Kocher, was made ; the flap was turned backwards towards the anus ; with a catheter in the urethra the dissection was continued deeply, until the resisting tumour could be felt. This was punctured with a trocar, and broken-down hydatid membrane with much purulent fluid evacuated. The cavity was well washed out with warm boracic acid lotion, and finally a glass drain-tube was inserted, and dry antiseptic dressing was applied and kept in place by means of a T-bandage. The patient had no serious trouble from the operation, and he was discharged on 28th February, 1898. The wound, however, did not heal ; even after his discharge it continued to give him trouble. I therefore had him again admitted, with the intention of scraping the sinus that would not heal. When scraping away the granulations under chloroform, I discovered a piece of rubber drain-tube at the bottom of the cavity. After its removal the sinus closed rapidly. I may say that, next to an opening in the chest for an empyema, such a wound as this is the most likely to have this interesting little calamity happen to it.

Case II.—J.F., æt. thirty-eight, was admitted into the Melbourne Hospital, not for the first time, under my care on 7th June, 1898. His abdomen had already been opened by me five times, and I had removed from various parts of the cavity just a little under fifty cysts. At this time he obviously had a large cyst in the right lobe of the liver ; there was also a large tumour low in the pelvis pressing on the bladder and rectum. On 9th June I did the intra-peritoneal operation on the cyst in the liver. From this he made a good recovery, and on 23rd June, fourteen days later, I operated on the cysts in the pelvis. After opening the abdomen in the mid-line below the umbilicus through one of the old scars, a large cyst was found behind the bladder, between it and the rectum. The bladder was lifted well out of the pelvis. There was another smaller cyst situated higher up. The patient was now raised into the Trendelenberg position, and the smaller cyst was emptied. Sponges were now packed around the upper part and sides of the large cyst ; a large piece of antiseptic gauze, one end of which was resting against the cyst, whilst the other end was brought out through the upper part of the incision and rested on the patient's abdomen, was then introduced. The cyst was opened, and its contents turned out. The position of the patient with the pelvis elevated made this part of the operation very easy, for the small daughter-cysts which tightly filled the large cavity ran out along the gauze, which acted as a bridge, even on to the chloroformist. The cyst was then well wiped out with sponges, and by means of an electric lamp it was seen to be quite empty and its inner surface clean. One or two sutures were applied in an attempt to close the opening in the cyst ; but as it became obvious that it could not be accurately closed, and as the cyst showed a very strong tendency to fold itself up, I dropped it back into the pelvis without closing it. The small cyst was treated in the same way. Then the abdominal wound was closed. The patient made a perfectly good recovery, and in less than three weeks was able to leave the hospital.

DISCUSSION ON HYDATID DISEASE.

Dr. Scott's paper on "Review of Hydatid Disease" was taken as read, being circulated among the Section.

Dr. Scott (Ballarat) stated that his paper dealt with the treatment of hydatids in the colonies. During the past sixteen years the treatment had altered, and possibly we were now approaching a stage a little more conservative; but the percentage of deaths, so far as he could judge from statistics, was still very high. In connection with operations, they should feel grateful to Dr. Hamilton Russell for his modification of Lindeman's system. As time went on Dr. Russell's method was the one they would adopt. But, personally, he would be sorry to try that method in all cases of liver hydatids. There was danger in the flow of bile. In all cases where the cyst was in the liver, bile would certainly, sooner or later, make its appearance. This was the reason Dr. Hamilton Russell's method was not altogether to be desired in all cases. Where the cyst was in the abdomen it was most desirable.

The Chairman said he was sure they were all indebted for the excellent papers read. He invited discussion.

Dr. Hamilton Russell (Melbourne) said he was glad of the opportunity of saying a few words to a meeting of influential surgeons in which his name had been mentioned—in a way in which he thought was hardly justified, and for which he was hardly prepared. The procedure advocated by Dr. Ramsay of anchoring the cyst to the abdominal wall was not an original method, and in his (Dr. Russell's) opinion had nothing very much to recommend it. He did not like any sort of a compromise between two methods like that. It appeared to him to have no advantages. In answer to remarks concerning the intra-peritoneal method with which his name had been mentioned, he was rather glad of the opportunity of saying he did not think so highly of that method as others did. When he published his suggestions some years ago—at the strong persuasion of Dr. Gardner—they were published purely on theoretical methods. With subsequent experience on his part, the theoretical ideas did not appear to him to be so well founded. He did not feel that the intra-peritoneal was a proper method to be adopted as an original measure as regards cysts of the liver and in some other situations. Personally, he did not intend to try to secure the introduction of the intra-peritoneal method for the liver except in some exceptional cases. On the other hand, it was an ideal operation in such cases as the example of Dr. Moore—of pelvic hydatids. To his mind, there would be no object, were they to be more painstaking and perfect in their methods. The objection to Lindeman's method in some cases of hydatids was that suppuration occurred in the cyst afterwards. The intra-peritoneal system was certainly fraught with a certain amount of danger. The surgeons might better turn their efforts to methods more calculated to escape sepsis. He had had only one case of pelvic hydatids; but he had seen it had caused suppuration where that method had been adopted. He was not at all satisfied in all cases where suppuration had occurred that the fault was due to infection from the bile; but he was led to believe it was from other causes.

Dr. Syme (Melbourne) said, like all hospital surgeons, he had had a good deal of experience in dealing with hydatid cysts. With regard to methods, he followed the views of Dr. Russell. He believed he was one of the first to criticise Dr. Russell's method when he introduced it, pointing out the dangers of the escape of bile, and he also reported the first fatal case that occurred from it. He concurred with him that in case of hydatids of the liver it was not altogether safe. He also concurred with Dr. Russell that the dangers of delay—or at least the disadvantages of Lindeman—could be largely overcome by attention to more perfect asepsis. He could not quite agree with him in his condemnation; in theory the method might have its defects, but in practice it had its good results. It was, too, a satisfaction to the surgeon in his operation. With regard to the treatment of hydatids in the pelvis, he was thoroughly in accord with what had been said by Dr. Moore. He was quite satisfied from what he had seen in the pratice of others that the proper method was that adopted by that surgeon.

Dr. Barnett (Dunedin) said he was pleased to hear papers read on these always interesting subjects, especially seeing that in the colonies there were so many cases of hydatids to deal with. Dr. Ramsay's method of dealing with hydatid cysts in the liver was very much the same as the method he had been in the habit of performing. It seemed to him a thoroughly good method. Dr. Hamilton Russell objected to any compromise between the intra-peritoneal method and the original method, but he (Dr. Barnett) considered that the compromise was a good one. Why should we not seek a middle way? It had seemed to him a method of treatment which was devoid of the risks of the intra-peritoneal method, and yet allowed them, in a great number of cases, to get healing after operation without that bugbear of long drainage. He had seen cases of hydatid cysts discharge bile for months, to the great inconvenience of patients, and causing trouble in every way. He had performed operations for hydatids on the liver three times, which occurred in the same person, and he would not be surprised if he had to do it a fourth time. The question of pelvic hydatids was extremely interesting—interesting in the first place as to why they should occur in such a place. The question of the development of hydatids in the peritoneal cavity was not yet explained. It would be interesting to make experiments with rabbits and other animals. He had been thunderstruck to hear Dr. Russell's representation. He had not an idea he had given the method up. He was extremely pleased with the paper when it appeared, and he thought the method had a great future, although he had never tried it. He was afraid, and waited for further records. He presumed Dr. Russell had given it up, because there had been some fatal cases as the result of the operation. He was extremely sorry that such had been the case. It seemed to him to be such an easy and rapid way of dealing with hydatids that he had hoped they would all be able to deal with cases in this way. Although Dr. Russell had decided to abandon the operation, he hoped he would yet discover some method of improving on our present methods of dealing with hydatid diseases.

Dr. Clubbe (Sydney) said the discussion served to show the advantage of bringing doctors together. He was under the impression that Dr. Russell advocated this method in all cases of hydatid diseases. He did not understand that he had given it up altogether. He thought the impression gained was that Dr. Russell really advocated it in nearly all cases of abdominal hydatids. Many of them hesitated about adopting it in hydatid cysts of the liver. He thought it was an admirable operation, and an admirable method of dealing with hydatids under certain circumstances. As to the risk of sepsis, it appeared that after a time the nurses sometimes became careless, and this was where the danger arose. They should insist upon great care.

The Chairman said he thought they had all learned a great deal from the discussion. Personally he had listened with great interest to Dr. Russell's remarks, as, after his experience, he agreed with him. As to keeping the hydatids aseptic, it must be very difficult in some cases. Recently he had been in the habit of introducing a second drain so as to keep the cavity always empty. This was of as equal importance as keeping it clean. There was another question that had interested him for some time and which had not been alluded to—that was, using a needle for exploratory puncture. Recently he had some evidence that the procedure was more or less unjustifiable.

Dr. Wolfhagen (Hobart) said he had learned a great deal from the discussion, and if he saw Dr. Ramsay he would be glad to tell him the result of the discussion.

Dr. Moore (Melbourne) said there was nothing to reply to in his paper. He was pleased with the remarks made. He scarcely thought his would have been so readily accepted as the best method. He certainly had thought it was the best, but he had been prepared to meet opposition. But he was not prepared for Dr. Russell's statement. It nearly took his breath away. Some of them had discussed this matter over and over again. In one case, on which he operated, he found on opening the abdomen that there were originally two cysts, with an opening from the smaller into the larger, and the larger extended up into the abdominal incision. He touched it, and bile almost immediately began to flow. It was a thick wall, and not a very clean cyst, and he closed the incision. Before he left Melbourne he removed the sutures, and the man was practically well. With regard to the escape of bile, he was perfectly satisfied it was due to the alteration of pressure. If the incision was closed the bile would not escape. He had operated in a large number of cases, and he had not lost a patient. The only one that gave him trouble was the first. He intended to go on until he saw some better reason for abandoning the intra-peritoneal method. He had certainly never had a death from this cause. Of course, having said that, he did not agree with Dr. Ramsay's method. He saw that in Dr. Scott's paper a reference was made to him which was not correct. He did not say that the walls of the cyst fall together. What he maintained was not that the cyst healed up, but that the cyst must be filled from the bottom. He maintained that the fluid accumulated in the cyst from below. With regard to the statement that operations had to be done over again, it was inconceivable to him that, when a hydatid was cured, that particular hydatid could fill again. That other hydatids developed was undoubtedly a fact.

Dr. Scott (Ballarat) claimed that something had been gained by his mentioning Dr. Russell's name and bringing him to his feet. He was sorry that those who had followed as his disciples should have been neglected. But he did not think Dr. Russell was such a renegade as he made himself out to be. He thought the Section was with them in holding that the intra-peritoneal method was good in some cases. He congratulated him on his attitude in connection with cysts of the liver. Personally, he would not attempt to close the incision in cases of that sort. Certainly he could not agree with Dr. Moore with reference to sewing up the abdomen and the compression, because the bringing the walls together was a safeguard in the intra-peritoneal method. He agreed with Dr. Moore in that he thought it impossible that a hydatid that had been killed could recur. He thought that was pretty well understood.

The Chairman said he could only repeat what he said before—that they had learned a great deal from the discussion. If they would allow him, he would mention a case in which hydatids could recur. Six years ago a cyst was opened by Lindeman's method, and a few weeks ago the patient came back with three hydatids in the scar.

SOME POINTS IN THE TREATMENT OF SEVERELY CRUSHED LIMBS.

By W. Moore, M.D., M.S. Melb.

In the text-books on surgery and in the lectures on the same subject, when I was a student, much space and time were devoted to the conditions accompanying compound fractures which called for amputation. These conditions were many, but it is not my intention to discuss them, and the number of amputations performed in those days was great. But since then the value of antiseptics has been more correctly appreciated, and their use much better understood, and amputations have become almost as rare in large hospitals as erysipelas. Still I think it is a question that might well be considered whether the limits of conservative surgery have been reached by the methods now most usually adopted for the saving of these badly injured limbs. In recent years I have not infrequently seen cases in which the attempt to save a badly injured limb has been followed by disaster, and I think that, in most of these cases, the method of treatment adopted has been in great part, at any rate, responsible for the result. The practice I have generally seen followed is to thoroughly cleanse the wound or wounds with antiseptic lotions, make due provision for drainage, generally by means of drainage tubes, remove all temptingly loose fragments of bone, accurately close the wounds with sutures, and then apply the usual antiseptic dressings. On more than one occasion I have heard surgeons say, "If tension occurs the sutures can be removed," or "incisions can be made." Now, my contention is that by the time tension is present the damage has to a great extent been done. I therefore not only do not close the wound or wounds accidentally inflicted, but I search about for suitable places in which to make additional wounds, and these wounds by incision are made with no sparing hand. Then, all these incisions, as well as the original wounds, are left completely open, and no other provision —by tubes or gauze plugs—is made for drainage. This line of treatment, unaccompanied by strict antiseptics, I first saw practised by Mr. E. M. James, for many years a surgeon on the staff of the Melbourne Hospital. In a case of severe compound fracture of the leg, I remember, when a student, seeing him make an incision extending almost from knee to ankle, and the limb was saved. There is little risk of making the incisions either too free or too numerous, for the original wound, with its severely crushed margins, to say nothing of the fracture itself, will be sure to take much longer to heal than the clean-cut incisions in the less damaged parts. The incisions should always be made with two main objects in view—viz., the prevention of tension and the provision of drainage. Parts that have been severely bruised should therefore always be freely incised; pockets that may have been made in the deep parts by tearing of the soft structures should be laid open, and incisions should be made through the skin and subcutaneous tissue when they have been stripped from the underlying structures. I am quite certain that I have many a time saved much tissue that must otherwise have perished by making these free incisions, and, as in these severe injuries there is almost necessarily a considerable loss of soft parts, so that frequently a large raw surface has to be closed, every particle of surface saved is of importance. The incisions themselves almost always heal comparatively rapidly—much more rapidly than the wounds accidentally inflicted. A very comforting fact is this: That the smaller the need for the incision, the more rapid will be its closure. This follows necessarily, since the less the tendency for tension to develope the less will the wound gape, and, consequently, the more rapidly will it heal. In carrying out this practice the surgeon will not infrequently be led to seriously question himself as to the need for adding to the already severely damaged limb a new series of injuries, but I can only say that, though at the time I have often wondered whether I have not used the knife too freely, I have never yet, in the subsequent course of the case, had any cause to think that my incisions had been needlessly severe. On the other hand, I have often regretted that the knife had not been more freely employed. I am satisfied also that it is much better, in the interests of the patient, to err on the side of its too free use than in the opposite direction. For the addition of several clean-cut wounds to a limb already terribly crushed and presenting a large open wound, is really not perceived, and these additional incisions will be healed long before the main and original wound; whilst, if the knife have not been used with sufficient freedom, tension will arise, there will be considerable pain, and there may be death of soft parts, really causing considerable risk to the limb, and certainly prolonging the time of healing. The foregoing remarks will apply to all severely crushed limbs, whether there be any fracture present or not. Of course, in almost all cases where the crushing is so severe as to require incisions, the bones will also be broken.

The next point I wish to consider is the method of dealing with the fragments of bone in cases where there is great comminution. It is the usual practice to remove all fragments that are completely or almost completely detached from the bone and from the soft parts. It is always apt to be rather a difficult point to decide whether certain comminuted fragments are to be removed or retained. If the periosteum still covers the fragment and holds it in contact with the bone, there then can be no question but that it should be retained. But if pieces are completely separated from the bone and from the surrounding soft parts, it has, with equal definiteness, been the practice to remove them. Then there is the middle class, which always give rise to difficulty in coming to a decision—those cases in which the fragments are very loose, but still have some slight attachments to the soft parts. It is now my practice to remove no fragments except those that are completely detached, and not even to remove these unless the surrounding parts are so severely injured that there is certain to be death of a considerable amount of the soft tissues, in which case the bone would be sure to die also, whereas if there appears to be some reason to hope that the soft parts are going to live it may also be expected that the bone will also recover.

The removal of large pieces of bone is apt to be followed by non-union. This was exemplified in two of my cases. A man, aged forty, came under my care in the Melbourne Hospital, suffering from a very severe compound comminuted fracture of the right humerus in its lower third. The ulna and median nerves were dissected out for three or four inches; the brachial artery was also completely exposed, and was occluded for several inches with blood-clot. The muscles were very severely torn; the fleshy belly of the biceps was torn completely in two. I made five incisions into the crushed tissues, and left the wounds quite open; *removed a large fragment of bone* that was completely detached; thoroughly irrigated the limb with perchloride of mercury solution (1 in 1,000); dressed with dry antiseptic dressings, and fixed the limb on a splint. Though the man was an exceedingly bad subject—he was forty years of age; he was a confirmed alcoholic; when the accident happened he was so drunk that he did not know anything about how it had occurred, though the injury was a very severe one: it was probably caused by the wheel of a heavy vehicle passing over the arm—yet he made an excellent recovery so far as the soft parts were concerned. The wounds all healed rapidly, but the bone did not unite for some months—not until glacial acetic acid had been injected between the broken ends of bone. He finally recovered with a useful arm.

In another case, a man, a little over thirty years of age, had a tramcar pass over his leg, causing a severe compound fracture, with much comminution of both bones. He was treated in the same way as the case just narrated, several detached pieces of bone being removed. The leg did excellently so far as the soft parts were concerned, but after several unsuccessful attempts by operation at obtaining union of the bones I was obliged to amputate.

Hence in more recent cases I have made every effort to retain the fragments, even though they are completely detached. I was the more strongly impelled to do this by the recent surgical practice of replacing portions of bone removed by the trephine and of bone-grafting. If under such conditions the bone will live and unite, it certainly is reasonable to expect that it would do so in most of the cases now under discussion. I might here refer to two of my more recent cases treated in this way.

Nearly a year ago a man, a little under forty years of age, also an alcoholic, was returning home somewhat late at night, and got off the road, wandering on to the wharves on the Yarra bank. He there pulled a barrel of cement, weighing 2 cwt., on to himself. He lay for several hours with this weight on his left thigh. In the small hours of the morning I was rung up to see him at the Melbourne Hospital. The lower end of the femur was severely smashed, there being considerable comminution, one large piece especially inviting removal, but I resisted the temptation to remove it, and, after making free incisions and thoroughly irrigating the injured parts, put the limb up carefully. The wound appeared to implicate the knee-joint; the house surgeon, who first saw the case, thought that it did do so, but I could not detect any communication. Subsequently his knee-joint filled with fluid, and the temperature rose. On incision, the fluid was found to be sero-purulent in character; it did not reaccumulate, and the wound into the joint quickly healed. The original wound did not run an aseptic course, which is scarcely to be wondered at, seeing how long the patient was exposed after the infliction of the injury, and how bad a subject he was. But the wound slowly healed. Splendid union of the fracture occurred; a small sinus, discharging a little, remained for some months, leading doubtless to the large fragment of detached bone, but only a small part of it can have died, and no sequestrum was ever seen to come away. I see the man in the street occasionally, walking with a limp, due to stiffness of the knee. This stiffness is not complete, and will probably lessen.

The other case is the one to which more than any other this paper owes its origin. Some months ago I was called to the hospital to see a patient, a man aged twenty-nine, a healthy subject who, whilst working in a sewer-shaft, was struck by a heavy pump which fell from a height of 16 feet. He had sustained a simple comminuted fracture of both bones of the right leg in the upper third, and in the left a very severe compound fracture in the lower third with great comminution of the bones, and extensive laceration and severe bruising of the soft parts; the muscles on the outer part of the leg were torn as well as bruised, and the inter-muscular planes were freely opened up. The circulation in the foot was good. Professor Watson, of Adelaide, was present, and made various comments on my proposal to save the limb. He thought that I was pushing conservative surgery to its limits (and I think I was too); whilst admiring my pluck in trying to save the limb, he thought I was very foolish. As the skin, the muscles, and the cellular tissues were freely torn and greatly bruised, I used the knife very freely. The fragments of bone were very difficult to manage; one small piece only was removed; other larger pieces, almost, if not quite, detached, and lying almost at right angles to their proper axes, were with difficulty worked into approximately their normal positions; the wounds were thoroughly irrigated with warm perchloride of mercury solution (1 in 1,000); the limb was then placed on a back splint, and carefully dressed with dry dressing; then two Cline's splints were applied to the sides. For a few days I had considerable anxiety with the case. The temperature rose several degrees; the discharge was very profuse and thin; the skin and other soft parts on the outer side of the limb, where the injury was most severe, looked inclined to slough. A little sloughing did take place, but the free incisions rendered any tension impossible, and the sloughing quickly stopped. After a few days the temperature subsided practically to normal, and I had no further anxiety. The healing necessarily took a long time, because on each side of the leg there was a large raw surface to close by granulation. I did not care to put him under chloroform to Thiersch-graft the limb, as no time would really have been saved, seeing that the bone was certain to take longer to heal than the soft parts. It was very interesting to watch a large piece of bone, which looked at first as if it must inevitably die, become slowly covered by granulations and eventually no perceptible portion of it separated as a sequestrum. The bone united slowly, and on account of both limbs being broken it was some time before we could get him up on crutches. He was discharged with sound limbs, slightly deformed, on 24th June, 1899.

In such cases as this I do not think a surgeon would be justified in attempting to save the limb unless he were prepared to slave at the case himself, dressing the limb himself for a few days, so that in the event of failure he could at once amputate and so save the patient's life, but I feel certain that with due care such a course would not often be necessary.

The first case that I treated in this way was very interesting. It was that of a stonemason who had his forearm crushed between two large stones, the upper one, that fell on his arm, weighing several hundredweight. The palmar and dorsal surfaces of the hand were crushed open; the metacarpal bones as well as the radius and ulna were smashed; the muscles, especially on the extensor surface, were pulpy to the feel. Under chloroform, I made seven or eight long incisions, and the man recovered with a limb nearly perfect, and almost as rapidly as if he had only had a simple fracture of the forearm.

My only failure is also worth recording. It was a severe railway accident, and was one of the earliest cases thus treated. There was a compound comminuted fracture of the leg, with extensive injury to the soft parts. I attempted to save the limb by means of free incisions and the use of antiseptics, but I was greatly afraid of failure. The chief interest of the case is in this: Had I amputated at the time, I must, in order to have got reasonably sound flaps, have amputated above the knee. Subsequently, when, through extensive death of the soft parts, the late Dr. Beaney amputated, he was able to do so, and to secure a good stump, below the knee, so that even here the patient was the gainer by the method of treatment, as it saved a considerable amount of skin which could not have been relied on to form flaps in the first instance.

Though the main object of my paper was to emphasise the importance, in the treatment of severely crushed limbs, of free incisions, and of refraining from the application of sutures, and of retaining, if at all possible, all comminuted fragments of bone, yet I would like to say a word or two on the subject of amputation in those cases where this is obviously the surgeon's only course. No doubt in the majority of these cases the proper time to operate is when the surgeon first sees the patient, but it has seemed to me that, in some cases when the shock is very severe and the patient is almost certain to die, he would be given a better chance if stimulants, warmth, &c., were first applied to combat the shock, and amputation performed later on if the patient recovered sufficiently. It has seemed to me that if you represent a patient's power of living by 100 the accident has perhaps destroyed 75 of the 100; then if the surgeon comes in and, with the assistance of the anæsthetist, knocks out another 40 or 50, the patient dies outright; whilst by waiting it might be possible to regain about 40 or more of the lost 75, and then the patient would have some chance of successfully going through the operation. Of course, in case of waiting precautions must be taken against hæmorrhage, and this is certainly the great objection to the frequent following of this practice.

Finally, as to another point on which I can speak with greater positiveness. Sometimes it would be a great gain to the patient that the amputation should be performed below or at a certain joint, but the skin and other soft parts are so damaged that sound flaps cannot be obtained to cover the stump. In such cases, I am sure that it is good surgery to saw through the bones or disarticulate at the desired place, and save much more skin than is necessary to cover up the stump, quite regardless of its shape and of its condition (provided it be not absolutely hopelessly destroyed)—in fact, to do a regular amputation, except that a large amount of skin, irregularly shaped, is left; then put no sutures whatever in the skin, but leave it lying loosely on the deeper tissues, and carefully dress the stump. Though some of the skin may die, it will almost certainly be found that more of it will live than suffices to cover the stump, and the operation can be carefully completed when the surgeon desires to do so. Another distinct advantage of this method of procedure is that it requires much less time, and consequently there is much less shock; and this alone is not unlikely in a certain number of cases to save the life of the patient, and to give him a much more useful limb than if his life had happened to have been saved after an amputation higher up. In the first case in which I carried out this method of treatment, a man in a railway accident had met with terrible injuries to the arm, reaching well up to the shoulder and almost into the axilla. His condition was very serious, so I simply disarticulated at the shoulder as rapidly as possible, stopped all bleeding, and left, without applying any sutures, a large amount of skin—some sound, but some severely bruised. The whole of the skin lived; and, as it was much more than was required to cover the stump, my senior colleague, Sir T. R. Fitzgerald, was able to use a considerable portion of it as a graft to a child's face.

Dr. Hunt (Warwick) mentioned the case of a man who fired off a heavily charged gun, which exploded, the trigger tearing away the flesh of the upper part of his arm, separating the muscles, and causing comminuted fracture. The case seemed hopeless; but the man begged that his arm should be saved. Four months afterwards his arm was practically as good as before the accident.

Dr. Bright (Hobart) said he could give a case as to the advisability of trying to save limbs seemingly hopelessly crushed. This was the case of a man who was run over by a vehicle weighing about 5 tons. It seemed as if both limbs would have to be amputated. Five or six fragments of bone were removed from each leg, and after a prolonged treatment the man was got up. The last he saw of him was when he was driving on his dray, apparently as well as ever. But it was a case in which the surgeon should strictly take it under his direct control.

Dr. Pain (Sydney) asked Dr. Moore how deep and long would be the incision in connection with a compound comminuted fracture?

Dr. Moore, in reply, said he would be guided by the extent of the bruising. Any injury that was inflicted without bruising would recover without incision. He could imagine it as really necessary to cut down on a comminuted fracture. If it were compound, he certainly would make an incision so as to see that the bone was put in its right place, and to prevent attention being necessary afterwards. If the wound was bruised, he was certain any incision would not prolong the healing. It was very hard to tell anyone exactly. And this was the point he wished particularly to refer to. If the limb was severely crushed, and there were wounds, then they could make their incisions freely, with a clear conscience, because the incisions were likely to heal long before the original wounds. Of course, really the points on which to go were the amount of bruising and tearing of parts.

THE SURGICAL TREATMENT OF PERITYPHLITIS.

Based on the Records of 347 Cases, Treated at the Prince Alfred and Sydney Hospitals during the last Six Years.)

By Charles P. B. Clubbe, M.R.C.S. Eng., L.R.C.P. Lond.

Hon. Surgeon to the Prince Alfred Hospital; Hon. Surgeon to the Sydney Hospital for Sick Children; Lecturer in Clinical Surgery at the Sydney University.

At the beginning of a discussion of this character, if it is to be of any value at all, we must try at the outset to get an intelligent grasp of the disease, and in order to do this it seems to me to be very important that we should have a clear and distinct picture in our minds of its pathology. In looking up the various authorities on the subject for this paper (and they are not a few), I found by far the clearest description of its pathology in Hawkins' book. Hawkins* says: "The old division of appendicitis into a 'perforative' and a 'non-perforative' form has lost its value for clinical use, now that we know that both an acute general peritonitis and a perityphlitic abscess may result from non-perforative disease of the appendix. But examination of a large number of specimens makes it clear that Osler's division of appendicitis into a 'catarrhal' and an 'ulcerative' form is a good one. Though these two do to a certain extent overlap, inasmuch as catarrh may end in ulceration, and both conditions may exist side by side in the same specimen, yet is this distinction nevertheless useful, since they arise from different causes. The cause of the 'catarrhal form' we do not exactly know, but it is clear that it is not dependent on the presence of fæcal or other extraneous matter; while the 'ulcerative form,' on the other hand, is produced mainly by the pressure of a fæcal concretion or a foreign body. Equally from both these forms may arise every grade of peritonitis.

"In a large number of cases it happens that the peritonitis arises through a local or a general 'necrosis' of the appendix, which may be the primary condition, or may arise as a result of a previous catarrh or ulceration. There is reason to believe that in these cases the necrosis is dependent upon a bacterial invasion of the tissues of the appendix-wall, and consequently a third form—viz., 'infective appendicitis'—may well be considered under a separate head.

"Catarrhal appendicitis," he explains, "may be, and probably is, of very frequent occurrence, for it produces no symptoms, and may subside without leaving a trace. It may be independent of the presence of fæcal matter."

The result of catarrh may be—

"1. Perfect recovery."

"2. Complete obliteration of the lumen, and therefore immunity from any further trouble."

"3. Partial obliteration causing a stricture, possibly impermeable."

"4. The appendix beyond the stricture may become cystic."

ULCERATIVE APPENDICITIS.

"A primary local ulceration of the appendix not preceded by a general catarrh, but due to a fæcal concretion or a foreign body." He goes on to say:—"As the fæces begin to exercise pressure on the wall, the first effect is a disturbance of the lining epithelium. As the process continues, the epithelium is completely destroyed, and the mucous membrane, the submucous tissue, and finally the muscular coats disappear before the advancing pressure.

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* Hawkins "On the Diseases of the Vermiform Appendix."

"In some cases it is hardly correct to describe this process as one of ulceration. It appears to be rather an atrophy produced by pressure, and evidence of inflammation may be scanty or absent."

INFECTIVE APPENDICITIS.

"It is a form which is almost of necessity followed by a virulent degree either of a general peritonitis or of the local suppurative form. . . . Most of the cases, but not all, are accompanied by a necrosis or sloughing of some part of the appendix-wall." He goes on to show that this "infective appendicitis" need not necessarily have been preceded by the catarrhal or ulcerative form, nor need there have been any fæcal concretion. He makes it clear that this infective appendicitis can excite a rapidly fatal peritonitis without any necrosis or perforation visible to the naked eye or microscope.

Hawkins maintains that this form of appendicitis may result from either the catarrhal or the ulcerative form, but that it may arise independently of either, and it appears in all cases to be associated with and due to a bacterial invasion of the tissues of the appendix-wall. To explain the frequency of bacterial invasion in the appendix when we know that other parts of the intestinal tract containing the same micro-organisms are frequently ulcerated and yet escape, Hawkins suggests that it is due to the appendix "having little power of resistance," "being an organ which is degenerate and functionless from first to last."

But we know that the mucous membrane of the appendix may be unhealthy and ulcerated without the walls becoming invaded with bacteria. This is probably due to "the varying virulence of bacteria under different conditions." His general conclusions are :—

1. "Catarrhal appendicitis, marked particularly by shedding of the protective epithelium, apt to pass into a chronic condition with thickening of the wall.
2. "The cystic condition of the appendix, whether due to a post-catarrhal stricture or to kinking.
3. "Ulcerative appendicitis, due to the presence of a fæcal concretion or a foreign body.
4. "Acute infective inflammation of the wall of the appendix commonly associated with necrosis of its tissues. This may occur when the mucous membrane shows slight signs of chronic disease, but in the majority of cases it supervenes upon one of the foregoing conditions.

"It is made clear, both by clinical and by *post-mortem* observation, that the first three of these morbid conditions are not attended by symptoms until the peritoneal coat of the appendix is involved. It is possible that vague sensations of uneasiness may arise before this point is reached ; but the general truth is, I believe, beyond controversy that, in all cases which claim medical attention, the peritoneum is already inflamed."

"The fourth condition necessarily entails the immediate onset of severe symptoms."

The points I want to emphasise are these—

Very grave morbid changes may, and very frequently do, take place in the appendix without giving rise to any symptoms. A man may have catarrhal appendicitis ; this may be followed after a time by a narrowing of the lumen, so much so that this may become stenosed at some spot, and, as a consequence of this, the appendix may become cystic beyond the stricture. There may be ulceration of the mucous membrane, with or without an enterolith. All these serious changes may take place without giving rise to any symptoms sufficiently severe to make the individual aware that there is anything the matter with him. I am sure that many of us fail to realise this. It is a point that is not made much of by many writers on this subject, but I think the importance of it can hardly be over-estimated, especially when we are considering the treatment of what are called "first attacks," "mild attacks," "appendicular colic," &c. I think it is very doubtful if there is such a thing as "appendicular colic." It is very improbable that there is ever sufficient contraction of the muscular layers of the appendix strong enough to give rise to any sensation that could be appreciated by the individual.

I remember once removing the appendix from a man who was supposed to suffer from "appendicular colic." The attacks of pain that he suffered from were fairly frequent, but they never actually laid him up. His appendix was thickened, there were a good many adhesions, and the lumen was much narrowed at one place.

For clinical purposes there are a variety of classifications, no two writers being quite in accord.

Talamond (*) gives five varieties—

1. Rapidly acute perforative appendicitis.
2. Sub-acute appendicitis with insidious progress.
3. Acute appendicitis with partial suppurative peritonitis.
4. Chronic relapsing appendicitis.
5. Parietal appendicitis with appendicular colic.

Kelynack (†) only three—

1. Simple appendicitis.
2. Perforative appendicitis.
3. Recurrent appendicitis.

Barling (‡) uses four—

1. Mild appendicitis in which neither abscess formation nor perforation occurs.
2. Appendicitis with abscess formation.
3. Acute perforating appendicitis.
4. Relapsing appendicitis.

Treves (§) adopts five—

1. An ordinary attack, the case ending in resolution.
2. The case ending in suppuration.
3. An attack of the mildest type.
4. A most intense and acute attack.
5. Certain peculiar forms of perityphlitis.

Hawkins' classification is clear, simple, and practical. He does not go beyond the three chief forms—

(1) An adhesive peritonitis limited to the right iliac fossa accompanied by the exudation of a coagulable material (perityphlitis).
(2) A similarly localised inflammation proceeding to the formation of pus (perityphlitic abscess).
(3) A general peritonitis.

(*) Talamond.—Appendicitis and perityphlitis, 1893.
(†) Kelynack.—The pathology of the vermiform appendix, 1893.
(‡) Barling.—The Ingleby lectures on appendicitis, 1895.
(§) Treves.—Article on perityphlitis in Allbutt's System of Medicine.

This is the classification I have adopted in dealing with the cases that have occurred in the Prince Alfred and Sydney Hospitals during the last six years. I find that there have been 347 cases, with 57 deaths, giving us a death rate of 16·1 per cent.

There were 205 cases of perityphlitis with no death, 35 cases of perityphlitic abscess with 8 deaths.

The appendix was removed in the interval between the attacks 59 times with 6 deaths.

The great mortality came in the cases of general septic peritonitis; there were 45 cases with 43 deaths. In 29 of these cases the abdomen was opened, the appendix removed, and the peritoneal cavity flushed out and drained in various ways, but only two recovered. There were 16 cases of acute septic peritonitis, originating from the appendix, that died without any operation being performed.

The diagnosis in most of these cases was made in the *post-mortem* room.

TABLE SHOWING THE NUMBER OF CASES TREATED AT THE PRINCE ALFRED AND SYDNEY HOSPITALS FOR THE LAST SIX YEARS:—

—	PRINCE ALFRED HOSPITAL.			SYDNEY HOSPITAL.			TOTAL.		
	Cases.	Cured.	Died.	Cases.	Cured.	Died.	Cases.	Cured.	Died.
Perityphlitis	111	111	...	94	94	...	205	205	...
Perityphlitis (abscess opened) ...	25	20	5	10	7	3	35	27	8
Operation for removal of the appendix in quiescent stage	46	40	6	13	13	...	59	53	6
Appendix removed during attack...	...	...	...	3	3	...	3	3	...
Septic peritonitis (operation) ...	23	1	22	6	1	5	29	2	27
Septic peritonitis (no operation) ...	10	...	10	6	...	6	16	...	16

—	Prince Alfred Hospital.	Sydney Hospital.	Total.
Total number of cases	215	132	347
Total number of recoveries	172	118	290
Total number of deaths	43	14	57
Death rate	20	11·8	16·1

I am much indebted to Dr. Cosh, late resident medical officer at the Prince Alfred Hospital; to Dr. Ludowici, my late house surgeon; and to Dr. Binney, late medical superintendent at the Sydney Hospital, for the great amount of time and trouble they spent in hunting up the records of these cases. It is only with their help I have been able to attempt an analysis of the cases.

The information we have obtained by looking through the records of these old cases ought to have been much more valuable than it is. Unfortunately, in a large number of cases, the notes are most defective, and so certain important facts, that one would have liked to have been quite sure about, cannot be made use of. One fact I was particularly anxious to obtain, because it has a most important bearing on the question of removing the appendix after a single attack of simple appendicitis. If it could be proved that a very large percentage of the cases of general peritonitis had previously suffered from simple appendicitis, this would be a strong argument in favour of the removal of all appendices after a single attack. From the cases under review nothing can be made of this point, because in so many cases the fact was not noted.

In 16 out of the 57 fatal cases it was noted that there had been one or more attacks of supposed appendicitis, but in the rest no note was made on this point. They may have had, or they may not.

Our death rate seems to be about the average. In Porter's 448 cases the death rate was 17·23, and in Hawkins' 264 cases it was 14 per cent.

Can we hope to lessen this death rate of 16 per cent.?

In the 205 cases of simple perityphlitis there were no deaths. Some may say, "What better result can you wish for than this?" But we must remember that these people are not really cured. A certain number of them had been in the hospital more than once for the same trouble. I find it noted that 23 had been in twice, 9 three times, and 1 had been in four times. A large number of them will surely appear again; and who can tell what form the peritonitis will assume at the next attack? It was formerly taught by some that each attack lessened the chance of a perforation and general peritonitis. If we remember the pathology of the subject, how can we hold this? In some rare instances, the whole lumen of the appendix is obliterated and the case cured; but this is not common, and each attack makes it more impossible.

For suppurative perityphlitis in which the abscess was opened, our death rate was 22·2 per cent. Some of these cases lived a long time, and subsequently died of pyæmia. The death rate of 10·2 per cent. for cases in which the appendix was removed in the quiescent stage is rather higher than it ought to be; but the great mortality comes in the cases of septic peritonitis—45 cases with only two recoveries. Certainly 16 of these were not operated upon: some few because they were too collapsed to stand any operation, the others because they were not diagnosed. The great majority of the 28 that were operated upon did not live more than thirty-six hours; 14—that is, exactly half of them—were under fifteen years of age.

DIAGNOSIS.

This is generally easy in the simple cases. In the cases that suppurate, when the patient is a woman, we have to exclude pelvic troubles, especially pyo-salpynx. It may be almost impossible to differentiate these conditions if the appendix happens, as is not infrequently the case, to hang over the brim of the pelvis, but even if the mistake is made the abdominal incision is the correct treatment. The diagnosis is often difficult in these very acute cases, call them whatever name you will—"fulminating," "perforating," or "septic peritonitis" (unless the general peritonitis has come on during a localised suppurative attack)—the onset is usually sudden. The abdominal pains are severe and general, there is tenderness all over the abdomen, the temperature and especially the pulse rate is raised, the breathing thoracic, and the patient looks profoundly ill from the onset. In the absence of a history of previous attacks or of any pain starting in the right iliac fossa, and without any special tenderness or fulness in that region, how can we be certain we are dealing with a peritonitis originating from a diseased appendix? Frequently it must be impossible, but we do or ought to know that we are dealing with a general septic peritonitis, and also that there is but one method of treatment likely to be successful.

TREATMENT.

Ever since appendicitis has been discussed amongst medical men, the question of treatment has always created the greatest interest. At consultations on each individual case the important question to decide is: "Shall we operate?" In America, as you are aware, the tendency has largely been to operate at all stages and in all cases; while in England it has been to wait and to operate only in those cases where an operation has been obviously indicated. What we now want is the collective opinion of Australian surgeons, and how to direct the rising generation in this matter. I think we shall do well to steer a middle course, avoiding on the one hand the excessive surgical zeal of the Americans, on the other the too conservative surgery of the English. Although there are many and strong arguments in favour of operation immediately appendicitis is diagnosed, there are many as strong against it. The question resolves itself into which procedure will save most lives. If every case of appendicitis could be placed in a properly equipped hospital, and have the services of a competent surgeon within twenty-four hours of the onset of the attack, then,. doubtless, the mortality would be diminished by early operation. This we know is impossible, as many of these cases cannot for various reasons be taken to a public or a private hospital, neither can they secure at a moment's notice the services of a surgeon accustomed to this class of work.

When to operate:—

* McBurney, in 1892, said: "Operations for acute appendicitis performed at the right time, and before the disease has progressed too far, are almost invariably successful." In 1896, in the *Medical News*, he says: "It is in the first twenty-four hours from the beginning of the attack that we can decide not only as to the diagnosis, but as to the probable course and result of the case. If in five or six hours there is urgence, the patient is not in immediate danger; kept at perfect rest in bed, if in twelve hours there is no increase in the severity of the symptoms, the patient should begin to improve. If the urgency of the case has steadily increased in twelve hours from the time when the diagnosis was made, an operation will probably be called for. After two attacks a patient is sure to have a third, and each attack renders operation more difficult and dangerous. All the advantages lie with operation between the attacks. In an operation during an acute attack the prognosis is worse."

† Murphy, in 1895, said: "Every case promising or unpromising should be treated by surgical operation at the earliest possible moment."

Another American opinion is: "We should operate on every operable case as soon as the diagnosis has been made. Mistakes are made by waiting and watching for symptoms to manifest themselves" (A. M. Cartledge, 1896, *Medical News*).

Yet another opinion: "The safest general rule is to operate as soon as the diagnosis of appendicitis is made" (C. P. Noble, *Medical News*, 1896).

If the American surgeons are rash, some of the English seem to err on the side of caution. In opening the discussion on appendicitis, at Carlisle, in 1896, Dr. MacDougal, in dealing with the subject of when to operate, said: "For me a quick thready pulse, repeated vomiting, marked depression (it may be somnolency), severe continued pain or pain which, while itself relieved, is associated with a recognisable advance in local signs, acute and extending tenderness, with increase in abdominal distension and thoracic respiration, are indications for operation. In children with less emphatic symptoms than these, early interference may wisely be undertaken." Surely, gentlemen, we court disaster if we are always to wait for such very "emphatic" symptoms.

In the discussion in the following year, at Montreal, Dr. Armstrong, in his paper, said: "The question of time to operate naturally arises here, and might be discussed for days. I believe it to be next to impossible to put into words a clear statement of the time to operate. There are two groups of cases upon which I think all will agree. In one the attack is so mild that the question of operation during the attack scarcely arises; in the other the condition of the patient, when first seen, is so grave that but few would dare to delay. But there is a much larger group where there seems to be room for difference of opinion; it is here that the judgment and experience of the surgeon needs to be exercised to their fullest extent. . . . . . My working rule is to operate on well-marked cases at once, This includes all cases in which there is pain, tenderness, muscular rigidity, with vomiting, elevations of temperature, acceleration of pulse, and anxious facial expression. Mayo Robson, in his address at the Medical Society in London, in 1896, said: "The most important points to settle in any given case of appendicitis are—1. Ought operation to be done? 2. If so, when?" He believes that an early operation as first advocated by McBurney would lead to a far greater percentage of recoveries than the method of individualising now adopted in England :—

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* *New York Medical Record*, McBurney, April 16, 1892; and *Medical News*, No. 24, 1896.

† Murphy, *Medical News*, January 5, 1895.

"Relief or cessation of pain with a marked rise in the pulse rate is an indication for immediate operations; it not infrequently indicates gangrene of the appendix."

"Distension of the abdomen with vomiting and rapid pulse are signs that admit of no delay, as they are indications of extending or of general peritonitis."

Again, in an address on "The dangers of delay" at Nottingham this year, Mr. Robson says: "The removal of an appendix before suppuration, perforation, or gangrene has occurred, or before firm adhesions have formed, is practically unattended with risk . . . In waiting for resolution we not infrequently land ourselves in serious difficulties, and have to operate under unfavourable circumstances" . . . Speaking about suppurative appendicitis he says: "Whatever form it assumes, whether with or without perforation, or with or without gangrene, it demands operation, which in my opinion should be advised at the earliest possible moment—that is as soon as possible diagnosis is arrived at." He has never regretted operating, but he has seen several cases which terminated fatally because the operation was not strongly urged.

Treves,* in the articles on "Perityphlitis" in "Allbutt's System of Medicine," under the heading of operative interference during the acute attack, says: "The main feature in this treatment consists in the urgent advice that a free incision should be made down to the inflamed area as soon as there is evidence that suppuration has taken place. . . . . It may be laid down as a rough rule that the use of the knife will very seldom be called for before the fifth day." Of course, Mr. Treves is here referring to cases of localised peritonitis with abscess, and not to perforative peritonitis. For, further on, in arguing against early operation in all cases, he says: "It is urged that certain cases have ended fatally within the first thirty-six hours by perforation into the general peritoneal cavity. Such cases are exceedingly rare, they cannot be anticipated, and they are not difficult to recognise. In most of them the first symptoms are those of perforative peritonitis. When such a case is met with the abdomen should of course be opened at once, and the perforation dealt with, as is the practice in dealing with other forms of perforative peritonitis." I quote this, because in this article Mr. Treves does not, beyond these few words, deal with the treatment of these acute perforating cases, and I happen to know that some men are now under the impression that he has modified his views so much that he does not operate in any case before the fifth day. As a matter of fact, he says nothing of the sort. When he speaks about the fifth day he is referring only to cases of localised perityphlitic abscess.

In cases of simple appendicitis I think we shall do well to follow the old rule and not interfere during the attack, but I think it is our bounden duty, seeing the impossibility of knowing the condition the appendix is in, to advise the patient to get the appendix removed during the quiescent stage. This fact has been more than ever impressed upon me since going through the records of the fatal cases that I have tabulated in this paper. A fatal case of general peritonitis has often been preceded by a mild attack some months before. In relapsing appendicitis the sufferer does not require any urging. He is thankful to get rid of a source of annoyance that so frequently interrupts his work and prevents him from enjoying life.

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* Treves: "Allbutt's System of Medicine," vol. 3.

In suppurative cases the necessity for an operation soon becomes obvious. In operating in these cases it is not wise to seek for the appendix unless it obtrudes itself upon you. Many deaths have been caused by a general peritonitis set up by too eager a surgeon when opening a perityphlitic abscess, but it is quite a mistake to suppose, as many writers would lead you to believe, that in these abscess cases the appendix gives you no further trouble. My experience is quite the other way. When the abscess has done discharging and the wound has healed, we should advise these people to have the appendix removed.

The following case is a good example :—

M.H. was admitted to the Prince Alfred Hospital, 7th February, 1896. He was then suffering from an ordinary attack of perityphlitis, and had a large tender mass in the right iliac fossa which disappeared with rest. He was sent home well after being in the hospital five weeks. He was re-admitted 10th June in the same year, and a large perityphlitic abscess opened, but the appendix was not removed. After a few weeks the wound healed, and he was discharged. Again re-admitted, 18th September, 1897. It was found that another abscess had formed in the old situation. From this some very foul-smelling pus was evacuated and the cavity plugged with gauze. The first time the gauze was changed fæces passed through the wound ; subsequently a tube was inserted, which was lost and ultimately passed through the rectum. He was discharged cured 20th January, 1898. Re-admitted for a third time, 19th May, 1899. He gave an account of four attacks during the intervening sixteen months, the last four days before admission. He described the acute symptoms of these as lasting only twelve hours, but that he felt ill "for weeks afterwards." A hard mass could be felt in the right iliac fossa. On 25th May the abdomen was opened over the cæcum, and, after some difficulty with thickened omentum and numerous adhesions, the appendix was removed. It was found to be much thickened but complete, and presented a discoloured spot near its base, evidently marking the seat of perforation two years previously. Here the wall was thinned, but soundly healed. He was discharged cured 23rd June, 1899.

The mortality in the acute perforating or infective cases must always be high. No doubt many would die even if operated upon within the first few hours, for it seems that in some of these cases vast hordes of malignant bacteria invade the peritoneum and reach its furthest limits in a wonderfully short space of time. A poison seems to be produced which, when absorbed, very quickly proves fatal. This poison is made and absorbed so quickly that flushing out the peritoneal cavity does not stay its lethal effect, because it is already in the blood. I think the time will arrive when we shall inject a "peritonitis antitoxine" in these cases, and by this means and early operation save some lives. But all these cases are not so fearfully malignant, and even now, with the means we have at hand, we ought to get better results. To do this we must get these cases earlier. We can only do so by impressing upon medical men, who see these patients when they are first taken ill, the great importance of recognising the nature of the disease at the very onset, in order that surgical aid may be obtained with the least possible delay. In looking over the hospital records, I am sorry to say that I found that in some of the fatal cases there was considerable delay between the time of admission and the operation, caused apparently by the patients being admitted under the care of the physician.

I maintain that all cases of general peritonitis and all acute cases where appendicitis may reasonably be suspected should be admitted under the surgeon instead of the physician. In operating on these acute cases, the surgeon may not always be quite sure which is the best place to open the abdomen. In the absence of any definite signs pointing to peritonitis being due to the appendix, he will probably decide to open in the middle line.

In the event of the appendix proving to be the cause of the trouble, and should there be any difficulty in reaching it, another opening over the cæcum does no harm and is advantageous for the purpose of drainage; it also facilitates a thorough flushing of the peritoneum. A point that might well be discussed is this: After removing the appendix in these septic cases, how should the abdominal cavity be flushed, and with what? Should counter openings be made, and where?

Mayo Robson advocates making counter openings in the loins. With this method he had five successful cases out of six in one year, but he says he does not expect to have such good luck again.

Mr. Cuthbert Wallace, at the Medical Society in London early this year, read the notes of three cases of appendicitis with diffuse peritonitis: "Each case had commenced with local abscess that had spread and infected the peritoneal cavity. In two instances the peritonitis was general, and in the third it was limited to the structures behind the great omentum, reaching no higher than the transverse meso-colon. In all three the appendix was removed, and the intestines were turned out on the surface of the abdomen and cleaned with sterile water, while the belly cavity was flushed with the same fluid. He believed that this was the most rapid and effectual method of dealing with such cases. It was next to impossible to clean the peritoneum while the intestines were *in situ*, because the stream did not flow in a sufficiently even manner, but simply washed infective material from one part of the abdomen to another. . . . All three cases recovered without a bad symptom."

For some time I have thought that this would be a good way of dealing with these cases, but I have never done it completely—perhaps out of consideration for the anæsthetist, and the natural dislike to a death on the table and all its attendant annoyance. Mr. Wallace's cases are certainly very encouraging, and I think the plan worth a trial. So far only two of my cases of general purulent peritonitis from a diseased appendix have recovered. In both these cases the abdomen was opened over the cæcum only, the peritoneal cavity was washed out and drained after the method of Morris.

To sum up then: I am of opinion that we should remove the appendix in all cases after an attack of appendicitis, both in the simple cases and in those in which a localised abscess has been opened, but in advocating this treatment I do not for one moment wish to imply that the operation should be lightly undertaken by all and sundry. You cannot whip out an appendix like you can a tonsil. What does McBurney himself say?—"It is a serious operation, often a very difficult one, one requiring general surgical skill, good assistance, and every antiseptic precaution, and I do not recommend the operation to one who has never seen it done." We should bear in mind the aphorism of the Father of Medicine: "The physician must not only be prepared to do what is right himself, but make the patient, the attendants, and externals co-operate." And unless we can do this by placing the patient in a suitable environment, and obtain for him skilled surgical aid, it will be far safer for him to keep his appendix.

GENERAL DISCUSSION ON APPENDICITIS.

Dr. Barnett (Dunedin) expressed his pleasure at listening to the excellent paper which had been read. It seemed to him, he said, to sum up in such a comprehensive manner the knowledge they had upon the subject of appendicitis that there remained but little more to be said. He would indicate, however, one or two minor points that he thought were open to criticism. He had an idea, although there was no decided proof of it, that some cases originated as tubercular troubles. More than one case he had seen had associated with it evidences of tubercular mischief. Another point was when to operate. It seemed to him that the question would be simplified by eliminating those cases in which almost every surgeon would be inclined to operate. In cases of recurrent appendicitis he thought the general consensus of opinion was that an operation should be performed. His own idea was that if such a patient has had one attack of appendicitis, and, in spite of having followed the instructions given him as regards diet and the prevention of constipation, gets a second attack, an operation should be performed. By that he did not mean to say that an operation should be performed immediately on the recurrence of an attack. He preferred to wait for a quiescent interval. He thought there was a good deal to be said in favour of operating at the very beginning of a second attack, provided the patient had paid proper attention to the orders given him. But the cases which offered the difficult problem to practitioners were those acute cases where they had the utmost difficulty in deciding whether to operate or to keep their patient under medical treatment. Like Dr. Clubbe, he preferred to take a middle course. He thought the patient should be watched carefully, and if by the fourth day the symptoms were increasing instead of diminishing—if they were found to be on the up-grade—the case should be operated upon. If, however, after four days the symptoms were found to be subsiding, he held that it was best to wait, and let the patient have a chance to recover without being operated upon. What he had said represented his thoughts on the matter, and if they had to deal with acute cases they would, he was sure, admit that they were likely to meet a very difficult state of affairs, because if pus had formed they ran considerable risk in operating. A practitioner, therefore, had to operate with the utmost care and gentleness, and he certainly thought that cases of acute appendicitis should be in the hands of one who had frequent opportunities of performing such operations. There was an idea abroad that the removal of the appendix was as easy as shelling peas. He had seen cases where the operation was easy, but on the other hand a practitioner might meet with difficulties which would tax the resources of the most competent surgeon.

Dr. H. Russell (Melbourne) contended that they wanted something a little more definite than generalisation. He thought the best view to take was to consider what was the ideal method. The American surgeon advocated in all cases and at all times the removal of the appendix. Personally, he thought that was the ideal method. He could not conceive of any reason which could justify any surgeon watching a patient with an inflamed appendix with a view to seeing what the result of the attack might be. He considered that under favourable circumstances the immediate removal of an inflamed appendix was the ideal method of treatment. In saying that, however, he was far from saying that all cases of appendicitis under any circumstances should be treated by the removal of the appendix. If ever he was was attacked with acute appendicitis, and he happened to be surrounded by skilful surgical friends, his appendix would be in a bottle within twenty-four hours. If he was in the country, however, where he could not get the advantages of such surgical aid, he would prefer the ministrations of his medical friends. That, it seemed to him, was the proper attitude for surgeons to take towards appendicitis. There were other suggestions, such as that acute appendicitis should be treated in a palliative way. His own belief was that the risk of operating in an acute attack was altogether over-estimated. He did not consider that the risk which a patient ran through being operated on by skilful hands and under favourable circumstances—in a hospital, for instance—was nearly so great as the risk he would run by being watched through it. Then, again, it had to be considered that the suggestion he had alluded to involved two serious things instead of one. The question was therefore a serious matter, and one which a practitioner had to take into consideration.

Dr. Doolan (Yass) said that, as one who had suffered from an attack of appendicitis, the discussion was of very peculiar interest to him. He had had an attack some twelve years ago, when the word was hardly ever used. It was a rather severe attack, and his medical adviser clapped on twenty leeches and put on hot fomentations, and the trouble subsided. Since that time he had had no trouble, with the exception of some little pain he had some three years ago, which was only severe enough to lay him up for three days. It had never occasioned any trouble since, and, that being so, he was certainly not in favour of operations in all cases, and particularly on the first attack. He thought the American teaching of operating on all and every case was abhorrent. He said that in all due deference to Dr. Russell, who thought it was the ideal method. Now, with reference to operating in the quiescent stage, about which Dr. Clubbe had given some figures, which showed that of forty-six cases operated upon in the quiescent stage in the Prince Alfred Hospital there had been six deaths. The figures showed, he thought, that the operation should not be undertaken lightly. If such a percentage of deaths could occur in such a hospital where there was such an excellent staff, and where the surroundings were favourable for successful operating, he considered that the operation should not be undertaken lightly. The figures which had been quoted should, he thought, act as a wholesome brake on those optimistic surgeons, who seemed to think so lightly of the matter. In the place where he was practising a practitioner had to do the best he could. His method was that, if after four days the pulse and temperature indicated that there was a probability of pus being there, he opened it up. He did not attempt to remove the appendix, and did not attempt to remove it when the attack had subsided. He agreed with Dr. Hopkins, however, in that he would recommend operation in a second attack. That, he thought, was the happy medium between the rash teaching of the American surgeon and the conservatism of the English surgeon. Since he had been in practice he had had seven or eight cases of appendicitis, and in none of those cases had there been a recurrence. Should he meet with a recurrence, however, especially if the attack were a severe one, he would recommend the removal of the appendix.

Dr. Scott (Ballarat) said he thought it was for the general practitioner to consider the prevention of appendicitis in its ætiology and pathology. In his experience the operation might be one of the simplest or one of the most difficult that an operator can undertake. In either case it is almost impossible to foresee the result and say that we should operate early in appendicitis. At the same time he was in favour, with Dr. Clubbe, that the patient should be distinctly informed that a recurrence of the attack possibly will occur, and then the question of operating would arise. He must have watched a good many of these cases, and he thought one of his first cases was one in which suppuration took place. It was a first attack, and the patient had an abscess—probably appendicular. If the operation had not been done, the patient would have been in much danger. The incision should be made in the middle line, and he (Dr. Scott) preferred sterilised water or normal saline solution for flushing.

DR. WEBB (Melbourne) said it was necessary to watch children with appendicitis very closely, inasmuch as the dread of perforation was always present. He did not think there was sufficient inducement to operate early in an acute attack. He would like to hear the opinion of those surgeons who were concerned more particularly with the surgery of children on the question of the liability of persons under eighteen years of age to get perforation. He then dwelt at some length with two cases which he had been connected with. One was operated on on the second day of the attack, and the other on the third day; and in both instances the operations were followed with violent deaths.

DR. HYDE (Clyde, N.Z.) pointed out that an aspect of operating in cases of appendicitis which affected the practitioner more than the patient was this: Supposing a practitioner advised the operation while the patient was in a comparatively good state of health, and, although he backed up his advice with the very best of reasons, the patient refused, and, worse still, was so inconsiderate as to remain perfectly well for years: what, he asked, became of the reputation of the unlucky practitioner? Was it not human nature for the patient to brag of his perfect health, and point to the practitioner as an example, and say, "That doctor wanted to cut me up." On the other hand, if the practitioner did operate and death resulted, what then became of the practitioner's reputation?

DR. BRIGHT (Hobart) said he had had no intention of taking any part in the discussion, but when he saw that the tendency was to inculcate the doctrine that in almost all cases of appendicitis, either in the first attack or in the interval between the first and second, an operation should be performed, he felt bound to rise, and raise his voice against it. He did not look upon the operation for appendicitis as the trivial matter that some speakers had considered it. He knew of one case in which a gentleman went down to Melbourne to see the Cup run. He was told that he had appendicitis. He was operated on by a Melbourne doctor, and, as a result, died. He knew another person who was operated on and also died. He had had three cases under his notice lately, and they had all got well. There had been no recurrence, so far as he knew. As an old practitioner, he was inclined to raise his voice against the tendency to operate either in the first attack or in the interval between the first and second. The system was departing from the ordinary aim of practitioners, which was to do all they could to preserve life. He believed that under the antiseptic treatment of to-day they were overlooking dangers which were dangers indeed. He believed the tendency was to overlook dangers, and he was afraid the question of the probable loss of life was not sufficiently borne in mind. He had not had a large experience of the disease, but he had had some cases, and he should be inclined to have a little more patience than some of the speakers had shown themselves inclined to show. Why this particular organ should be cut out and cast from the person, he did not know. They could have repeated attacks on other organs, but they never thought of operating on them in such a manner. He was altogether afraid that the tendency at present was to operate in cases where the risk could very well be avoided.

DR. CAHILL (Wellington) said he merely wanted to refer to one point. Theoretically, he believed the operation was the ideal method. He, however, did not think it was always the practical one. If they were certain that in all cases of appendicitis they could cut down and find the appendix without any difficulty whatever, the operation would be all right. But when they had great difficulty in finding the appendix he thought that with the softened, congested, inflammatory condition of the bowel, the amount of mechanical work necessary to find the appendix must result in injury to the bowel. Then, again, considering its close proximity to germs in the bowel, especially the *bacillus coli communis*, he thought they were liable in such cases to lead to inflammation. The whole question was one which, to his mind, could not be solved at present, for it would be very difficult to select one case from another. It would be impossible for them to tell whether it would be an easy matter to find the appendix or not. Until that question was decided, they would have to take the chance. So far as he had observed, a patient generally recovered from one attack under general medical treatment.

Dr. Syme (Melbourne) said the subject so ably introduced by Dr. Clubbe was one which had had his attention for a considerable time—in fact, his first paper on the subject was written in March, 1891, and since that time he had made notes on over sixty cases on which he had operated. Unfortunately, not having come to read a paper, he had not separated or classed the cases up to date, but he had notes of papers read on various occasions. With regard to tho ætiology of these cases, it seemed to him that was one of the points which required a good deal of elucidation. If we were able to inform our patients as to what measures could be taken to prevent the disease, or if we could ascertain this more definitely, we might be able to modify the conclusions of Dr. Clubbe's paper. It is a serious matter to advise a patient who has only had one attack of appendicitis to undergo an operation; and while he quite agreed with everything Dr. Clubbe stated on the subject, he was quite satisfied that the proper course from a surgical point of view is, in every case where appendicitis is known to recur, to remove the appendix during the interval. He had operated twelve times, removing the appendix during the interval, and so far without a single death. That contrasted very remarkably with the mortality which is given as an average mortality of 16 L per cent. He quite agreed with Dr. Barnett that a proportion of the cases was tubercular. Now, taking the question—in which this Section is particularly interested—of operation, he (Dr. Syme) could not say that he had departed very much from the opinion he had stated in his first paper in 1891, when he said it was quite possible to divide the cases into classes similar to the division put before them by Dr. Clubbe. He would further say that so far from one attack probably diminishing the liability to further attacks, and that the subsequent attacks would be mild, his experience was that subsequent attacks were more severe, given the conditions you would always expect. In several cases which were operated on after twenty-four hours the patients did just as well as those cases operated on in the interval. He would say the longer the disease goes on the greater the risk in every possible way. His experience was that he was called in generally about four or five days too late. Richardson says the dangerous period is from the third to the sixth day. He found that in practice, whatever may be your theory, you cannot carry out the ideal, and he agreed very much with the remarks quoted by Dr. Armstrong, of Montreal—namely, that you have to judge every case on its own merits, and the surgeon is generally called in at the most difficult period for that decision to be made. The Americans say, "Operate on every case at an early period." It seems difficult as far as the surgeon is concerned, and it is not the safest thing for the patient. Dr. Barnett had said that he would wait till the fourth day and then operate. His experience, however, was that the fourth day was not at all a good time to operate. The same thing had been pointed out by almost all the American observers. His practice had been either to operate very early or to wait a little longer, watching the patient very keenly indeed. They had to use their individual judgment, for there were no two cases alike. The personal equation had to be considered. Another point was about the technique of the operation. He thought it was always advisable when there was a suspicion of more than simply local appendicitis to make a second incision which was best made in the middle line. Great caution had then to be exercised in working towards the sides. With regard to making counter openings in the loin, he thought that if the incision was made far enough back they were very often unnecessary. In some cases he had done it, and he thought with benefit. Then again with regard to flushing out. He had not himself adopted the proceeding of turning all the contents of the abdomen out. He admitted that it was very difficult to cleanse things without doing so, but the shock produced was so great sometimes that he had hesitated in doing it. As a rule it was possible to do the cleansing fairly well without doing so. With regard to the incision made in cases where operations were performed at an early stage, he generally operated along the edge of the rectus, and in cases later on, when he found it dangerous to operate, as far back as possible. Operating for recurrent cases, he generally retracted the rectus and opened the *post rectus*, after which he found that the rectus slipped back and acted as a valve. Now, with regard to appendicular colic. He took it that the complaint was brought about by the attempts of the appendix to cast off something which is not needed. His own impression was, that a great many of the symptoms which they found in recurrent attacks were really due to the presence of adhesions. With regard to mortality: Dr. Clubbe's cases were entirely hospital cases. His experience had been that the deaths in the hospitals as the result of operations were almost double what his had been in private practice. That state of affairs was no doubt due to the fact that all the worst cases found their way to the hospitals. In conclusion, in this disease, more than others, you have to exercise your own individual opinion and individual course of action in every case. With regard to the technique of the operation, the difficult cases are in the female, where you may not only have appendix, but tubal trouble also.

Dr. Hope Lewis (Auckland) said he fully agreed with all Dr. Clubbe stated, and he did not think there was much more to be said after him. With regard to the different theories, the last case he operated on before leaving New Zealand was one of thirteen attacks in fifteen months, and the patient had been advised to wait for another, but he (Dr. Lewis) strongly advised him not to wait. He had operated within the first twenty-four hours and removed the appendix. All he had to say was that he had never regretted operating.

Dr. Nash (Wallsend) spoke with reference to the time for operating. It seemed to him that to lay down rules, stating exactly when they should operate, would be misleading. Professor Tait had laid down rules years ago to the effect that there were two factors in regard to operations. One was the individuality of the patient, and the other was the individuality of the surgeon. In the majority of cases a practitioner had to first survey a patient and take into his mind the condition of the patient at that particular moment. That being the case, before any hard-and-fast rule could be laid down for the guidance of the ordinary practitioner, they should be able to lay down a hard-and-fast rule as to the condition of the patient at the particular moment he is under observation. Such a rule in his opinion could not be laid down. Under any circumstances the rules could only apply to the average case, but as the average case got better, and was therefore all right, those cases which were not average ones were the ones they were more particularly interested in. All he had to say with regard to the individuality of the surgeon was, that an operation which was as simple as A B C to one surgeon would be a matter of serious undertaking to another surgeon.

Dr. Samuelson (Armidale) said he had attended with the idea of learning, and there were one or two matters which prompted him to speak, not that he should instruct, but that he might learn. He had heard, as they all had, of appendicular colic, but to him it was difficult to understand how colic could produce the symptoms they had seen in some patients. It was a difficult thing for him to understand appendicular colic, but recurring appendicular colic was utterly incomprehensible to him. To the general practitioner in the country the time of operation was a matter of very considerable importance, and he had had just sufficient experience to teach him that it was just as well for him to leave the operative treatment of appendicitis as much alone as his conscience would allow. He was perfectly satisfied that it was an operation which one, who could not call himself an operative surgeon, had better leave alone. For that reason he could quite understand how it was that those who were more distinctly operative surgeons should strongly advise operation after the first attack, providing their recommendation was limited to themselves as the operating surgeon, and he only wished they would continue their practice, because, if they had done so in the past, they would have saved those surgeons who were not especially operative surgeons many and many a difficult and fatal case of a recurring attack. Knowing the tendency of recurrence, he should be glad to hear how they were to avoid it. Was it a matter of dieting? Was it a matter of regulating the intestine flux? Were they to keep it quiet, or were they to keep it quick? They were left, in his opinion, on the horns of a dilemma. They were not to operate because they were not expert. Then, he asked, what were they to do?

Dr. Doyle (Newcastle) said the discussion was interesting to him from a personal point of view, for, ten years ago, when he was house physician at the London Hospital, he had had two attacks of appendicitis. He was advised by a very eminent surgeon that it would be very great foolhardiness on his part to leave London before he had been operated on for the disease while it was in a quiescent state. He, however, did not have the operation performed; not that he mistrusted the surgeon's skill, but because it was not convenient for him to have the operation done, and he had never had the slightest symptom since. That was a personal case, and might be interesting to some of the surgeons, as showing that there had been cases which had recurred once and had afterwards not shown the slightest sign. As to appendicular colic, he could say that he had seen a large number of cases, and remembered one which had been put on the table to be operated on. The case turned out to be one of lead-poisoning, brought about by morning beer-drinking. During the last eight or nine years he had not seen a case of appendicitis which had been operated on, and he had not seen a single death from appendicitis during that time, although he had seen a very fair number of cases.

Dr. Moore (Melbourne) said he did not intend to enter into the discussion to any length, simply because he had been unable to be present during the whole of the time. He would, however, like to say a word or two on some points. Referring to appendicular colic, he instanced a case in which he removed an appendix. The appendix was very large and was bound down by adhesions, but from the nature of the adhesions he did not think they could have caused the attack. With regard to operating for recurrent attacks, he did not suppose they should ever come to an agreement on the matter. Patients did recover after one or two attacks, but there was always a great risk that they would not recover. He had met with cases which had forced him to come to the conclusion that operation was advisable after two attacks. Another reason why he had no hesitation in operating in these cases was that in the hands of a surgeon who has been in the habit of performing abdominal surgery there is as little danger in the operation as any operation that is performed With reference to Dr. Syme's incision, he admitted that it made a very pretty operation, but he had a very strong objection to any vertical incision in the abdomen away from the middle line. A certain portion of the rectus muscle was almost invariably paralysed by the operation in question. He then explained the manner in which he operated to overcome the difficulty of injuring the fibres of the muscle.

Dr. J. Thomson (Brisbane) pointed out that the difficulty was to diagnose such cases. He then referred to several cases which had come under his notice, and said he was inclined to fancy that some of the cases that were looked upon as appendicitis were something else altogether. What that something else was he did not know, but he was there to learn. He thought that sometimes cases were mistaken and misunderstood.

Dr. Faulkner (Toowoomba) said as a general practitioner he was very much indebted to Dr. Clubbe for his paper, and agreed with the view he took. There was one thing he was sorry about in the paper, and that was his objection to the term "appendicular colic." No doubt cases of appendicular colic come properly under the notice of the surgeon, but in the course of general practice we did not come across these cases. In one case which came under his notice the patient had had several attacks in the course of two years, with severe pain, temperature 100° at the time the pain lasted, which was for a few hours only. The patient had had eight attacks in two years, and he strongly recommended that he should have the appendix removed, putting it down as a case of this so-called colic. He (Dr. Faulkner) operated on him, and on getting down to the appendix found the appendix standing out at right angles to the gut. In another case he had under his observation the patient had had several attacks in the three mouths; then the attacks passed off after a few hours. In this case he recommended the removal of the appendix if the attacks recurred. He (Dr. Faulkner) considered it advisable to remove the appendix after the first of these attacks, abstracting whatever it is that causes the colic.

Dr. Halford (Clermont) said that previous speakers had dealt but scantily with etiology. In no case was the disease primarily due to micro-organisms. It was not a specific disease like typhoid. Pathologically, the disease must be regarded as an infection complicating an actual morbid state of the appendix. There are always many micro-organisms waiting for an opportunity to complicate seriously any simple affection of this organ that may arise. In a series of attacks in one individual they might find a distinct organism concerned in each case, so that one attack may arise from an invasion by *B. coli communis*, and others might be due to the different pyogenic cocci. This fact of the multiplicity of causes explained the want of immunity noted in the disease, there being rather a liability to recurrence. Similarly in pneumonia they had a multiplicity of causes, and there they found no immunity conferred by a previous attack. This certainly pointed to the fact that the lung of this individual suffering from pneumonia and the appendix of that one down with appendicitis have relatively a lowered vital resistance or a liability to morbid changes precipitating a disease peculiar to each organ. Appendicitis was then primarily dependent for its etiology on anatomical defects or morbid changes in the organ. To prevent the occurrence of the disease, they must remedy the natural or acquired defect. They were, however, only apprised of the morbid tendency by the occurrence of an attack. Thus, an attack was to be taken as a serious warning of the existence of such tendencies, and prevention of further attack could only be reasonably expected by removal of the organ. Early operation would cut short the attack before it reached the dangerous state, and might save the patient's life. This was theoretically better than the intermediate operation, as a certain number of patients must be sacrificed by those who advocate it. Could autitoxic sera only be of use when they were in possession of the precise etiology in each case? However they determined this, delay was unavoidable; therefore, he regarded the successful treatment and prevention of this disease as a matter that was, and always would be, largely in the hands of the competent surgeon.

The President said the question Dr. Clubbe wanted to have some light thrown on was when to adopt operative measures. What they wanted to do was to formulate some general rule that would be of the greatest good to the greatest number of cases. He thought he was right in saying that about 70 per cent. or so of cases of appendicitis got well of their own accord. The other 30 per cent. of cases either got operated on or died. According to Dr. Syme, the operation at the acute stage would not be any more dangerous than one performed in the quiescent stage. In face of that he thought the mortality should not be more than 2 or 3 per cent. at the outside. On account of 70 per cent. of the cases recovering without operation he thought the majority of surgeons got on with other means than operation. His practice was that if a patient had one attack and, after careful dieting got another, he recommended operation. In cases of acute appendicitis, if they were under favourable circumstances he thought it was better to err on the right side and operate. In such cases he had never regretted operating on a patient.

Dr. Clubbe (Sydney) said it was only when the bacteria got into the outer layers of the appendix that symptoms of appendicitis showed themselves at all. It was on that account that surgeons urged that it would be wise for a patient to have his appendix removed under proper conditions. He thought that it was safer to have the appendix removed rather than wait for a second attack, which might prove fatal. They might be dealing with a case of appendicitis in which there was ulceration. In such cases they should be careful, for a second attack would probably result fatally. Therefore he thought as surgeons they were right to recommend a patient, if he could command the services of a man who was accustomed to the class of work, to have his appendix removed after an attack. If they could get patients operated on at once they would, he was sure, get a better percentage of results. In mild cases where that could not be done, they would, of course, temporise. If arrangements could be made later on, however, it would be better to have the patient operated on. Dr. Hyde had raised the question of the difficulty of a doctor recommending an operation and the patient refusing to undergo it and living on for years perfectly well. Of course, surgeons must put up with those contingencies. In urging an operation on his patients he always put before them the full facts of the case. He said, "You have a certain trouble going on in your appendix. You have had an attack; you may never have another in your life; but you may have one again next month which may prove fatal. Therefore, I advise you to make arrangements to have your appendix removed." In such a way he always put the matter before his patients, and he thought it was the best position for a surgeon to take up in the matter. Several of the speakers had stated that they had had attacks of appendicitis, but, perhaps, it was not appendicitis at all. At any rate, he thought they were very fortunate in not having another attack. If he had appendicitis, and he was in a position to command surgical aid, he would have his appendix removed. Dr. Barnett had asked what he did in cases which could not be operated on at once. Well, in such cases he advised the patient to keep in bed so long as there was any pain. He gave them purgatives and kept their bowels open. As to the question of appendicular colic, he did not believe that there was any such disease. He thought it was colic of the appendix without any disease.

SUPRA-PUBIC AND PERINEAL CYSTOTOMY.

By H. V. Critchley Hinder, M.B.,
Sydney.

The operation of supra-pubic cystotomy must always claim a prominent place in the surgery of the urinary bladder, because the supra-pubic route gives us a clear view of the whole of the interior of the viscus, and also affords ready access to pathological conditions when surgical interference is necessary.

Even though the operation of lithotrity has reached the position which it now occupies, owing to the genius of Bigelow, and later on to the dexterity of Freyer and others, who have made such excellent use of the opportunities afforded them, still there is no doubt but that supra-pubic cystotomy for stone will always remain a favourite method with those whose opportunities for crushing stone are so few and far between that they feel that their patient will be safer in their hands if they cut rather than crush.

It has already been pointed out that the supra-pubic operation may be performed with somewhat greater facility in children, because the distended bladder of the child rises somewhat higher than does that of the adult, leaving quite 2½ or 3 inches of the bladder wall uncovered by peritoneum in the middle line. This gives room for a fairly free incision through the bladder wall. In adults greater difficulty has been experienced in making a free incision without opening the peritoneal cavity.

Kocher makes a transverse supra-pubic incision, following out his general rule of making skin incisions along or parallel to the natural skin folds, and, further than this, he cuts through the tendons of origin of the pyramidales and recti, separates the bladder from the back of the pubes, draws it up, and incises as far as may be necessary.

This method is open to serious objections: Firstly, because, unless absolutely necessary, transverse incisions through muscles should always be avoided because the resulting union is apt to leave a feeble scar which is sure to stretch, and, in such a situation as the supra-pubic region, is particularly liable to be subjected to severe strain and consequently would greatly tend to weaken the abdominal wall. Secondly, many bladders opened supra-pubically are likely to be septic, and the loose cellular tissue behind the pubes would certainly be an extremely dangerous situation for the introduction of septic material.

It would appear, then, that room for more freely incising the bladder wall should be sought elsewhere, and this greater freedom can be obtained by separating the bladder wall from the peritoneum superiorly, for so loose is the connection that by this means the bladder wall may be drawn up into the wound, and incised for some inches, if necessary, without opening the peritoneal cavity.

There is another anatomical point worth considering in the supra-pubic operation—namely, that the reflection of the parietal layer of peritoneum on to the wall of the distended bladder laterally takes place at varying distances from the middle line, so that, in one case at least, an incision made into the distended bladder 1 inch above the pubes and a quarter of an inch on one side opened into the peritoneal cavity.

In performing the operation of supra-pubic cystotomy, let the operation area be rendered aseptic, after the method used by the operator.

Kocher professes to disregard the use of rectal or bladder distension, and his method of operating probably allows him to do so to a marked extent.

The operation by vertical incision is certainly safer under bladder distension, and there are very few bladders whose walls will not safely admit of an 8 oz. or 10 oz. distension, although in some cases where micturition has been frequent, and the bladder has retained but 1 oz. or 2 oz. of urine for some considerable time, the reflexes are so very lively that it requires deep anæsthesia to allow of 3 oz. or 4 oz. distension, nor does the distension have by any means an ill-effect, but, on the contrary, is often of service. I certainly have never found the distension to do any damage to diseased bladder walls, such as is alleged may occur.

I myself never employ rectal distension, but distension with a rectal bag holding 6 oz. will surely do no harm; it is very rarely necessary, but there can be no objection to its use if the operator doubts his ability to reach the bladder with ease. In stout subjects with protuberant abdomens, rectal distension will certainly be useful in bringing the bladder more easily within reach of the supra-pubic wound.

Distending the bladder with air is sometimes practised. If water be used, let it be sterile, whether weak solution of antiseptic be added or no. Percussion affords one a very fair idea of whether the bladder is near the abdominal wall immediately above the pubes.

The incision should be a vertical one through the skin, extending from the pubes upwards for 3 inches or 4 inches, and directly in the middle line. If on cutting through the linea alba the incision happens to carry the operator into the sheath of the rectus, let him feel with the handle of the knife for the linea alba and start afresh. If by any chance he incises so much from the middle line as to by accident open the peritoneal cavity, this really matters little, as the opening may be closed with a catgut ligature and a fresh incision made.

On passing thorough the linea alba, draw the edges of the wound asunder, divide the cellular tissue with knife or dissecting forceps, and the bladder wall may be easily felt or recognised by the smooth layer of fat covering it, through which frequently course fairly large veins. On dividing this the muscle fibres of the bladder can be easily detected. In order to be perfectly certain, a fine aspirating needle may be thrust through, and, if a positive result be obtained, plunge the knife boldly in in a vertical direction, and the fluid or air will rush out as the blade is slightly rotated. Take out the knife, enlarging the wound towards the pubes, and slip in the left forefinger in its place. This stops the rush of fluid, and allows of digital exploration if necessary with a taut fairly distended bladder. The finger, too, is of great assistance in enabling one to pass through the bladder wall on each side temporary double silk ligatures, which act as retractors in keeping the wound open during subsequent manipulations.

The bladder incision may now be extended to the pubes, and by means of the forefinger in the bladder after the incision through the linea alba has been enlarged to, say, 3 inches or 4 inches upwards if necessary, the bladder wall may be lifted up, and by pushing the peritoneum off the bladder wall to which it is fairly loosely attached, and drawing the bladder wall up into the wound with the forefinger, the bladder may be divided bit by bit for some inches without opening the peritoneal cavity.

This procedure is at times of great use in cases of prostatectomy. The vessels divided in the bladder wall will rarely require ligature. Specula of various kinds have been used for examining the bladder through the supra-pubic wound. A simple glass Ferguson's speculum is used at times, and some surgeons prefer dilating wire specula.

I have found nothing better than two specula very like Ferguson's, but cut across squarely at the smaller end and fitting fairly accurately, the one within the other. The inner tube differs from the other only in having a plain glass window at the bottom. By the help of these, if the wound be just large enough to allow the tubes to slip in, fitting closely, the bladder may be filled with water by means of the larger tube, while the smaller tube with the window is used to push the water down in a piston-like fashion, and too free leakage is prevented by wrapping a swab round the inner tube.

The advantage of this arrangement is that the bladder remains distended while, with the assistance of overhead light from window or mirror, the greater part of the bladder wall may be easily examined while stretched out. If a single open tube speculum be used, the puckered mucous membrane rolls into the end of the tube, presenting a rather deceptive appearance, and the tube cannot be moved about with any degree of facility.

If it be desired to carry out operative procedure, the outer tube is pressed over the growth, the inner tube is removed and the growth attacked. If hæmorrhage is free, the vessels may be caught with long forceps, or the tube may be firmly pressed down and tampons of gauze or hot gauze pads pressed on the bleeding part till hæmorrhage is arrested.

It will be found that this method is a distinct advantage over that of stuffing the bladder with gauze for a time; and by means of this tube only can any marked pressure or even a dull cautery, if necessary, be applied to the part, though I have never found this latter necessary.

A free incision into the bladder may be made supra-pubically even in cases where it is not possible, owing to inflammatory trouble, to strip the peritoneum from the bladder. Under these circumstances, I have found that, with the left forefinger in the supra-pubic wound pressing the bladder against the upper angle of the wound, the peritoneal cavity may be opened and a catgut suture passed through so as to unite the peritoneum covering the bladder wall, as it bulges up into the wound, to the parietal peritoneum, so as to make this water-tight and prevent the general peritoneal cavity from being contaminated. When the suturing has been completed, the bladder wall may be incised with a pair of scissors.

If the Trendelenberg position be adopted and a good free incision through the bladder wall be made by either of the methods mentioned, when the edges of the wound are drawn aside an excellent view of the bladder may be obtained, and all details clearly displayed and operative procedures of magnitude conducted with great freedom.

It has seemed to me that there is a marked want of thoroughness in the removal of malignant bladder growths. At the very least they should be excised as well as the complete thickness of the bladder wall for a fair distance round.

If it be necessary, open the peritoneal cavity, and shut off the operation area with sponges; freely divide the fundus of the bladder wall, remove the growth, and suture the bladder as one would do intestines.

If an aseptic operation has been performed in a bladder with aseptic urine, there is no reason why, provided hæmorrhage has ceased, the bladder should not be sutured with a double row of continuous sutures, and, inasmuch as it is somewhat difficult to bring about an accurate approximation, it is safer to insert sutures through skin and muscle, &c., and leave them untied; fill the wound with sterilised gauze, which, if the bladder wall remains sound for two days, may be removed, the sutures tied, and a good secondary union obtained. Even if the bladder be septic, and it has been decided to use a perineal drain, the wound may be treated in the same way; but perhaps sterilised iodoform gauze would be more efficacious, though about this I have some doubt. If at the end of two days, even with septic cases, the wound shows no sign of suppuration, then I have always found it safe to fasten the sutures.

The supra-pubic and perineal are both good routes for bladder drainage, but the perineal is better for the following reasons:—The supra-pubic syphon drainage may answer well at times, and if under the supervision of a skilled person, but under any circumstances it is without doubt sometimes extremely troublesome owing to the tube becoming blocked, even though two or three openings are present, with the consequence that the patient becomes uncomfortably wetted. If assistance be at hand, however, this is easily remedied. If there be much mucoid or purulent or phosphatic material in the bladder, the tube is very frequently blocked, and the drainage eminently unsatisfactory.

If the supra-pubic method be used in a case where the bladder rest only is required, and where the urine is clear and fairly normal, then the supra-pubic syphon will usually give its best results. In women, perineal drainage may often be conducted very comfortably through the urethra with a double-channelled glass catheter.

Perineal Cystotomy is an operation which, if neatly performed, is all over in about one minute, and with the least damage to the patient.

Like many other simple operations, its simplicity depends on a close attention to details. All the instruments needed are a long narrow-bladed knife, a centrally grooved staff, and a gorget. The gorget requires special mention. The ordinary gorget is probe-pointed and necessitates the removal of the knife before it can be passed; and when it is passed, as often as not the groove in the staff cannot be felt, and sometimes it may be necessary to reintroduce the knife. All this may be got rid of by constructing the gorget in such a way that the groove on the upper surface runs right to the very tip.

The patient is placed in the lithotomy position, the staff passed and held as usual by the assistant, who at the same time holds the scrotum out of the way by clasping it against the staff with his little finger. Instruct the assistant to press the shoulder of the staff downwards and towards the operator, so as to bulge the perinæum, and to keep it strictly perpendicular and in the mid-line.

Make an incision three-fourths of an inch long in the median line of the perinæum whose posterior limit reaches to within three-fourths of an inch of the margin of the anus. There is no need to introduce the finger into the rectum as a guide, and besides the finger will be rendered septic. Plunge the knife into the perinæum in the upper or anterior part of the incision, at first depressing the blade somewhat so as to escape the bulb, then pass straight on slightly upwards, so as to strike the groove in the staff just behind the shoulder. Press the knife along the groove so as to incise the urethra for three-fourths of an inch, then take the grooved gorget upside down and slide it along the back of the knife till it strikes the metal in the groove of the staff, and hold it firmly in the groove. Withdraw the knife so that it widens the incision as it is withdrawn. Pass the gorget on into the bladder, and, as the urine rushes out, let the assistant disengage the staff as the operator turns the gorget round, dropping the handle in such a way that the grooved surface of the gorget is now directed upwards. Pass in the perineal drain and withdraw the gorget.

A clean longitudinal incision thus made is never followed by stricture as some suppose, but heals with a fine linear scar. This I have verified *post-mortem*.

The danger of wounding the rectum is unduly magnified. I never saw a case where this has been done. The introduction of the finger into the rectum while it may be necessary to use the same finger later on is hardly good practice. If it be considered necessary to do so for any special reason, a rubber finger-stall should be worn.

It is probably owing to the haphazard way in which the perineal drain has been used that this route has met with some slight disfavour. I have seen a tube thrust in and the other extremity passed into a urinal, or tied in and the extremity placed in a vessel by the bedside. It is absolutely necessary that if a tube be used at all it should be removed and thoroughly cleaned once a day, and that it should be a tube such as will allow of a thorough washing-out of the bladder with the greatest facility every three or four hours whilst the patient is awake. It must be remembered that aseptic principles should be as closely applied to urinary surgery as to any other branch of surgery. In order to do this I use a metal tube which I have already described and figured in the *Australian Medical Gazette* for 20th February, 1899. It has so far given me every satisfaction.

There is a certain class of cases which cannot be well drained by the perineal route *viâ* the urethra. I allude to those in which the prostate is considerably enlarged, and in such a way as to leave a post-prostatic pouch.

Here a calculus frequently forms, and neither the ordinary perineal nor supra-pubic drain is likely to be of great service.

If there exists, then, a large prostate and a post-prostatic pouch, in which is found either a calculus or a pocketing of ammoniacal urine, thick with phosphates and pus, and it be considered inadvisable to remove the prostate, drainage should be established in the following manner :—

If a supra-pubic opening has already been made for the removal of stone or for other reason, instruct an assistant to place the instrument (the handle of a sound will do) through the supra-pubic wound into the pocket. Then the operator, having placed his patient in the ordinary lithotomy position, covering his left forefinger with a rubber shield, slips it into the rectum until he feels the tip on the handle of the sound in the bladder. With this guide he cannot well go wrong. He makes a small skin incision half-an-inch in front of the anus, and thrusts the knife straight in till he strikes the guide in the bladder. The grooved gorget is passed along the back of the knife till it passes into the bladder, the finger is withdrawn from the rectum, and the shield removed. The operator then passes the finger through the supra-pubic wound till he feels the gorget safe inside, and the hole in the bladder large enough. The drain is then passed along the gorget in the usual way.

If the bladder be full and felt bulging into the rectum, it is quite a safe procedure to pass a long-bladed knife, after Cock's method, right into this pouch, guided only by the finger in the rectum, though this should not be done till cystoscopic examination indicates that no stone is present, for operative interference with abnormal bladder contents could not be very satisfactorily conducted through this opening. If necessary, after a time, a permanent but flexible rubber tube might be retained with a tap attached, though it should be removed and cleansed of phosphatic concretious once daily.

Such a procedure as the one I have described is invaluable in those cases from which a stone has already been removed by the cutting or crushing operation, and when stone is likely to re-form. Patients of this class may be found to return every year with a freshly formed stone needing removal. This can be obviated by keeping the prostatic pouch clean, and to do this is extremely difficult, to say the least of it, if not impossible, by any other means. In young men the post-prostatic pouching sometimes exists, if there be urethral stricture present, but usually with the removal of the cause the bladder quickly recovers. The fibrous wall of the chronically thickened degenerated bladder of an old man will alter but little.

The track left by a perineal drain, too, in young men closes often in two or three days, while there is much less difficulty in maintaining the patency of the perineal opening in old men, for frequently six to eight weeks will elapse before urine will cease to pass by the perineal wound after drainage has been discontinued.

In conclusion, then, I may state that there is no real rivalry between the supra-pubic and perineal routes.

The supra-pubic syphon is suitable for cases where the bladder is not septic, but in septic conditions perineal drainage, either by incision through the urethra or directly into the post-prostatic pouch, as the case may demand, is by far the most satisfactory method.

(This paper was illustrated by a series of explanatory diagrams.)

SOME FORMS OF CYSTITIS.

By Alfred Austin Lendon, M.D. Lond.,
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I am not quite sure how far the descriptions I shall give you of cystitis, and the views to which I shall give expression in this paper, will be considered novel. Possibly you may tell me that the facts are all familiar to you, and the explanations simple and already universally accepted, but my descriptions and my views appear to me to differ in some important respects from those given by English and American writers of the present day. For example, neither in Henry Morris nor in Howard Kelly's works can I find an account of some of the forms of cystitis as I have seen it both in hospital and in private practice. The explanation of this is, I take to be, that we in general practice often encounter ailments and phases of ailments which do not usually come under the ken of the consulting physician or surgeon, and which are not exactly severe enough to require the admission of the sufferer to a general hospital.

In the first place I look in vain for an adequate clinical description of what I should provisionally term *catarrhal cystitis*. One writer (H. Morris, quoting Guyon) speaks as though acute cystitis never occurred without pus being formed, although he admits that it may not be present at first. Another writer (Howard Kelly) speaks of purulent cystitis as a "higher grade" of ordinary "inflammation." Now, whilst I do not deny that a few leucocytes may perhaps be found under the microscope, or that there may be a trace of albumen, what I wish to demonstrate, and to insist upon, is that there is a form of cystitis which may be acute, or may be chronic, in which pus does not appear from beginning to end of the disease, and in which there is no tendency to decomposition of the urine in the bladder. In support of this contention I will read brief notes of a couple of cases.

Mrs. J., æt. forty-six, consulted me for frequency of micturition, which had followed an attack of diarrhœa three weeks previously; micturition was frequent, both by day and night, and there was pain both during and after the act; the temperature was 99·5°. The urine was faintly turbid, of neutral reaction, sp. gr. 1004, non-albuminous; under the microscope a few epithelial scales and débris were seen. Pelvic examination revealed no uterine trouble, although the patient had obvious symptoms of the approaching climacteric. She was given a mixture containing benzoic acid, borax, and buchu. In two days the temperature was normal; in a week she could pass the whole night without micturition, though there was still some frequency by day; at the end of a fortnight this latter symptom had disappeared. Examinations of the urine showed on these several occasions a progressive improvement in the specific gravity—viz., 1005, 1010, 1020; there remained a little general weakness and some digestive disturbance, which were soon rectified. This I should consider a subacute case. I have met with more acute cases., *e.g.*, in a gentleman of forty-two, after acute diarrhœa contracted up country, but it was equally amenable to the same treatment.

A more chronic case was sent to me a year ago under the following circumstances: A miner, thirty-seven years of age, had suffered from some obscure bladder trouble for fifteen months; in the more acute stage the symptoms comprised pain before micturition, which was frequently necessary, the passage of but a small quantity of urine together with a few drops of blood, and the deposition of a slight sediment from the urine on standing; but of late months he had vastly improved, although still unable to work. He went into a certain public institution which shall be nameless, and, after the bladder had been sounded and otherwise examined, he was pronounced to have a tumour of the bladder; it was further intimated to him (so he informed me) that his choice lay between going into the Home for Incurables and submitting to castration. From this we must infer that an enlarged prostate was diagnosed. On examination I found that the urine was faintly acid, its sp. gr. 1020; it contained a trace of albumen, but no sugar; it was quite clear and nothing was found under the microscope; I sounded him but found no stone in the bladder; his prostate did not feel enlarged per rectum. He complained of frequency of micturition and other signs of vesical irritability. The same mixture was given as in the previous cases; twelve days later it was noted that he was much improved and the amount of albumen less; in two months' time he was at work again.

The points upon which I desire to lay stress are:—

(1) The very considerable amount of discomfort caused without any correspondingly obvious change in the urine. The patient may look and feel ill, may be quite unfitted for work, his night's rest, as well as his occupation by day, being constantly disturbed by the calls to micturate; yet the urine is acid, there is merely a slight turbidity at the time it is passed, which results in an amorphous sediment which cannot be microscopically identified.

(2) The absence of any tendency to the formation of pus in the bladder, however acute the symptoms may be.

(3) The absence of any tendency to decomposition of urine with the production of triple phosphates.

(4) The complete and rapid cure with drugs internally, and without the necessity for washing out the bladder.

Now, contrast with the foregoing cases other instances of cystitis in which pus is present in considerable amount. A lad of eighteen consulted me on account of frequent micturition, and his urine when passed was as thick as pea soup, but strongly acid in reaction; on standing, a deposit of pure pus occurred: he had recently been treating himself for gonorrhœa by means of an urethral injection. Besides the frequency of micturition, there were no severe symptoms; there was no fever; the pain was slight; there was merely general malaise, caused through broken sleep, loss of appetite, and mental worry. This purulent cystitis proved very intractable to internal treatment; the copaiva, which subdues urethral inflammation so readily, appeared to have no effect; the benzoic acid mixture also failed, nor did it yield to washing out of the viscus with boric acid or thymol lotion. Directly, however, a solution of mercuric chloride was employed the cystitis improved, and the patient speedily got well.

Again, a multipara had retention for some twenty hours after a tedious confinement; a silver catheter was employed without what we should now consider adequate antiseptic precautions; retention of urine did not occur again, but within a couple of days the frequency of micturition, pain during as well as before and after micturition, and turbidity of the urine, proclaimed the onset of acute purulent cystitis; the pus was as copious as in the gonorrhœal case, and the urine was always acid; the benzoic acid treatment failed, the perchloride of mercury lotion speedily cured.

Here, then, we have two instances of fairly acute purulent cystitis, which differ from the previously-described cases of catarrhal cystitis in the copious secretion of pus in the former, and in their origin from obvious external causes, but agree in this respect that in all the urine throughout remains acid; again, whereas the catarrhal cystitis is speedily cured by internal remedies, these are useless in purulent cystitis, which requires local treatment.

With regard to the use of perchloride of mercury I should advise commencing with a strength of 1 in 20,000. I have known 1 in 5,000 cause great distress, though by another patient it may be well borne. Another point to which I would draw attention is the absence of any considerable degree of pyrexia in catarrhal or purulent cystitis. We notice the same thing in catarrhal and gonorrhœal ophthalmia. This apyrexia prevents the drawing of a distinct line of demarcation between acute and chronic catarrhal or purulent cystitis. Gonorrhœa, by the way, is not always followed by purulent cystitis. It is far more common to meet with a catarrhal cystitis after an attack of gonorrhœa which speedily yields to benzoic acid. On the other hand, I have frequently met with catarrhal cystitis for which no cause other than "taking cold" could be assigned.

This brings us to the question as to the various immediate causes of cystitis, and here I feel that I am venturing upon less safe grounds than when I am dealing with the clinical aspect of the disease. As regards suppurative cystitis, we know that no precautions worth mentioning were taken to render catheters aseptic some twenty-five years ago, and yet how rarely did cystitis follow the use of a catheter. We are told that catheterisation cystitis has been only slightly reduced in spite of most careful sterilisation of instruments (Rovsing), and that the normal urethra swarms with the cystitis microbe. It would seem as though some other factor were required to start a suppurative cystitis than the mere introduction of the pyogenic microbe; such, for instance, as a slight wound inflicted by the catheter perhaps, or lessened power of resistance to infection on the part of the mucous membrane from detention of urine or from hyperæmia. Is gonorrhœal cystitis again a specific infection, or is it that the presence of the gonococcus somehow prepares the way for the suppuration of which the staphylococci are the actual cause? This theory would explain why the cystitis which accompanies gonorrhœa is of the catarrhal form usually and only rarely suppurative. We see the same thing occurring in gonorrhœal orchitis: severe inflammation is the rule, and suppuration the rare exception. I should be inclined to consider all suppurative cystitis as due to the staphylococci, and the gonorrhœal variety to be an instance of mixed infection. Catarrhal cystitis is also believed to be microbic in origin, and to be determined by the presence of specific non-pyogenic microbes, *bacilli coli communis*, &c., in a bladder predisposed to infection as before suggested; this predisposition seems a necessary assumption, as it is known that typhoid bacilli and pyogenic cocci may traverse the bladder without exciting cystitis: the infection of *B. coli communis* is supposed to be hæmatogenous, and to reach the bladder *viâ* the kidneys. A direct invasion of the bladder from the rectum where the mucous membrane of the latter is inflamed or abraded would be a fascinating theory, were catarrhal cystitis restricted to the male sex.

Another variety of cystitis is that in which the urea is decomposed, the urine being strongly ammoniacal when passed, turbid in appearance; a copious deposit of triple phosphates speedily taking place; there is no tendency to suppuration.

Miss M., æt. twenty-six, consulted me on 28th May, 1898, for severe dysmenorrhœa of twelve years' standing; the urine was then acid, sp. gr. 1,030, and showed a deposit of uric acid crystals and amorphous urates; her weight was 8 st. 5 lb. On 8th June an examination under ether was made, and the uterus curetted; the same evening urine was passed voluntarily, though with some difficulty; none was passed on the 9th, and the patient did not draw either the nurse's or my attention to the fact; hence it was assumed that it had been passed. On 10th June a catheter was passed by a probationer, under the superintendence of a charge nurse, and by the same probationer in the evening; on both occasions it caused pain, and the operation appears to have been somewhat bungled; it is alleged, however, that a rubber catheter was used, and that it was disinfected in the usual way. On the 11th the water was passed naturally, but from this date its passage was always accompanied by pain, and there was a distressing frequency; patient left the private hospital on the 14th. On the 18th the urine was acid, and contained an excess of phosphates, besides a trace of albumen, but no pus; the benzoic acid mixture was prescribed; on the 23rd June a few stray pus cells were seen under the microscope, and there was still albumen. On 2nd July the bladder was first washed out with perchloride of mercury lotion; the same treatment was continued on alternate days, and finally every day, the strength of the lotion being increased. On 3rd August the urine contained no albumen, but it was acid and deposited urates; the distressing symptoms continued during the next two months; though there was no fever, the patient lost a stone of weight, and her digestion became much impaired. Alkalis and bitters, acid and bark, phosphoric acid were all tried as medicines; first, nitric acid, then boro-glyceride was tried as lotions, but without the slightest improvement. By the end of September a great change had occurred in the urine; when passed it was as thick as pea-soup, and like it in colour, ammoniacal in odour, and strongly alkaline; on standing, a dense precipitate was deposited of triple phosphates—under the miscroscope, myriads of those wriggling animals we always see in decomposing urine (the urea bacillus), but no pus cells; sometimes a little, but usually no albumen was to be found. For the next three months patient was under constant treatment; the bladder was washed out daily, except during the menses. Solutions of formalin, of silver nitrate, of mercuric chloride, and of boracic acid were in turn persevered with; by the mouth she was saturated with salol and other drugs. In January, 1899, I introduced a winged male rubber catheter into the bladder, and kept it in constantly, except during the menses; the bladder was washed out, and antiseptics were given by the mouth, including urotropin, which has a reputation for destroying the germs that occur in cystitis. I cannot say that there was any obvious improvement, though patient thought that there was. In February the catheter was only removed during two days of the menses, and the washing out of the bladder was managed by the patient herself. On the 21st capsules of santal-midi were commenced; an improvement was soon obvious. In less than a month all signs of decomposition had disappeared, and the catheter was dispensed with. There was now only a slight deposit from highly acid urine; a few pus cells and oxalates were seen under the microscope; a month later there was neither albumen nor pus, but only urates.

I narrate this case at some length, because it is unique, in my experience, as an instance of an almost pure form of cystitis resulting from the decomposition of urea in the bladder by the urea bacillus. I say an almost pure form because it was preceded by a cystitis, which one could not distinguish from the catarrhal form previously described, except by the failure of treatment to relieve it, and by the fact that there was a history of possible injury inflicted upon a long distended bladder; the intractability of this cystitis to treatment must be explained on the assumption that the lesion of the mucous membrane did not heal; we know how disinclined to heal is the simple solitary ulcer not due to traumatism (Fenwick). If we eliminate this preliminary catarrhal cystitis, we may say that it was a pure form of urea-decomposition cystitis, for there was neither pus (other than in microscopical quantity) nor mucus mixed with the phosphatic deposit.

Does this decomposition of urea take place in the bladder, or in the kidney, or its pelvis? I had a boy with extroversion of the bladder. After the failure of many plastic operations, I succeeded in transplanting the left ureter into the rectum, but failed in the attempt with the right ureter; at the time of the operation it was noticed that the right ureter was normal in appearance, whereas the left was much thickened. After the operation, the urine passed by the rectum was still ammoniacal, whilst the bladder surface had lost that peculiarly pungent odour which had previously been so marked.

Lastly, I should like to allude to, though not to dilate upon, those mixed forms of cystitis which are more familiar to us, and which embrace many of the cases which are very chronic in duration. The mixed infection may be that of the catarrhal and purulent forms, or of either of those forms with that of the urea-decomposing variety; possibly of all three; we see these mixed forms in cases of cystitis associated with the presence of stone or tumours, or foreign body in the bladder. A specific form of infection is the tubercular cystitis which usually becomes complicated with, or else a complication of, the purulent and the urea-decomposing forms. One form I have noticed in cases of extroversion of the bladder is peculiar; in addition to the decomposition of the urea there is a tendency when the mucous membrane is irritated to the secretion of abundance of transparent tenacious mucus; after an operation in such a case an outside rubber tube is speedily caked with phosphates, and the slimy mucus hangs about everwhere. In chronic cystitis from stricture, I have seen this also; the mucus forms a semi-transparent coherent deposit, which can be lifted out of the conical glass of urine on a glass rod leaving no other sediment, and which resists efforts to tear off pieces; if admixed with some pus cells, it will have a more opaque look.

NOTES ON SKIAGRAPHY.

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In view of the fact that the "Röntgen rays" have become an essential part of the equipment of up-to-date surgery and medicine, not alone for the more accurate diagnosis of pathological conditions of the organism itself, but also for the diagnosis and localisation of foreign bodies (otherwise almost impossible), I have been induced by my colleagues to place before the members of the Congress some of the more important results of my experience in this matter of over three and a-half years, dating in fact from the inception of the practical application of the "rays." Assuming that the members are familiar with the apparatus in general, I will confine myself to some remarks upon a few of the more important details, both in construction and application, as they have suggested themselves to me. While some of these have been the result of my personal observation, it is quite possible that they may have occurred to others working in the same field.

Firstly, as to the power: I have had opportunity of testing, I may say, every kind of power suitable for energising a coil for the production of the rays. I first of all tried the primary batteries—Bunsen, Groves, and Bichromate. The two former had their objections—the latter also, though to a less extent; but, in fact, every primary battery is objectionable for this work on account of the time required for its attention. Discarding primary batteries, I substituted secondary batteries or accumulators. These I found suitable, except for the slight inconvenience of having to remove them to a station to be recharged. I then experimented with the electric current from the main, 100 volt circuit (alternating), by transforming down to 30 or 60 volts, and running the current through a chemical rectifier. This system answered very well, but, of course, the solution was not everlasting, and like primary batteries required attention. Under my direction the Melbourne Hospital has been fitted up with rectifiers and transformer, which have worked very satisfactorily, enabling me, for instance, to take a case of a bullet in the brain, which required a long exposure.

With this rectifier, a contact-breaker is used, although working with the alternating current. If an alternating current be used without the rectifier and a reversal of current occurs, the tube may be damaged, for the current is only now and again running in the right direction. The advantage of this rectifier is that it practically allows the current to run in one direction, thereby becoming an almost continuous current, although, when the cells want replenishing, there is a slight reversal apparent in the tube.

There is another way in which the alternating current may be utilised —by means of the Wehnelt interrupter. Although I have not heard of this being used in connection with an alternating current, but only on a continuous circuit of 100 volts or less, it occurred to me to try it in series with the chemical rectifier. The result was feeble, owing probably to the fact that the voltage suffered a loss of 66 per cent. I therefore tried it direct from the main, and obtained a very satisfactory result. The intensity of the rays was so great that, even with an incandescent lamp of 16-candle power burning continuously throughout the exhibition at a distance of 5 feet, I was able to see, by means of the screen, the bones of my arm and hand quite distinctly. In order to obtain the best results with the screen, it is necessary to have the room darkened, but the above will serve as a comparison to show the intensity of the rays obtained by this interrupter. The interruptions may be increased or decreased, according to the size of the active electrode (anode) in the interrupter. The smaller this electrode, the more rapid the interruption. My method at the Melbourne Hospital is to pass the current first through the transformer and then through the interrupter, thereby regulating my pressure, which I found to work best at 60 volts, with the particular lamp I was using at the time.

Mr. Campbell Swinton has discovered that the frequency of the breaks depends on the self-induction in the primary circuit, and also on the size of the active electrode (anode). There is no doubt but that the Wehnelt interrupter has greatly improved the efficiency of induction coils for the production of "Röntgen rays." You get by this means a steadiness of the image on the screen, which is unobtainable by other contact-breakers. The danger in using this interrupter is the fusing of the anti-cathode; but this can be overcome by using a transformer. This Wehnelt interrupter has also another advantage —namely, that, as soon as contact is made in the circuit, the coil is at once energised; there is no danger of stoppage, as in other contact-breakers. On account of this, it has suggested itself to me that in order to take cardiac enlargements, aneurisms, &c., an instrument might be employed on the principle of the metronome—making contact simultaneously with the contraction of the heart; this would energise the coil and tube at the period of systole, and you would thereby get a skiagram of the contracted heart (close of systole), which would do away with that penumbra which is always present in cardiac skiagrams, and which in my opinion is exaggerated in proportion to the rapidity of the heart's action. Similarly with diastole. Take a patient with a pulse of 120 and one of 60: the heart of the former would throw a larger shadow than the heart of the latter, but by the above device the exact size would be obtained in both cases independently of the alteration in pulse rate.

I was favourable at one time to the mercury break, either worked by motor or attached to the coil, but the objections to it are that it requires continual attention, the mercury gets oxidised, and the contact is apt to be uneven. The Wehnelt interrupter has, in my opinion, superseded all other breaks. One thing noticeable in the discharge in the tube by using the Wehnelt interrupter was that in placing the screen behind the anti-cathode the bones of the hand and arm were easily seen on the screen. This result, of course, is unobtainable with the ordinary break, as the rays are only given off anterior to the anti-cathode, and not posterior as just stated. For those who only occasionally use the apparatus, perhaps the simpler contact-breakers are more convenient. Of these Watson and Sons' "Vril" break is probably the best.

The Coil.—So much for current and breaks. Concerning the coil, little need be said. The most useful size of coil is probably a 10-inch. I possess a 20-inch coil, which was made by Mr. Tilley, of Melbourne, and has given every satisfaction; I have also an 8-inch, which is capable of doing almost any work, especially with the Wehnelt interrupter.

Tubes.—Since the discovery of the rays, great improvements have been made in the construction of the tube. Professor Jackson's focus tube has given great satisfaction. Mr. Campbell Swinton's adjustable cathode tube is also useful, but one called the "Queen," invented by Mr. Sayen, of America, of a more ingenious construction, is said to be good for screen work, but not for skiagraphy. My experience, however, has proved it most valuable in both. This tube is automatic in regulating the vacuum, so that, in screen work, by the time the eye is accustomed to the screen, it works up to its greatest intensity, and is capable of penetrating any part of the body.

Exposure.—Great improvements have also been made in the direction of shortening the time of exposure. Experience alone can guide here. No fixed rule can be laid down, except that the duration of exposure should be inversely as the square of the distance from the tube to the plate.

Position of Tube.—The position of the tube in taking a skiagram is very important, especially in localising foreign bodies. In fractures and dislocations it is less important. Particular attention must be paid to the lateral position and distance of the tube, otherwise the shadows will be greatly distorted. In the case of foreign bodies—*e.g.*, say a needle in the hand—if you place your tube so that the central rays impinge on the base of the phalanges, and find in the skiagram the needle lying over the base of the metacarpal bones, with a penumbra lying on the carpal side of the needle, it may be due to a number of faulty conditions—the tube may not have been central enough, and the external rays have been allowed to impinge on the foreign body at an acute angle, giving the penumbra on the farther side from the tube. Again, if the needle has a double penumbra, the lamp has probably been too close to the object, or the foreign body may be deep in the part that is more remote from the plate. I have a plate regulated for the hand, say, and by the distinctness of the shadow I am able to judge approximately the depth of the foreign body; I judge its location laterally by means of a pilot needle. This I have found invaluable. I may mention two cases which put it to a severe test—in one, a needle in a child's buttocks, which was broken into two parts. These were removed without difficulty. In another case, there was a needle in the glutæus maximus of an adult. I found a pad of cotton wool and collodion placed over the incision, which had been previously made by the attending physician, who had failed to find the needle. Previous to arranging the limb on the plate, I inserted a needle just beneath the skin one inch to the inner side of the wound, perpendicular to the fold in the nates. The negative—which I am sorry to say was spoiled owing to an accident, so that I was unable to obtain a print—showed the foreign body lying external to the pilot needle, and made the localisation so accurate that the surgeon cut down on the mark I had made, came right on the needle, and extracted it without any difficulty.

In reference to the position of the tube, I omitted to mention that there is a focal distance. I mean by this that if a tube were placed, say, 4 inches from a hand, the skiagram of the hand would be greatly distorted—the shadow would be much larger. You may put it this way: that the nearer the tube is to the object, up to a certain distance—that is, the focal point—the larger and more distorted will be the skiagram; and the nearer the object to the plate, the sharper the negative—that is, provided the tube is at or beyond the focal point. Again, the greater the distance the tube is from the plate, the longer the exposure, and this is inversely as the square of the distance, as previously mentioned. Owing to the non-refrangibility of these rays, it is impossible to obtain a skiagram smaller than the object itself—that is, all skiagrams must necessarily be of the same size as the object skiagraphed, or larger. Negatives may be reduced afterwards, as in lantern slides.

For localising foreign bodies in the eye, the localiser that has met with most favour is that devised by Dr. Mackenzie Davidson, which is somewhat complicated. I am in the habit of using the following simpler method:—After placing the patient's face with the affected eye nearest to the plate, I place on the upper and lower lids,-in the median line, two small shot, which may be fixed with absorbent wool and collodion. With practice, one will be able to locate the foreign body in its relation to the two pilot marks.

Screen Work.—For screen work you require your tube to be working well. I test my tube first with what I term a fluorometer, which is simply my hand. Here again the requisite knowledge is only gained by experience, the density of the shadows on your screen being your guide. Fluorometers are sold consisting of a block of aluminium, *e g.*, one quarter of an inch thick, with holes bored into it at various depths, *e.g.*, in No. 1 the deepest hole would be more transparent than, say, three or four removes from this. You may not have this handy, but your hand always is.

Preparation of Patient.—Beyond what I have said as regards locating more accurately foreign bodies, I may mention that for abdominal skiagraphy, *e.g.*, stone in the kidney, it is advisable to prepare the patient by administering large enemata to remove all fœcal matter. It is also advisable to cover the parts submitted to long exposure by a thin fabric (silk or linen). I have been accustomed to do this, hence probably my immunity from a resulting dermatitis.

Results in Diagnosis.—A very useful application of the Röntgen rays has been made in diagnosing a lung trouble not discoverable by ordinary methods. In this way I discovered a consolidated patch about the size of a half-crown situated in the centre of the middle lobe of the right lung. This case came to me suffering from a fracture at the neck of the humerus, and after taking a skiagram I put the screen on the chest and saw the patch. I told the attending surgeon what I had discovered, and he informed me that the patient had been under several physicians, but nothing had been discovered either by percussion or the stethoscope, although from other clinical signs he suspected lung trouble. This is easily explained by the patch being superimposed by healthy lung tissue giving normal sounds. Hydatids of the lung are very easily diagnosed by means of the rays. Miss D—— consulted me about some slight ailment, and, noticing her breathing rather short, I examined her lungs and found the right side somewhat enlarged, which she informed me had been the case for some years. I then examined her with the screen, and found the right side of the chest very opaque to the rays up as far as the third intercostal space, and above this line the lung was quite transparent. This line did not correspond to the line of dulness of percussion, which was an inch or more lower. This is easily explained by the bulging effect on the upper surface of the cyst, so that the anterior surface was covered with lung tissue giving a resonant note on percussion. This case was operated on by Mr. Syme, at St. Vincent Hospital, and a very large hydatid cyst removed. This was found to be situated on the upper surface of the liver. Some weeks after leaving the hospital she consulted me again, and on examination I found the heart displaced upwards and inwards to the left nipple. I again examined her with the screen, and diagnosed another cyst on the upper and outer border of the spleen, showing distinctly crescent-shaped upper surface pushing up the diaphragm like the one on the liver, only to a very much less degree.

Pleuritic effusion and empyema are also capable of being diagnosed by means of the screen. A case of empyema, where it had ruptured into a bronchus, and had been operated on by removal of part of a rib, may be instanced. On examination, the patient had a tube in the side and was discharging a small quantity of pus. The doctor in attendance could not understand why the breathing was so short, and yet on percussion it was resonant all over the affected side (left). The skiagram showed no collection of pus, but the lung was quite collapsed and hugged to the spine, and more opaque than an expanding lung. The lung under these conditions with the tube acting as an educt could not expand. I recommended a silver tube to be inserted in place of the indiarubber one, so that it could be blocked with a cork and released at times to allow the discharge to escape. and yet by its occlusion allow expansion of the lung to go on. The lung expanded under this treatment.

Vomica of the lung, pneumonia, induration of the lung, mediastinal tumours, foreign bodies in the bronchi, &c., are all diagnosable by this means. The heart shows out well, especially if surrounded by healthy lung. Should there be consolidated patches surrounding the heart, they interfere with the outline of the heart in the skiagram. Bouchard claims to have detected aortic insufficiency; he asserts that the ascending aorta is observed to pulsate towards the right of the sternum, and the descending aorta to pulsate behind and to the left of the vertebræ. Aneurism of the aorta shows very clearly, so also enlargement of the heart. Dextrocardia can also be diagnosed by means of the rays.

I have had several cases of foreign bodies in the œsophagus—chiefly coins. One was published by Dr. Wood in the *Intercolonial Medical Journal* (April, 1897), and within the last month I have had two interesting cases. A boy was admitted to the Melbourne Hospital, under Mr. Syme, who was supposed to have swallowed a penny, but had no symptoms. Before taking a skiagram I examined him with the screen, and found the penny in the œsophagus opposite the first dorsal vertebra. Had I placed my plate and lamp to take the stomach, I, in all probability, would have missed it. I exposed the plate for fifteen minutes, and obtained a good negative showing the penny over the first dorsal vertebra as was seen by the screen. In the second case a boy swallowed a large pearl button over 1 inch in diameter (*i.e.*, larger than a halfpenny). When I took the skiagram, seven days had elapsed since he swallowed it. As in the other case, there were doubts about its location. The patient being able to swallow solids, naturally the button was thought to be in the stomach. Previous to taking a skiagram, I examined him with the screen. In viewing it sideways, I was able to see it more plainly, as pearl is not so opaque as metal. In this position I got its greatest density by viewing it through its larger diameter, which showed out like a dark line. In bringing the screen more anteriorly, I foreshortened it, and was able to see that it was something circular. This case had the same exposure as the former—namely, about fifteen minutes. Pearl being less opaque than metal, the button was not so distinct as the penny in the skiagram.

Foreign Bodies in the Larynx.—Recently a child, aged four, was sent to me suffering from dyspnœa. The attending physician telephoned informing me that there was a suspicion of the boy having swallowed something supposed to be a dress-hook. He also said that there might be some diphtheria, but he failed to see any membrane. At first sight one would say the boy was suffering from tracheal diphtheria, with great dyspnœa and marked recession of the walls of the abdomen on inspiration. I submitted the little fellow to an examination by means of the screen, and at once saw the foreign body; and, at every attempt to swallow, it was seen to move upwards. From this, together with its anterior position (the screen examination being from the side) and the other existing symptoms, I diagnosed a foreign body in the trachea—a hook about ½-inch long. Being armed with this assurance, I proceeded to take a skiagram; but, owing to the child's restlessness and dyspnœa, the exposure only lasted three minutes. I gave up hopes of obtaining a record of this case without the aid of an anæsthetic. I developed the negative with great care, and forced it to its utmost, and, to my delight, obtained a fairly good result, showing the foreign body in the trachea nearly opposite the cricoid cartilage.

Foreign Body in Bronchus.—A boy, aged fifteen, having swallowed a pencil, was admitted to the Melbourne Hospital, under Sir Thomas Fitzgerald, suffering from dyspnœa. I took a skiagram on 13th September, and found a foreign body in the left bronchus, measuring about $\frac{3}{4}$-inch long by about $\frac{1}{4}$-inch broad. I informed Sir Thomas Fitzgerald what I had found. He operated the following day, 14th September, and removed the metal pencil cover which I here produce.

Therapeutic Results.—Gocht, of Hamburg, has published several cases treated successfully with the ray, *e.g.*, carcinoma of the breast, trigeminal neuralgia, pilosis pigmentosus and lupus. In the latter case prolonged application of the rays under very high tension set up intense dermatitis, disintegrating the lupoid tissue and healing the ulcerated parts. In all the cases that I have had under my notice, I must say that I have never had a case of dermatitis or depilation. No doubt, idiosyncrasy plays a prominent part in its production. If dermatitis, ostitis, and periostitis are set up by streams of matter electrically charged, why not introduce medicinal agents, mercurials for instance, into an affected part and treat it locally by means of the rays? Used in this way, the rays may have a curative effect. I tried an experiment to see if the rays would take with them chemicals into the deeper parts. I proceeded as follows:—Iodine was the solution selected; a starch mucilage was my test. I placed a solution of iodine in a wooden matchbox previously coated with varnish to make it water-tight, and placed this between my lamp and my starch solution. At first I got a negative result, but subsequently obtained a faint trace of the blue iodine of starch. I am not quite satisfied with this result, as it may have been due to the vapour of the iodine affecting the starch.

Experiments with Colour and Anatomical Skiagraphy.—After some observations I had made with colours, which were published in the *Intercolonial Medical Journal* (August, 1896), it occurred to me that it was possible, for anatomical purposes, to show the arteries by means of the X rays, and on consideration I adopted the following plan of procedure, the object being to render the arteries more opaque to the rays than the other tissues:—The best method of doing so appeared to be that of injecting them with some metallic substance. Lead being the metal most opaque to the rays, and my previous experiments with colour having proved that red was most opaque, a combination of the two—viz., red and lead—seemed most suitable. The result is certainly very satisfactory, for, the lead being more opaque than bone, the vessels passing into or behind the bones are very distinctly visible. The first of these series was the arteries of a seven months' fœtus. The skiagram showed a high bifurcation of the brachial artery, curiously enough symmetrical. It also showed the difference in size of the two iliac arteries and also centres of ossification.

The *Intercolonial Medical Journal* (January, 1897) contains two skiagraphs—one of the knee and one also of the ankle-joint—taken in the same manner. The December number, 1897, shows the brain and intracranial vessels. In one case I succeeded in obtaining a skiagram of the popliteal artery in the living subject, the red blood probably being opaque to the rays. In the same way red muscle can be seen, and in one case a rupture of a muscle was thus diagnosed.

I published some time ago a skiagram of an Egyptian mummy, in which I pointed out an extra sesamoid bone in the ungual phalanx of the thumb which Professor Allen and Mr. Syme informed me was previously unknown to them. I put forth the suggestion that possibly this was due to the kneading of the clay in the manufacture of pottery in ancient days. I may say that the supposed age of this hand is 2,500 years. A case came under my notice where there was a large swelling at the base of the first phalanx of the first finger. This I skiagraphed, and found to be an enlarged sesamoid bone—these are not always present, and the same remark applies to the one at the base of the first phalanx of the little finger. From the skiagram I diagnosed an enlarged sesamoid bone, which was operated on and removed. This was caused by a tennis racket, the patient being an enthusiastic tennis-player.

I trust that my experience with the Röntgen rays, here very briefly outlined, may help to demonstrate the possibilities of this invaluable discovery. Though what is seen may be "through a glass darkly," it is generally accepted that "seeing is believing," and a direct observation with the eye is apt to be more convincing to the average man than the most logical deductions from symptoms and signs, however carefully obtained.

There can be no doubt that the prompt application of the rays may be in a large number of cases the means, not only of preventing much suffering, but of carrying out the highest principles of surgical science—namely, the preservation of human life.

[This paper was profusely illustrated by excellent lantern slides, reproduced from negatives by the author, clearly demonstrating most of the cases quoted above.—Ed.]

THE ÆTIOLOGY AND TREATMENT OF INGUINAL HERNIA IN THE YOUNG.

By R. Hamilton Russell, F.R.C.S.
Surgeon to the Melbourne Hospital for Sick Children.

In passing under review the multitude of operative procedures that have been from time to time devised for the radical cure of inguinal hernia, it is impossible to escape the reflection that there must surely be a certain analogy between our position as surgeons in respect of this matter and that of the members of our sister science of medicine under somewhat like conditions. In medicine it is notorious that where a large number of drugs share the reputation for being curative or beneficial in the case of a particular ailment this fact may be unfailingly interpreted as indicating that no one of them is entirely satisfactory, and that our friends, the physicians, have yet to do a little more "finding out" before they will have the disorder in question under complete control, and be able to erase 95 per cent. of the drugs from their list. During the last three and a-half years I have operated for the cure of inguinal hernia in young children a large number of times, and, without going into unnecessary detail of a purely personal character, I must be content with saying that, as a result of this surgical experience, my views on the subject of hernia have undergone very considerable modification; and my mental attitude, and I may add my surgical attitude also, have witnessed change of a very profound and far-reaching nature. This change has been by no means confined to the inguinal hernia of childhood; its consideration and close study have, on the contrary, brought in their train a clearing-up of many of the problems that are propounded by the study of hernia in older subjects. And I would state my conviction that a just appreciation of the cause and nature of hernia in the child will go far towards clearing away the cloud of mystery and doubt that overhangs the subject of hernia in the adult. I think

I shall probably render myself clearer if I begin by stating the propositions of the truth of which I shall endeavour to convince you. Inguinal hernia is classified as—

(1) *Funicular*, due to failure of obliteration of the upper portion of the processus vaginalis; and I see no reason for separating from this class that which is most unfortunately termed *congenital* hernia, in which the processus vaginalis is totally patent. These two forms, funicular and congenital, I shall, with your permission, henceforth designate respectively "partial" and "total funicular hernia."

(2) *Infantile*, due to the presence of a non-funicular peritoneal pouch.

(3) *Acquired* inguinal hernia, which may be either oblique or direct.

There are certain subdivisions of one or other of the above forms with which we need not now trouble ourselves. A moment's consideration will show that, while the first two forms are mechanically in the same class, being dependent on the presence of a pre-formed sac, the third, the acquired form, stands in a different category, and is an entirely distinct affection, depending on a different mechanical process. To put it shortly, in the first two forms, the sac is the primary factor, and it is the presence of the sac that causes the hernia; in the third, the acquired form, the hernia is the primary factor, and the sac is formed by it. My main purpose, in this paper, will be to prove that acquired oblique inguinal hernia in the young subject has probably no existence in fact, and that it is a pure myth. I shall further endeavour to show—(1) Why oblique inguinal hernia is never acquired; (2) how it has come about that the mistake of supposing that it may be acquired has crept in; and I shall further try to make it clear (3) that the question as to whether oblique inguinal hernia may or may not be acquired is not one of mere pathological interest, but that, on the contrary, the belief in the acquired nature of oblique inguinal hernia is an error that has rendered insecure the foundations of our knowledge, and confounded our surgical practice.

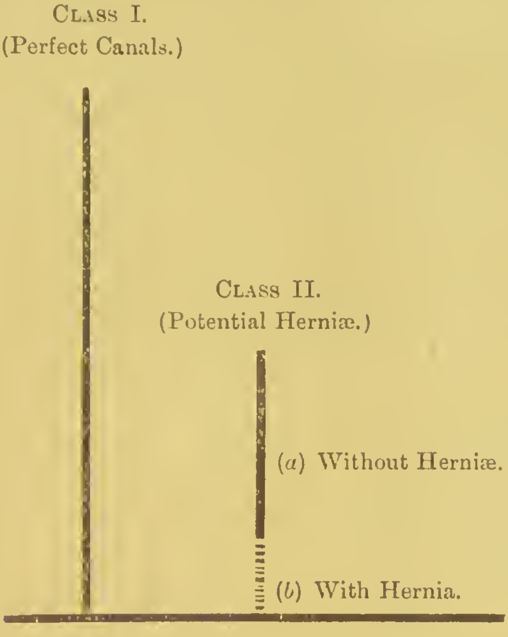
In order to explain the reason why oblique inguinal hernia is never acquired, I must ask you to glance for a moment at the anatomy of the inguinal canal. Without going into superfluous detail, I would point out what appears to me to be the most striking feature of its construction. It is composed of two essentially different portions—First, the floor, rigid and immovable, formed by Poupart's ligament; and secondly, the arched lower fibres of the internal oblique and transversalis muscles, which complete the boundaries of the opening. Thus the inguinal-canal has this curious peculiarity: That it is formed by a rigid tendinous half and an actively contracting muscular half, and this anatomical arrangement in itself serves to invest the inguinal canal with an interest of its own; it is, so far as I am able to recall, anatomically unique—it certainly holds a unique position among the various seats of hernia, and must, therefore, be placed in a special category, and be accorded a study and examination proper to itself alone. Now, when curved muscular fibres contract, they straighten, and the effect of the straightening of these curved fibres is to cause them to descend towards a position parallel to the floor of the canal, and so close the opening. In other words, the action of the curved fibres is sphincteric, and the inguinal canal is a half-sphincter, and is closed, so far as the structures passing through it permit, by this sphincter action. I need not, of course, remind you that the region of the internal ring is covered in and further supported by a portion of the internal oblique as it springs from Poupart's ligament, that the conjoined tendon fortifies the inner portion of the canal, and that the whole region receives support from its covering of external oblique aponeurosis; but the enormously predominating factor in the prevention of hernia is the sphincter-like action of the arched fibres which limit the canal above, as they pass to their insertion into the conjoined tendon. This consideration has a most important bearing on our views when we come to investigate the conditions that underlie the occurrence of inguinal hernia. The immediate cause of the production of hernia is, in all cases, forcible compression of the abdominal contents, as in straining, and, in the absence of such straining, there is no chance of the occurrence of such hernia, for there is ample room in the quiescent abdomen for all the abdominal viscera, and a *vis a tergo* is required before any viscus can be forced out. Furthermore, in the normal condition of the inguinal canal, there is no real opening except such as is occupied and accurately filled by the spermatic cord; for the rest, the canal is a potential opening only, and does not become open until something is forced into it to make it so. Now, the compressing force which tends to the production of hernia we may for practical purposes regard as being invariably the contraction of the muscular system by which the abdominal contents are enclosed; but the contraction of the abdominal muscles involves also the contraction of the curved fibres which we have seen constitute what I have ventured to call the sphincter of the inguinal orifice. So that the effort that is made by the abdominal muscles to force a piece of bowel into the inguinal canal defeats itself, so to speak as far as the canal is concerned, by closing the opening of the canal with a firmness that is in direct and exact proportion to the effort expended in trying to force it; or to put it in a way that savours rather less of hostility, the force which is exerted on the abdominal contents is exerted simultaneously in exactly corresponding degree in protecting the orifice of the canal.

It is of importance to study the capabilities and functions as a sphincter of these fibres. It has been objected that their power as a sphincter must be exceedingly feeble—so feeble that their influence in preventing hernia cannot really be very great. This is true in one sense only. If we contrast the inguinal sphincter with the typical sphincter muscle of the body, the sphincter ani, it will be at once evident that the mechanism upon which their value depends is entirely different in the two cases. The duty of the sphincter ani is, by the strength of its grasp, to close a tube lined with mucous membrane to the passage even of fluids and gases when they are forced along it by the muscular action of the bowel, aided by gravity. This formidable task can only be accomplished by the possession of great muscular power, such as is not demanded, under normal conditions, of the inguinal sphincter. The office of the latter is altogether different in nature; its function is to keep the portion of the abdominal wall immediately contiguous to the spermatic cord in close relation with that structure, and by so doing to maintain such an anatomical relationship that it shall be only the abdominal aspect of the curved fibres, and never their concave margins, that shall be burdened with the task of opposing hernial protrusion. This moderate task is one which is, I am convinced, never beyond the capacity of the inguinal sphincter, and it is only when a demand is imposed upon it which it is quite unfitted to meet, when it is called upon to act as the sphincter ani acts, and to close a serous tube leading from the abdominal peritoneum into the scrotum by the strength of its grasp, that it fails, and permits the descent of a portion of bowel. This statement is equivalent to an assertion that oblique inguinal hernia occurs in the young subject as the result of a developmental defect, which takes the form of a congenital sac, and that it occurs under no other conditions.

Let us turn now to the anatomical structures concerned in femoral hernia; here, every one of the characteristics of the inguinal canal finds its direct antithesis. The femoral ring is of considerable size, large enough to admit the finger-tip, and bounded by rigid structures, devoid of any protecting musculature; on the other hand, any abdominal straining has a tendency, if anything, to open the femoral ring the wider, for Poupart's ligament, the only one of its boundaries that can possibly be capable of any movement, will tend to be pulled upwards by the muscles attached to it. If, on the cadaver, we examine the inguinal and femoral regions from within the abdomen, we cannot fail to be struck by the contrast; no spot in the abdomen so ill-guarded and so favourable to the occurrence of hernia as the femoral ring; no region better fortified and more innocent of any inducement to the advent of hernial protrusion than the seat of inguinal hernia when normally formed. And yet the inguinal canal is the chosen seat of hernia in childhood. Of sixty cases of hernia in children that have passed through my hands, only two have been femoral. When femoral hernia does appear, its mode of occurrence is just what we should be led to expect; a very gradual process of enlargement as the peritoneum is stretched in front of it, which, when compared with the sudden descent of inguinal hernia, presents a contrast as striking as that afforded by the anatomical study of the two regions.

It is of the first importance to realise the true relationship, or rather the singular absence of relationship, between inguinal and femoral hernia. The close contiguity and the superficial resemblance of the two affections have led to their being regarded as twin brethren, whereas they are in reality radically opposed to one another, both in origin and nature; and the non-recognition of the full significance of this fact has been highly disadvantageous to surgery. Femoral hernia is the type of the true acquired hernia. The anatomical structures concerned are eminently favourable to it; the clinical manifestations are consistent with it, and they are consistent with nothing else. In the case of inguinal hernia, on the other hand, the anatomical structures concerned would appear to render acquired hernia extremely improbable; the clinical manifestations are quite inconsistent with it, and all the evidence appears to me to point in a manner that is quite conclusive to the presence of a congenital peritoneal sac as the essential factor in the production of oblique inguinal hernia. To put it tersely, in femoral hernia, it is the presence of a sac that causes the hernia. Now, I am well aware that there are occasional exceptions to this rule. Thus, I have long suspected that the femoral hernia of young children must surely be the product of a congenital sac in the femoral canal, and I am confirmed in this view by the eminent authority of Lockwood, who, I find, holds a similar belief. On the other hand, inguinal hernia is without doubt sometimes acquired in young adult life, but with the utmost rarity, if ever, in childhood. When inguinal hernia is of the acquired type, it is, in my belief, always direct, and I can find no reason for regarding any case of oblique inguinal hernia during, at any rate, the earlier decades of life as unconnected with the presence of a congenital sac.

Now, it is obvious that if this statement, with reference to inguinal hernia, is to be demonstrated as true, the first question that must be answered will be, "Can it be shown that the presence of a congenital peritoneal sac is of such frequency as to account for all the inguinal herniæ?" And this question may be at once answered in the affirmative. It is only necessary to make a careful inspection of the inguinal region from within the abdomen in a few bodies to become satisfied that more or less patency of the processus vaginalis is exceedingly common. Where this occurs, there is a peritoneal tube down which a probe may be passed through the inguinal canal, and along the cord, for a variable distance. So potent is the presence of this unobliterated peritoneal tube, so completely nugatory does it appear to render the agencies whose office it is to prevent hernial protrusion, that *a priori* one would suppose that the existence of this developmental defect would almost necessarily be associated with inguinal hernia. Yet, surprising though it may be, and as I think it is, such is far from being the case, and it is certain that "hernia enters the passage only in the minority of cases in which it remains open" (Macready). In other words, the number of those born with some patency of the processus vaginalis is amply sufficient to fill the ranks of ruptured children, and leave a considerable surplus of patent processes without any hernia at all. It is of such importance that the significance of this fact should be adequately grasped, that I have ventured to make a simple diagram illustrating it. The tall column represents the normally formed inguinal canals (Class I.); the short column the much smaller number, with open funicular processes, and of this column a fractional portion has been marked off to represent the minority in whom hernia has occurred (Class II. *a* and *b*).



Now, it seems to me an irresistibly logical position to take up, that if hernia occurs only in a minority of the open funicular processes, we should refuse to believe that hernia ever is present in the normally formed canals, in the absence of some incontrovertible proof. That the strength of this position is not quite adequately recognised by even high authorities is easily shown. Thus Macready observes, with reference to this matter:—"Several authors of eminence have regarded all oblique inguinal herniæ in childhood as occupying some part of the processus vaginalis. If this were so, all the oblique herniæ of children should give evidence of pre-formed sac. But in many cases it is very difficult to obtain assurance of the congenital origin of the sac." I must, with deference, contest this position, and hold that, in view of the inherent probability of the hernial sac in any child being congenital, the *onus probandi* lies with the other side to demonstrate the non-congenital origin of it. Now, not only has nobody succeeded in doing this, but I am able to discover no particle of direct evidence in support of the belief that the sac is ever non-congenital. Assertions there are in plenty, but of evidence none whatever, and I seek in vain for any rational objection to oppose to the view that the sac of an oblique inguinal hernia in, at any rate, a young person, is invariably congenital, and is, in the vast majority of cases, an unobliterated portion of the processus vaginalis.

The foregoing arguments constitute a body of presumptive evidence merely, and are inconclusive, but I should not have ventured to obtrude views of this kind upon you backed merely by arguments that were not entirely conclusive, no matter how plausible and seductive they might be. I am, however, able to adduce evidence in support of the views I have advanced, which, I submit, is adequate to remove them out of the realm of debate, and transfer them to the category of demonstrated fact. In order to do this, I must first offer a sort of axiomatic statement for your acceptance, and it is this—"If, in a case of inguinal hernia, the sac be ligatured and removed, but no other measures be adopted for combating the supposed conditions that have caused the hernia, and the patient be allowed to get up and go about as soon as the skin-wound is healed, the hernia will, if of the acquired type, at once begin to reform, but if it does not recur, this fact is proof that the sac must have been the original cause of the hernia, and that the hernia was not of the acquired variety, but one of those that are dependent upon the presence of a pre-formed sac." I presume you will not hesitate to grant the truth of this self-evident proposition, but, unless you do, I, of course, admit that I can prove nothing. Assuming that you do grant it, however, the crucial test of the truth or error of my views shall rest on the result of operation. If it be true that oblique inguinal hernia cannot occur in the young subject except in the presence of a pre-formed sac, then it must of necessity follow that removal of the sac will completely and permanently cure the rupture. This is truly a very severe test. It might quite fairly be urged that, after a hernia has existed for some time, such alteration may have taken place in the region of the abdominal wall involved that mere removal of the sac cannot be expected to alone prove effective for the cure of the hernia. There is, however, no need for me to urge any such plea, for, surprising as it will no doubt be to you to hear me make the statement, I have abundantly satisfied myself that mere removal of the sac in a child will effectively and permanently cure the rupture without fear of relapse.

During the last three and a-half years, I have operated upon sixty cases of inguinal hernia in children. Since the first one or two cases, in which Bassini's operation was performed, the operation has consisted in every case (with one exception) of removal or obliteration of the sac, after free separation from the cord and the walls of the canal. In a number of the earlier cases, Macewen's method of treating the sac was followed, but in no case were the walls of the canal approximated by sutures, nor was any expedient adopted for the future prevention of hernia other than complete removal of the sac. Out of this relatively large series of cases, in no single instance has there been any return of the hernia, and, more than this, to the best of my belief, there is not to be found in the ranks of my patients a single inguinal region that has been the seat of operation for hernia wherein there is the slightest weakness to be detected, with one exception. This one exception was a case in which, evidently through some fault in the performance of the operation, there was apparent distinct weakness from the first rising of the patient from bed; and it is a matter of extreme significance that, in spite of this circumstance, the weakness has not increased nor progressed towards return of the hernia in the slightest degree, although it is now nearly three years since the operation, and no truss has been worn.

I am convinced that there must be a widespread feeling of bewilderment at these statements, which, only by your courtesy, is forbidden to take the name of incredulity. Surely it is by no means uncommon for recurrence of the hernia to take place after operation for radical cure in young children. Perfectly true, but you will please to notice that I say there is no recurrence after simple removal of the sac; I do not say that there may not be recurrence after one or other of the multitudinous operations for radical cure, and I cannot doubt that, had the anatomical principles that I have put forward in the early part of this paper as underlying the occurrence of inguinal hernia been always fully appreciated, but a small minority of the devices that have been originated for its radical cure would have seen the light of day. It is impossible for me in the time at my disposal to enter upon a critical analysis of the various operations for radical cure, but I would merely point out that these operations often show a lack of recognition of the sacredness from the knife of the constituent layers of the abdominal wall. No more unfortunate error is conceivable than that which has sometimes carried the knife through the curved fibres of the inguinal sphincter, for these fibres, once cut, can never be restored in their integrity, but will be thenceforth marred by the presence of a line of fibrous tissue connecting the cut ends. It is true that prolonged recumbency after the operation may suffice to tide this line of fibrous tissue over the period during which it is liable to be stretched ; but we may, I think, confidently expect in a series of cases operated upon in this way a good percentage of "relapses," and in any case the prolonged confinement to bed is a very great evil. The truth is that the recurrence that takes place after the abdominal wall has been treated in such a disrespectful manner as this, is not a "relapse" at all, but a new hernia altogether; a hernia caused by the injury done to the abdominal wall, a traumatic hernia, precisely the same in origin, nature, and mode of occurrence, as the hernia which, if I mistake not, has been known to supervene on operation for shortening the round ligaments of the uterus. I would further point out that the suturing of the walls of the canal, which forms a part of almost every operator's programme, may be by no means the innocent expedient that it appears. That it is absolutely unnecessary in the case of children I have proved beyond the possibility of question ; in the case of adults with hernia of long standing, the case is rendered different, owing to very substantial alteration and impairment of the abdominal wall, which will have resulted from the presence of the hernia and the use of a truss during a lengthened period of time. Given the perfection of surgical skill and judgment, it is probable that the suturing would be at least harmless, but that it may be a source of evil is certain, notably in the event of the sutures being tied tightly. Now, the fibres included in a tight suture will be ulcerated through, and fibres that are ulcerated through are permanently damaged in precisely the same way as though they had been divided by a knife, so you will at once see that if these important fibres are severed three or four times at short intervals, it would be hardly surprising if, later on, we should be confronted with some evidence of their impairment in the form of hernial weakness.

I must now revert for an instant to my series of operation cases. Leaving out of count the cases that have been operated upon during the last six months, there remain forty-two cases in which periods varying from six months to three and a-half years have elapsed since the operation. Of these, I have just examined or heard of thirty-six, in every one of which the inguinal region has remained free from any tendency to recurrence. Now, by virtue of the axiomatic statement with which I prefaced my allusion to these cases, I hold it demonstrated that every one of these cases was due to the presence of a pre-formed sac; and if thirty-six out of thirty-six are proved to be of that nature, then we may surely conclude that acquired inguinal hernia, if it ever occurs at all in childhood, must be so excessively rare that its consideration may be henceforth disregarded.

The two following important propositions, then, I submit as demonstrably true—

I. Inguinal hernia in childhood is always due to the presence of a congenital sac.

II. Removal of the sac in early life completely cures the hernia.

I cannot refrain from narrating here one case, which is a fine illustration at once of the truth of this view, and of the practical value of its recognition. A few minutes back I was telling you that all my cases, after the first one or two, had been treated by simple removal of the sac, after ligature, with one exception, and the case I refer to is the exception.

A boy, æt. three, was operated on in September, 1898, for a large right inguinal hernia. The case is the only one I have as yet seen in which the sac was clearly not formed by an unobliterated portion of the processus vaginalis. It was characterised by its remarkable thinness and membranous appearance, and by the looseness of its connections. It was in no way closely connected with the cord, but I am unable to say what was the precise positional relationship between the cord and it. On opening the sac, which tore very readily, its contents were found to be several inches of small intestine, which had passed through an aperture about the size of a threepenny piece. The intestine had to be pushed back into the abdominal cavity with a probe, and by the time this was accomplished the flimsy sac had been partly torn to shreds, and partly, I suspect, pushed up into the abdomen with the gut, so that, when the time came to ligature and remove the sac, there was no sac to be found. I must confess I was a little perturbed, for there was the inguinal opening, with the naked intestines plainly visible lying at the bottom of it, and no way of closing the peritoneum over them. However, I reasoned thus: "If there is no sac for me to ligature, then there will be no sac for a hernia to enter, and all the muscular forces that act upon that circular opening make for its rapid closure; and, seeing that nothing but the presence of a sac will ever again induce it to open to permit the descent of a rupture, the absence of the sac, in whatever way it has been caused to disappear, should ensure the permanency of the cure, if my belief be founded upon the truth." I accordingly contented myself with merely cutting away the coverings, and closing the wound in the usual way, putting on an extra pad of wool over the part, and keeping the patient recumbent for a full fortnight. The operation was performed twelve months ago, and the result has been perfect, for there has never been the slightest return of the hernia, nor any suspicion of weakness about the affected region. The result has been, in fact, identical in character with all the other cases in which the sac was ligatured.

Let me now draw your attention to the title of this paper, in which the expression is used—"Inguinal Hernia of the Young." I have chosen the vague and indefinite expression—"the young," with a very definite object, for, while the views I have been putting forward have been formed mainly on the study of inguinal hernia in childhood, their establishment as a conviction in my mind has been associated with a great extension of the range of their applicability in respect of the age of hernial subjects. I can see no reason why the arguments I have adduced in support of these views on the etiology of inguinal hernia in childhood become in any way weakened when they are made to embrace the herniæ of adolescence and of adult life. There does come a period, however, in later life, when the abdominal muscles lose their vigour, and the belly becomes pendulous, and when inguinal hernia is prone to make its appearance for the first time. There are factors introduced here which serve as an effective counterpoise to the theory of the congenital origin of the sac, and although I am by no means confident that congenital defect may not play a predominating part, even in this type of hernia, I do not propose to discuss the matter, but to admit frankly that these herniæ of elderly people may be placed in a different class altogether from that which is signified in the title of my paper, under the elastic and comprehensive term of "Inguinal Hernia of the Young." I would therefore define the scope of the expression "the young," as comprehending all inguinal herniæ (with rare exceptions) throughout the earlier and middle decades of life, up to a certain more or less indefinite barrier which marks the oncoming of old age, beyond which we enter the category of "Inguinal Herniæ of the Old."

Let me here interpolate a few words with reference to the exceptions to the great and important rule that inguinal hernia in the young is the result of a congenital sac (and there is hardly any rule in respect of hernia to which apparent exceptions may not be found). Personally, I cannot believe that an acquired inguinal hernia in a young person ever comes through the internal ring; the relatively ill-guarded region of the external ring seems to be the more favourable situation for acquired hernia in a young person, but Mr. Lockwood, in his great experience, has met with one clear case, and one or two that he considers doubtful. For my own part, I would place these cases of acquired direct inguinal hernia in a distinct class. I only wish to allude to the rare exceptions in order to say that, beyond recognising the bare possibility of their occurrence, and, knowing how to deal with them, our purpose will be best served by leaving them out of our calculation. It would be merely confusing and misleading to allow the rare occurrence of a direct acquired inguinal hernia, which is one thing, to cloud our knowledge and undermine our judgment in respect of inguinal hernia *par excellence*, which is an entirely different thing; and I should not have ventured to bring these views before you at such length, and with so much insistence, were it not for my assurance that they cannot fail to modify our attitude towards inguinal hernia in general, and especially towards hernia in young children. If these views are correct, then I venture to submit that the whole subject of the treatment of hernia in childhood by means of the truss requires very careful reviewing.

The present opinion of surgeons as regards the treatment of inguinal hernia in childhood is characterised by an unanimity rare among those who follow our craft, and it may be expressed thus:—"Operation for hernia should never be undertaken in the case of a young child when the hernia is susceptible of efficient control by a truss, for in a large number of cases treatment by truss for a time will result in cure of the rupture." It is with an almost trembling sense of my own temerity that I venture to dissent from this time-honoured doctrine, and I would ask in justification what the cure by a truss really consists in. Does it mean that the sac becomes obliterated from the internal ring downwards, by cohesion between the opposed surfaces of its walls? Let us at once dismiss this supposition, which is one that is by no means without adherents. Were this possible, one of the most evident and established laws of pathology would be violated, for the walls of the sac being lined by a layer of formed endothelium cannot cohere, any more than could the palms of the hands unite if bound together, unless first denuded of their epithelial covering. It is beside the mark to say that cohesion naturally does take place at one period of our existence, which is, of course, true, but that must {be before the period of the formed endothelial covering. The same argument would hold good in respect of the two halves of the palate, but it would be not more impossible for the two halves of a cleft-palate to unite without paring than for the walls of the peritoneal sac to blend, unless denuded of their lining. To bring practical observation into the question, a French surgeon, Le Dran, took the trouble to examine a number of bodies of hernial subjects that had been cured by truss treatment, and, needless to say, in not one case had obliteration of the sac taken place. What really happens is this: The bowel being prevented for a time from entering the internal ring, the inguinal sphincter gets, as it were, a fresh start, and, contracting round the neck of the sac, renders it under favourable conditions quite small, so that it will only admit a probe, and often a knuckle of bowel will not happen to force an entrance, and so the hernia will be said to be cured. But more than this it is impossible for the inguinal sphincter to do, and as much as this it often, very often, fails to do; and in any case it is obvious that the difference in point of liability to hernia between an inguinal canal, in which there is such a peritoneal tube prepared to assume the rôle of a pre-formed sac, and an inguinal canal in which no such tube is present, is wide as the poles asunder. To illustrate the point by our diagram, the utmost that the truss can accomplish is to translate the patient from the ranks of the hernial subjects (Class II. *b*) into those of the patent funicular processes without hernia, the potential herniæ (Class II. *a*), while it is powerless to place him among those with the perfect inguinal canals (Class I.). The hernia will have been, in fact, not really cured, but only obscured. Now, I am of course ready to admit that, if a hernia were never likely to come down any more after having been treated by a truss, it would not matter to the patient whether the peritoneal sac were present in his inguinal canal or not; but experience has abundantly shown that there is no real security against the return of a hernia after it has been made to disappear by the use of a truss.

I am convinced that, while everybody will agree that the truss treatment is uncertain, and therefore leaves much to be desired, it is in reality very much more uncertain and inefficient than is generally supposed. It is not uncommon for a hernia to appear suddenly, and as it is thought for the first time, during the period of adolescence or the earlier decades of adult life, during the performance of some arduous feat or the lifting of a heavy weight. Now, it must be clearly understood that a young man, in whom an oblique hernia occurs in this way, is of necessity a member of our second class, with a preformed sac (potential hernia). It is, moreover, exceedingly probable, if inquiry is carefully made, that the patient will be found to have been in childhood the subject of hernia that was supposed to have been cured by the use of a truss. Should this be so, it is obvious that this patient is much more likely to be vaunted as an instance of a hernia satisfactorily cured by a truss during many years, rather than ultimately be placed in the opposing balance with those in whom the truss treatment is known to have failed. Take it how you will, it is an exceedingly unsatisfactory thing to be content with transferring a child from Class II. (*b*), the herniæ, to Class II. (*a*), the potential herniæ, instead of doing that which our surgical resources render us easily capable of doing—viz., promoting him to Class I., the perfect inguinal canals, with no predisposition to hernia at all. This can only be done by operative removal of the sac; and unless it can be shown that the perils and drawbacks involved in the operation are such as to turn the scale in favour of the truss, then I hold that the matter can hardly come within the scope of rational argument, and that the treatment of hernia in childhood by means of the truss should be discredited and abandoned.

As to the risks incidental to operation, I regret to have to confess that my series of sixty cases is marred by one fatality. This was the case of a child, a year old, who was admitted to the Children's Hospital with the hernia strangulated, and the cause of death was suppuration of the ligature placed upon the neck of the sac. For this unfortunate occurrence, I take the entire blame upon myself: the calamity was a perfectly avoidable one, and was the result of my being persuaded to employ a ligature material which I had not sufficiently tested beforehand, and which proved to be fatally wanting in the qualities such as a ligature should possess. I have no wish to explain away or in any way minimise the significance of this circumstance, but to simply state, without any sort of extenuation, that out of my series of sixty operations one proved fatal. Speaking broadly, as the result of my experience, I am distinctly of opinion that the operation is not more dangerous to the life of a child a year old than it is to the healthiest young adult, but I am bound to say that in the case of the child the operation is incomparably more difficult, and demands some operative skill and delicacy. There stands, however, the inexorable fact that my operations show a mortality. Taking this as an indication of risk, the question now arises as to whether this remote chance of a fatal result should tell so strongly against operation as to leave the laurels with its competitor, the truss. To this question I answer unhesitatingly, "No." Even were there a certain risk attendant on operation from which the truss is free, the advantages of operation are so enormous as to justify us in incurring some additional risk in order to gain them. But I would point out that, while we may apportion a very small mortality to operations in childhood, it is an error to suppose that the truss is altogether free from blood-guiltiness; for the truss means, in a goodly percentage of cases, hernia uncured, and in every case hernial predisposition, and it will not be denied that hernia is a weapon in the armoury of Death which is occasionally employed for the exaction of his tribute. In plain language, there is a mortality associated with the truss, but a mortality that occurs later on in life; and it is certain that this mortality can hardly be less than that attendant on operations in childhood in practised hands.

This leads me directly to speak of the method of operation followed by myself. The incision is made in the groin, parallel to Poupart's ligament; the cord is sought for, the coverings of the cord successively opened, and the cord, sac, and testicle drawn out of the scrotum. The *vas deferens* and the vessels of the cord are next defined and separated from the sac, and the sac is freed from these structures as high up in the canal as possible. A chromicised catgut ligature is now applied to the neck of the sac in the form of the Staffordshire knot, and the sac removed; the superfluous coverings of the cord are cut away, and the testicle carefully washed and dried, and replaced in the scrotum. The wound in the groin is closed by the subcuticular method of suturing, a single thread of silkworm-gut being used for this purpose, the two ends of which emerge from the skin about half-an-inch from either extremity of the wound. A single layer of gauze and collodion is then applied to the wound, and, when this is sufficiently dry, a pad of dry dressing is bandaged on over all. The child, if a small one, is placed in a double long splint, to which it should have been accustomed for a day or two before the operation. At the end of a week the silkworm-gut thread is withdrawn without disturbing the collodion and gauze, and a day or two later the child may be allowed up to run about. To recapitulate the salient features of this procedure, the operation consists in simple removal of the sac, and nothing else; the walls of the canal are untouched by the knife, and they are not sutured in any way, there is no after-dressing of the wound, and there is no prolonged period of recumbency such as is almost invariably insisted upon by authors. No matter how large the hernia, nor how wide may appear to be the opening of the canal, no variation is made in the operation. And I believe that this method affords a means of absolutely reliable and permanent cure in every case of inguinal hernia in a child, and I look upon it as an operation unsurpassed in surgery, by reason of the perfection in which it fulfils every surgical ideal. I may add that in young children it is quite unnecessary to incise the external oblique aponeurosis, the neck of the sac can easily be pulled down outside the external ring, so that when the stump is allowed to go back it passes up at once well within the curved fibres, and becomes flush with the rest of the parietal peritoneum. In the case of older subjects, however, where the canal is relatively long, I always follow the teaching of Lockwood, who advances good reasons for insisting that the canal should be opened up.

One word in respect of inguinal hernia in female children. The operation differs widely from that in males, in that it is probably the simplest in surgery. It can be easily done in five minutes, and is a procedure of less magnitude than the suture of a single hare-lip. The application of a truss to an inguinal hernia in a girl, I can only characterise as absurd.

With respect to the age for operation, I would not by choice operate before twelve months—that is to say, till after weaning has been accomplished. Nevertheless, my facts include one case in which the operation was quite satisfactorily carried out in an infant of nine weeks, in whom strangulation was present.

In conclusion, let me in a few words make my position clear in reference to the operations at present in vogue for the radical cure of hernia, and, for convenience, I will instance Bassini's operation as probably the most popular. The subject requires a little minute dissection; strictly speaking, we are not fully accurate when we entitle Bassini's operation one for the "radical cure of hernia." Let me for once give its full title, which would be this—"Operation for the radical cure of hernia, to which is superadded Bassini's device for repairing the injury sustained by the abdominal wall through the prolonged existence of a hernia, and the use of a truss." Thus, the operation is a compound one, having a twofold object; and of the two objects that it is designed to compass, the first is to correct an error of Nature, and the second to correct an error of Art—

our Art. None of these methods of operations are called for, except in the presence of the gaping opening that is seen in old-standing hernia. In recent oblique inguinal hernia in the adult, Bassini's operation or any kindred device is, in my opinion, quite superfluous, while it is assuredly quite unnecessary for any case submitted to operation during childhood. While I recognise that there must always be a certain limited sphere of usefulness for the truss, I hold that it should no longer be tolerated as a means of treating inguinal hernia in the young, under the mistaken belief that it can ever be really curative. In order to act rightly, we must first see and think aright, and, in my belief, the keystone of the edifice of sound knowledge in respect of inguinal hernia is the recognition of the fact that oblique inguinal hernia has nothing in common with any other form of hernia, but is a distinct affection due solely to a congenital defect which furnishes a pre-formed sac; is to be remedied by operative removal of the sac; and is not truly remediable by any other means at present within the scope of our knowledge.

THE OPERATIVE TREATMENT OF INGUINAL HERNIA.

By H. V. Critchley Hinder, M.B. Syd.

As the technique of aseptic surgery becomes more and more perfect, and the chances of death or a prolonged condition of incapacity from a suppurating wound become less, the patient loses some of his dread of operative treatment, and clamours more frequently to be relieved from the irksome onus of a truss.

The question that immediately arises is, With what degree of certainty can absolute cure be guaranteed? A satisfactory reply to this query seems hard to give, but, judging from the number of operations described by various surgeons, is one which vexes the minds of many operators, and at the same time appears to still tax their ingenuity to its utmost.

There is no doubt but that a great deal of this difficulty in laying down a precise line of treatment lies in the fact that almost all herniæ vary in their anatomical appearance when they come under the operator's care, and also that operations for the relief of hernia are too often planned out on the normal subject rather than on the subject afflicted with hernia. Though I would be averse to involve you in an anatomical disquisition, yet it is hardly possible for me to proceed without briefly drawing your attention in the first place to a few details involved in the structure of the normal canal. I will then proceed to those divergent conditions arising in the majority of cases of oblique inguinal hernia; and, finally, to the method of treatment to which I would wish to draw your attention.

Without entering into too much detail, we must accept the fact that there is a diminished resistance offered to intra-abdominal pressure at the internal ring—that is to say, at the point of entrance of the spermatic cord into the inguinal canal; and this because the abdominal wall at this point is practically made up of but one layer of muscle—namely, the tendinous portion of the external oblique. The fascia transversalis is densest at this situation, and is, no doubt, also of some assistance in maintaining the integrity of the abdominal wall. The beginning of a hernia, therefore, is at this spot, and consequently there must be a giving way of the fascia transversalis or of the tendon of the external oblique.

The inguinal canal is but a potential canal, formed, where it is completely formed with opposite anterior and posterior walls, by the want of complete approximation of the tendons of the internal oblique and transversalis constituting the conjoined tendon posteriorly, the external oblique anteriorly, and the floor by the curving round of Poupart's ligament as it becomes inserted into the ilio-pectineal line as Gimbernat's ligament inferiorly.

There exists then an obliquely placed wedge-shaped canal with an opening at the inner side of the wedge more patent than that at the outer side, and the anterior and posterior walls of the wedge approximated less closely externally than internally.

It is evident, then, that in the earliest stage of the formation of the hernial sac, when the fascia transversalis gives way at the internal ring—whether due to a distinct congenital funiculus or a mere digital fossa, or to a giving way of its basis of support, the tendon of the external oblique—its further advance takes place along tho line of least resistance, which a glance will show to be along the canal.

The outer edge of the conjoined tendon is as a rule by no means sharply defined, and is usually composed of fibres of the transversalis muscle alone.

As a consequence of the pressure of the advancing hernia, the tendon partly becomes forced on one side and partly atrophies, and this helps to bring the internal ring opposite the external. At this time, too, the hernial protrusion engages the external ring so that in very early cases of hernia, the exploring finger introduced into the external ring discovers a certain slackness of the pillars of the ring, together with a looseness of the intercolumnar fibres which, to a certain extent, brace the pillars of the ring together. Then, as the hernial sac protrudes from the external ring, the internal pillar becomes most influenced by the pressure, but usually only to an extent sufficient to affect Poupart's ligament and the fibres of that part of the external pillar as it rises obliquely from Poupart's ligament.

As the hernia increases in size and remains down longer and longer, the canal gradually disappears as a canal owing to the disappearance mainly of its posterior wall, and later also of its anterior wall. The conjoined tendon is now represented by a few fibres loosely attached to the ilio-pectineal line and part of the crest of the pubes, but its main firm attachment is now confined to the crest of the pubes, sometimes only as far out as the internal pillar of the ring.

In every case of oblique inguinal hernia, the condition of the inguinal canal varies somewhat between the condition observed when a hernia first protrudes from the external ring and that extreme patency found usually in old cases where the canal is practically obliterated.

In these hernia of long standing and of a fair size there are usually two types: In the one in which the individual's abdominal muscles are fairly, or very well, developed, and his abdomen firm though possibly somewhat protuberant, the main feature is the absence of flaccidity; in the other class the abdominal wall is lax and protuberant in the lower half, particularly laterally towards Poupart's ligament. There is a feebleness of contraction, and resistance on pressure is slight. When the patient voluntarily puts his muscles in action there is none of that hardness and firmness that exists in the healthy abdominal wall. The muscles, too, on section are pale and thin, and the fasiculi loosely attached to one another, and both fatty infiltration and degeneration are present.

The external ring bulges forward from the abdominal wall more than is usually the case, and presents a funnel-like appearance, and very frequently the disappearance of the conjoined tendon is not so marked, probably on account of the slackness of the external oblique offering less resistance anteriorly.

Having now briefly discussed the condition of affairs which exists during the progress of the formation of inguinal hernia, the principles of operative treatment become somewhat more easy to grasp.

It appears to me to be obvious that no amount of padding in the shape of twisted-up thickened sac is likely to be of the slightest service in preventing recurrence of hernia, because, though the sac be one-eighth of an inch thick when opened, this is only the case by virtue of its position and the function it has to discharge, and also probably in consequence of inflammatory attacks of the tissues forming the sac.

So soon as its position is altered so soon will absorption of this thickening take place, and the mass will shortly be represented by a shadow of its former self. And, again, pressure from within directed against this twisted or buckled up mass, unless well buttressed by firmer tissues outside, will soon force it clear, for it is really only held, practically suspended, by the peritoneum and transversalis fascia, which are both capable of considerable extension. Provided the stump of the sac be not placed opposite the weakest point of the abdominal wall, so as to act as a wedge, it matters little what becomes of it. The main feature is undoubtedly the closure of the inguinal canal.

Under normal conditions there is no doubt but that the contraction of the fibres of the transversalis and internal oblique leaves but little space between their lower border and Poupart's ligament, and even in early cases of hernia it is somewhat difficult after laying bare the external ring to force the finger into the internal ring. In cases of failure of these factors, it is obvious then that the restoration of the normal principles of action is of paramount importance.

This has been attempted in various ways, but I would strenuously oppose any method that interfered with the integrity of the external oblique.

This has been freely split up the whole length of the canal and afterwards sutured, or a semi-circular incision made so as to dissect back the flap containing the pillars of the ring, and laying bare the internal oblique and transversalis and internal ring. The flap is later on replaced and stitched in position. The objects in view are that the contents of the canal may be better dealt with, the cord disposed of in a way to please the operator, and the conjoined tendon or, at any rate, the transversalis and internal oblique muscles attached to Poupart's ligament.

Lockwood states that if the tendon of the external oblique be incised in the direction of its fibres, on contraction taking place, the fibres become approximated so that the damage done is but necessarily slight. That this is not always the case I have myself observed, for, on division of the intercolumnar fibres and separation of the fibres of the external oblique gaping at once occurs, and certainly increases on rest, and they will not always approximate if the patient strain, so that the tendency on the whole is for the gap to increase.

Anyone who operates on the abdomen knows how likely an incision through tendinous structure only, is to be followed by hernia, and for that reason incisions are, as far as possible, made through muscle, and by merely separating their fibres when possible. It is obvious then, as our experience goes to show us, that a division of a muscle or tendon is replaced by a union of fibrous tissue, which is sure to stretch and to be a feebler tissue than that which existed before.

On the other hand, if the sac be opened, I see no reason why the operator should not pass his finger right up the canal inside the sac, and so make sure that there are no adhesions of omentum, &c., or any other condition likely to interfere with the proper occlusion of the canal. Though it cannot but be admitted that the opening up of the canal makes its precise condition more absolutely certain than a mere digital examination, yet abnormal conditions of the contents of the sac or canal are not sufficiently common to justify interference to the extent of, in every case, weakening one of the main factors in the preservation of the integrity of the abdominal wall.

The method of operating I usually adopt is as follows:—The suprapubic, scrotal, and perineal regions are shaved. A green soap poultice is applied for at least one half-hour. This is scrubbed away with a soft nail brush and warm water. Next the skin is scrubbed with a sterilised soft nail brush and permanganate of potash till the skin is a very dark brown. The skin is then discoloured with a solution of oxalic acid in sterile water. The whole area is covered with a sterilised towel, bandaged on and left for the short time which intervenes between the cleansing and the operation.

The skin incision is made parallel to Poupart's ligament, and just above it, so that the lower end of the incision does not encroach on the scrotal tissues, but ends just over the external ring.

The structures are cleanly divided as far as the aponeurosis of the external oblique, and superficial vessels are ligatured. Then, with the handle of the knife sweeping downwards, the subcutaneous tissues are swept aside from the aponeurosis, and the pillars of the ring are laid bare.

The sac is then sought for, the operator holding a dissecting forceps in each hand. The sac is recognised by its opaque, white colour, and is nearly always in front immediately beneath the cremasteric and transversalis fascia, though rarely it may be behind or to one side—usually the inner. Having found the sac, take it up, incise it between two forceps, slip the forefinger into it, and separate the neck of the sac.

If the sac be very dense and thick it may be removed, but under any circumstances it is as well not to rend and tear the scrotal contents in freeing them from their attachment to the sac.

Hold well the edge of the neck of the sac, and cut it off as far down as you intend to separate it. Next, with the left forefinger, explore the inside of the neck of the sac, and in advanced cases you may be able to look right into it. At all events, deal with the contents whatever they may be, whether omental or intestinal adhesions, or fatty tumours, &c.; and in dealing with omentum, be careful not to mistake mesentery for omentum, which may rarely accompany the sigmoid flexure, and in a landslip fashion present itself in the hernial sac, and remain attached within the canal while the small intestine slips back into the peritoneal cavity, leaving the loop of attached mesentery in the canal to be, by mistake, ligatured off and returned. The result is naturally disastrous, for a portion of the intestine becomes gangrenous in consequence of its blood supply being cut off.

When the canal has been cleared, take the mouth of the sac in a pair of forceps and twist it round and round when the patient is breathing quietly and not straining, lest omentum be forced into the funnel-like inner end. In early cases the twisting naturally leaves a cord-like mass right up to the internal ring. Draw on this twisted sac somewhat, and ligature it off as high as possible with stout catgut or kangaroo tendon, or, if the sac be too thick, transfix and ligature, cut off the superfluous sac, but leave the ends of the ligature long.

Next pass the left forefinger well up the canal till the tip reaches the arching fibres of the internal oblique and transversalis. Separate the transversalis fascia from the internal surface of the transversalis muscle, and pass a stout, slightly curved, handled needle from without through the external oblique, internal oblique, and transversalis on to the finger tip, following the finger down as it is withdrawn from the canal. Thread the needle with one of the ends of the ligature attached to the sac and withdraw it. Repeat this manœuvre, so that, on drawing upon the two ends which protrude from the external oblique, the stump of the sac is drawn well up behind the transversalis muscle. If the hernia is one of long standing, so that the external and internal rings are almost opposite one another, then it will be found that the sac may not be twisted up to such a marked extent, but needs freeing behind the pubes and laterally, so that the stump may be drawn up out of the way behind the transversalis muscle.

Having disposed of the sac, it is now necessary to turn one's attention to the closure of the inguinal canal.

In order to do this the main object is to approximate the transversalis and internal oblique, whether in the muscular or tendinous part, to Poupart's ligament. In connection with this closure, I am firmly of opinion that an altered disposition of the cord can be of very little avail.

In the first place, do what you will in the way of closing the canal or approximating the transversalis and internal oblique to Poupart's ligament, there is sure to be a narrow, somewhat triangular aperture just inside the spine of the pubes. This being present of necessity, let the cord receive its exit at this spot, at the same time keeping it posterior to the transversalis and internal oblique as far as possible, a space which will vary in extent according to the presence or absence of the insertion of the conjoined tendon into the iliopectineal line, an attachment which, as I have explained, does not exist in advanced cases.

In the second place, making the exit of the cord right opposite the opening of the internal inguinal opening, whether it be brought through the external oblique or no, only alters the situation of the point of least resistance, or rather makes two instead of the one that cannot be avoided—namely, that just internal to the spine of the pubes.

Pass the left forefinger then in front of the cord up the canal till the origin of the arching fibres of the internal oblique and transversalis is felt as they spring from Poupart's ligament.

Pass a curved handled needle through Poupart's ligament on to the top of the finger which acts as a guide, and then straight across beneath the transversalis and internal oblique and out again through the external oblique. Thread the needle with stout catgut or kangaroo tendon and withdraw it. Repeat this every half-inch all the way down, keeping always in front of the cord. If the internal ring be large and practically quite opposite the external ring, then the rectus as well as the conjoined tendon may need to be taken up by the needle, and on the lower side that part of Cooper's ligament which is attached to the crest and posterior surface of the pubes may be made use of.

After all the sutures are in place, begin to tie off from the outer end, and it is surprising how, as one proceeds, the opening, which at first looked hopeless, becomes very neatly closed.

If the operation be conducted aseptically, the wound may be dried and sewn up with horsehair. No drainage tube is necessary. Apply sterilised gauze and wool dressing, and bandage very firmly and tightly with a spica bandage.

In young children I leave this for three days and dress the wound, which is now dry, with collodion.

This is advisable because a nurse may watch the child well for the first two or three days, but as it becomes livelier the chances are that it will soil the dressing and contaminate the wound.

In adults the wound may be left for eight or ten days and the stitches removed in a fortnight. The patient leaves his bed in three weeks, retains the spica for five or six weeks longer, and avoids marked exertion for four months.

Lockwood, in particular, does not approve of operative treatment in the case of long-standing hernia, and particularly where the abdominal walls are lax and pendulous. If the patient is exceedingly stout and has a heavy omentum, the chances of a permanent cure are certainly not great, yet at the same time old folk, or at all events those getting on in years, very frequently do not take kindly to trusses, but are as ready to submit to operative treatment as younger patients. Under these circumstances, then, the choice should be left with them. For my own part I have by no means found the ultimate result an unsatisfactory one, for, though the flabbiness and feebleness of the abdominal muscles may be looked upon as contra-indicating operative treatment, yet when we come to consider how much quieter lives old folk lead, and how much more free they are from any exertion likely to tax their muscles, their intra-abdominal pressure will probably not be sufficient in the majority of cases to force the aperture which has been closed.

It is only during the last very few years that a union of the wound after herniotomy could with any degree of certainty be guaranteed, and it is this primary union that has everything to do with the thorough repair of the approximated structures.

Some little time ago, and even now, operators have been heard to say that suppuration is rather a good thing, inasmuch as the excess of scar tissue will make a firmer mass of resisting tissue. Nothing can be more fallacious. Apart from the risk and inconvenience to the patient attending septic infection of the wound, suppuration means early disturbance of the sutures which temporarily hold the approximated structures in close apposition, and may also destroy normal tissues to a marked extent. Scar tissue is very prone to stretch, and without doubt is not by any means to be compared to normal fibrous tissue or muscle—in fact, the less scar tissue formed during repair the better for the permanency of the union.

It seems to me that the more recent operations which aim at a complete closure of the inguinal canal have little to choose between them.

Sufficient time has not elapsed to seriously test any of them, and to accurately follow up a large number of patients is almost impossible. At the same time I maintain that the best operation is that which places the stump of the sac out of harm's way and closes the canal without dividing the abdominal wall.

(This paper was illustrated by a series of explanatory diagrams.)

DISCUSSION ON INGUINAL HERNIA.

Dr. Barnett (Dunedin) said he had only one or two words to say, because Dr. Russell's paper was so elaborate and had been so well thought out, and gave so much food for reflection, that it was beyond his power to say much in the way of criticism without mature consideration. He must congratulate him on the originality and general excellence of the paper. Of course, it was a paper which gave much food for reflection, because it made statements in direct opposition to those usually accepted. Still, it brought forth abundant proof, and one ought to carefully consider his arguments before replying. He had stated that if the funicular process was present at birth it could only be closed by operation. He had described somewhat accurately his operation; but he (Dr. Barnett) had not got as many details of it as he should like. Personally, he found in operating for congenital hernia it was not easy to separate the spermatic sac. He thought it was the usual experience of all that the cure of congenital inguinal hernia was an easy matter. A good many cures did take place from the use of trusses. It was his practice to try the truss in the case of congenital inguinal hernia. If, after twelve months' use of the truss, the hernia was still in existence, then he was inclined to recommend operation. In regard to Dr. Hinder's paper, one point was that such extensive suturing would require considerable attention. There was thus some risk.

Dr. Scott (Ballarat) said he had not very much to say. They had a distinguished anatomist present, and he would very much like to hear his opinion as to the correctness or otherwise of Dr. Russell's theory. He must congratulate that gentleman on his masterly paper and the originality of his theory. He had certainly worked it out ably. He (Dr. Russell) must start, so far as he (Dr. Scott) could see, with an "if"; and if his contention was that processus vaginalis was present in all cases of hernia, he was with him. He had not had the number of cases that Dr. Russell had, but he had had one case of thirteen weeks. The child was brought with a strangulated hernia, and the hernia had been down for a period of twelve hours. He immediately operated, but he did not think that could be purely congenital in the way of Dr. Russell's argument. He thought it was a separate sac. But he had no difficulty in separating the sac from the cord. He was strongly of opinion that, in most cases of hernia, operation was desirable. He agreed with Dr. Russell that the truss, to say the least, was a most uncertain method of procedure. It in some cases secured an apparent cure, but there was no surety that the cure was permanent. They could say they had seen some cures by trusses, but there had been no opportunity of judging whether the hernia had recurred. He leaned, therefore, strongly towards operation. Dr. Russell had given them a very valuable note—that was, in not dividing the structures more than necessary. The less one separated the structures, so much the better for a permanent cure. The same with Dr. Hinder's paper. He agreed with him in that regard; but he did not agree with him in putting in so many sutures. One could get the structures together with less sutures. One could not be sure, either, of the sutures being sterilised. Therefore the fewer one had the less fear there was of suppuration.

Dr. Nash (Wallsend) said there was one point in Dr. Russell's paper to which he would like to refer. He had said he could not conceive how the peritoneal surfaces in the canal could adhere. That would hold good in cases where the apposed surfaces were still. But as in the cases of eucalyptus trees, the young branches of which, rubbing together, formed the strongest part, so with the peritoneal structures. This was the way in which the matter appealed to him.

Dr. Syme (Melbourne) said the matter which had been brought under the notice of the Section had previously been—in a less explicit manner, perhaps—brought before the Medical Society in Victoria, and had been discussed, and that was why some preferred to hear first the opinions of others to whom the matter was perhaps new. There was no doubt that, logically and theoretically admitting certain things, Dr. Russell had given them a very complete demonstration of his position, but he must say that in the chief applications of that demonstration it had seemed to him that Dr. Russell had somewhat exaggerated some points, and first as regards the anatomical position. He (Dr. Syme) had been in the dissecting-room for nearly half his life, and he had been very observant. He must say he failed—and he had been giving it a good deal of attention since Dr. Russell read his paper—to recognise the sphincter action to which that gentleman had drawn attention. He was not prepared to admit this fundamental principle. It had been pointed out that an operation by the Bassini method would do away with this sphincter action. Therefore one would expect that in Halstead's operation the operation would not be a success in some cases. It was to be expected that in a young child, instead of adding to the success of the operation, it would have the opposite effect. He would like to hear what Dr. Moore had to say in this respect; he had performed the operation a number of times. He (Dr. Syme) was very reluctant to speak on this point; while he had had a large experience with inguinal hernia, he had not had a large experience with children. Then there was the other point which previous speakers had drawn attention to. Dr. Russell assumed that the funicular process, if once present, would remain for the rest of life. That was an exaggerated position to take up. He could not see that, because the developmental process was delayed, that it was necessarily delayed altogether, or stopped; and, again, he thought they must take into consideration the accumulated experience of a large body of medical men. No doubt, as Dr. Barnett had stated, and as would be heard represented by men who had seen a large number of cases, the application of trusses in childhood in a fair proportion of cases produced, at all events, what was a satisfactory result. He knew Dr. Russell said that if they followed these cases up they would find they were not a success. But the *onus probandi* remained with Dr. Russell. He knew it was said in Victoria—by Sir Thomas Fitzgerald, among others, who had a great amount of experience—that in cases of trusses ordered in childhood no further assistance was required. At the same time, while thinking there were these points to be considered in criticising what Dr. Hamilton Russell had said, he admitted the originality and cleverness of his paper, and he thought Dr. Russell had produced sufficient reasons to induce surgeons to operate in inguinal hernia in children rather than to advise the use of trusses. He (Dr. Russell) had placed the whole case before them, and the danger attending the operation—assuming the operation was done by a man who was accustomed to doing it, and under favourable circumstances. Assuming that, he, at all events, would always advise that an infant with inguinal hernia should be operated upon rather than wear a truss. Of course, there was the middle course recommended by Dr. Barnett. He had adopted the course of wearing the truss for a time, and, if it was not a success, of then performing an operation. He would certainly say Dr. Russell had given them sufficient evidence to show that there was a great probability of the main portion of his views being correct, and to induce surgeons to advise operation of inguinal hermia in infancy, and also that in infancy the operation that he suggested was all that was necessary. He (Dr. Syme) did not think it was altogether necessary to perform the operation in infancy because the structures had not had time to get separated. But of course he did not profess to speak with any authority, from a personal point of view.

Dr. Moore said he did not feel disposed to discuss Dr. Russell's paper any further. He could throw no new light upon it. With regard to sphincter action, they could not get sphincter action unless the sphincter was there. He quite agreed with Dr. Syme's remarks. With regard to operating, his experience with children was limited; but he did not think that with children such an operation was necessary. Then, he would go a good deal with Dr. Russell in connection with inguinal hernia in young adults where the hernia was comparatively recent. But he wished more particularly to refer to some points in the second paper. A remark was made that the very important thing was the restoration of the peritoneal canal, and importance was attached to the non-division of the external oblique. He did not think it was necessary to take great care in closing that external oblique. They would find that the aponeurosis would remain closed. It seemed to him that the opening of the external oblique had nothing to do with it. It merely showed that tho hernia had arrived at, and passed through, the opening. It had to travol some distance before reaching the external oblique. He thought the external oblique was of very little importance. The great reason for opening it was that it allowed the two important steps of Bassini's operation. By dividing the external oblique they exposed the whole length of the hernia sac, and they were able to clear the sac well out. And that was what was recommended by Bassini, and was one of the most important points in the operation. That could not be done with any degree of accuracy unless the external oblique muscle was divided. In the same way he did not see how any surgeon could bring the deep parts together merely by the use of his finger. He certainly would not allow himself to do it. These seemed to him to be the two great points to secure the radical cure of hernia. He thought the high ligature of the sac was of more importance than the closing of muscles. With reference to any statement by a writer that sufficient time had not elapsed to know the results in these cases, he maintained that sufficient time had elapsed. Bassini's paper was published many years ago—more than five years ago—and even at that time he had cases going for periods up to almost five years. Some of these men were soldiers, and had been in active service; but Bassini had letters from the army officers that the men were perfectly well. He had had a case of a man with strangulated hernia. He operated on him, and in less than two months he went to work (without a truss) as a coal lumper, and he had been working for about two years now without any relapse whatever. In another case the man was a miner, and he had had the same experience. Men had been at laborious work without anything occurring at all. He operated on an engineer (who did hard work) on both sides. The first operation had now been done some time, and the man remained perfectly well; so that he objected to the statement that the comparatively recent operations had not been done a sufficient time to allow one to judge. He thought the new departure was a retrograde step. With reference to passing the cord behind the muscles, he did think there was something in that. It had often occurred to him that this might be done, but he had not liked to do it. He had slavishly followed Bassini's method; but in all probability some advantage might be gained. Some reference had been made to the ligature used—catgut and kangaroo tendon. He did not think the practice was a good one or that it was likely to last. These things could not be rendered aseptic in the same way that silk could, and their absorption was not altogether to be desired. He thought the mechanical cure, as in Bassini's case, was the better. Personally, he preferred to look upon the cure as a mechanical one, and he had therefore adhered to Bassini's method altogether, and had always used silk. In not one case had he seen silk reappear. He had one case now in which he expected to see it.

Professor Watson said there were many points which wanted light upon them at this point. With regard to the funicular process, he thought, after having listened to Dr. Russell, that there wanted to be more or less evidence that his contention was probably correct. But no friend of his would be operated upon (for oblique hernia) if the aponeurosis of the external oblique was not split up, because the whole field of operation was then not placed under visual control. He had made a *post-mortem* on a man who had been operated on by Bassini's method a year previously, and he would not have had occasion to do that *post-mortem* if the hernia had been noticed before the second operation was performed. With regard to placing the cord behind the sutures, he had seen that tried, and it was a distinct advance. He had seen a man of sixty years of age with a double inguinal hernia operated on with the cord behind. He had worn a truss for thirty years; but he wore one no longer. He was working hard now at Broken Hill. It was only eighteen months ago, but it showed there was no danger in slitting up the external oblique. He did not think sufficient attention was paid to splitting the sac. And he had seen operators pay not the slightest attention to how the sac was delivered.

Dr. Hopkins (Brisbane) said it seemed to him almost a pity that they did not have the two papers separately—one treating with the hernia in the young, and the other treating with the different methods of curing hernia in adults. There were a few points he wanted to touch upon. Now, with regard to the sphincter action, he thought Dr. Russell impressed the fact that he did not refer to the sphincter action as similar to that of the sphincter ani. Without going into detail, he thought all surgeons would agree with him that there was a difference. It was all very well to talk about applying the truss to an infant; but it was another thing to see the truss was properly worn. Infants were brought up with the trusses in all places but the right place. The surgeon instructed them in the right way; but before they got to the end of the street it was off again. He thought the figures laid before them by Dr. Russell very strongly encouraged them in the view that all these children ought to be operated on. The one death, he would be excused for saying, was a preventable one. It ought not to deter them from advising parents to have these children operated upon in early youth. When they saw the number of cases brought to them, and the number of strangulated herniæ, it all forced them to the conclusion that no child should be left to grow up with a hernia. That had been his opinion and his practice as far as possible. He knew these operations were sometimes absolutely refused, and that sometimes a truss was supposed to have cured a case. Whether it did cure it or not, they could not decide, he maintained. It was in the experience of most of them to have patients brought to them with strangulated hernia, and they were told that this was the first time the hernia had appeared. He maintained that the majority of herniæ in adults were really congenital; that the processus vaginalis had not been closed up, but remained until the extra strain came on. That forced the bowel into the already strained sac, and hernia was the result. He remembered Dr. Godlee, in the hospital in which he (Dr. Hopkins) had studied, saying that 95 per cent. of all herniæ were congenital. That appeared perhaps a big statement. The operation that Dr. Hinder had described had been performed by him (Dr. Hopkins) for the past six or seven years. He had performed it in over forty instances, and in only one case was there a recurrence. Many of the men in England he had operated on had heavy work in iron foundries, and so on. The results were satisfactory so far as he saw the cases; the men reported themselves regularly every three or four months. He found a recurrence in only that one case. There was one point in which he did not agree with Dr. Hinder. He did not put so many sutures into the walls of the canal. He generally satisfied himself with three or four. He never tied them tightly, with a view to preventing ulceration. With regard to Bassini's method, he had never done it himself. The objection, to his mind, appeared to be that they substituted, for normal muscular tissue, scar tissue. The principle he went on was that if a large amount of scar tissue was bad, a small amount could not be so good as none at all.

The CHAIRMAN said that personally he had listened with the greatest pleasure to Dr. Russell's paper. It had been carefully prepared and well thought out. It had interested him. At all events, one of the principles in the theoretical care of inguinal hernia appeared to be that the peritoneal surfaces ought to be made flush. A method he had adopted in two or three cases lately might be called the latest modification of Kocher's method. It was the turning of the sac inside out. It was an excellent method. He had been interested in the discussion in connection with Bassini's method. There was good in every method, if they selected the proper cases for it. In ordinary cases of inguinal hernia, he thought it would be a pity to lay the fibres open. If suppuration took place, it must cause a weakening of that portion of the abdominal wall. In some cases he had adopted Halstead's method, and in certain cases it was a good operation. Where there was an enormous hernia, it was most suitable. He would not detain them longer, because the discussion had already been prolonged. He called upon Dr. Hamilton Russell to reply.

Dr. Russell said there was very little for him to say. Dr. Syme and Dr. Moore had cavilled at the sphincter action as he put it; because they did not grasp it in the hurried reading, as Dr. Hopkins appeared to have done. When they came to read it they would find they and he were at one on the matter. Dr. Barnett had asked if the peritoneal sac would close. He did not suggest operating on a child really very young. But if a child had attained the age of twelve months there was not the slightest chance that it would close. As to the difficulty of removing the sac, it required practice. That was all. It was always separable in practised hands; but it was difficult. With regard to Dr. Syme's remark that the *onus probandi* lay with him (Dr. Russell) in connection with the use of trusses, he accepted the challenge. He had seen plenty of instances. He frequently found children of ten or twelve years coming up for hernia after being treated with a truss. He did not say that in every case where a truss was used the hernia would recur; but it might. The operation which made a certainty of a cure was better than a procedure after which hernia might recur.

RECURRENT CARCINOMA OF THE BREAST TREATED WITH THYROID EXTRACT.

By James P. Ryan, L.R.C.S.I., M.K.Q.C.P.I., Chevalier of the Legion of Honour, Melbourne.

C.K., a single woman, first consulted me in October, 1890, about a lump in her right breast which had been growing for eight or nine months. From being no bigger than a filbert, it had increased to the size of a small apple; it was fairly movable with the breast, but the skin in front was becoming adherent to it. There were no perceptibly enlarged glands in the axilla or over the clavicle. For two or three months previously she suffered much from pain, particularly of nights, and the breast was markedly tender on manipulation. She had always been more or less delicate, though she had never had any serious illness; and she informed me she had lost some flesh, probably through want of rest and anxiety of mind. Her age was then forty-one, and her menstruation regular. On the 23rd of October, the whole breast, with a fairly large elliptical flap of skin, was removed. The axilla was not opened. On microscopical examination the tumour proved to be carcinoma.

In March, 1893, she consulted me about an ugly-looking indurated ulcer which occupied about 2½ inches of the upper and inner part of the cicatrix of the first operation. It was removed as thoroughly as possible, and the wound was healed in three weeks. All went well with her for a time, but early in 1894 she began to suffer from pain under her right arm, and she noticed a lump in the axilla. I saw her in March, and removed from the axilla a mass of enlarged glands and hardened tissue; but the axillary vessels were so closely embraced by the growth that, of necessity, a part of it was left behind. She was fairly comfortable for a time, but in October of the next year, 1895, I had again to open the axilla and clean out a large cancerous mass, without the satisfaction of being able to remove the whole of the diseased tissues. Her health was not again as good as previously. She suffered much from neuralgic pain and vomiting, and was becoming emaciated: and by the middle of the new year the axilla was again partly filled up with a cancerous mass, and there was evidently a return of the disease also in the cicatrix of the breast wound. I now felt that nothing more could be done to stay the progress of the disease, and that one's power for good was limited to employing such means as would alleviate her sufferings and smooth her way to the grave.

Shortly before this time I had read in the *Lancet* the paper by Dr. Beatson, of the Glasgow Cancer Hospital, on the treatment of inoperable cases of cancer by öophorectomy and the administration of thyroid extract. My patient was approaching the change of life, and, as she was in a miserable condition of health, I thought it would be unsafe, and, for that matter, useless, to subject her to any further operative measures. However, I saw no harm in giving a trial to the medical part of the treatment recommended by Dr. Beatson. Accordingly, the administration of thyroid extract was begun towards the end of October, 1896, with 10 gr. a day, increased to 15 gr. in December. She was also taking opium and bismuth for the relief of pain, and to control the vomiting which was most persistent. The daily quantity of the extract was gradually increased to 30 gr., which she continued taking for some weeks. I then reduced the dose to 5 gr. twice a day for a while, again gradually bringing the quantity taken daily up to 30 gr. By the middle of 1897 there was marked improvement in the patient's general health. She had put on a considerable amount of flesh, the vomiting had practically ceased, and according to her own account she had not been so well for ten years. But the change for the better in the condition of the axilla and in the appearance of the chest scar was still more significant. The nodules had disappeared, the scar was on a level with the skin or slightly depressed, and the colour had changed from a dull red to a pale pink. As to the axilla, the mass of hardened tissue which almost filled it had been absorbed, and, though the skin was considerably bound down, there was no kind of enlargement to be detected beneath it. I advised her to continue taking small doses of thyroid extract for some time longer. I saw her occasionally, and up to the end of the year 1898 she maintained the improvement which she had made, and appeared to be in excellent health. I lost sight of her for some months, but towards the end of May she consulted me about a small tumour in her right groin. It was the size of a cherry, darker in colour than the surrounding skin, and was evidently quite superficial. I excised it together with a large base of skin, and on examination it was found to be a malignant secondary growth. Of course, my attention was immediately directed to an inspection of the old foci of the disease, and I was hardly surprised, though somewhat disappointed, to find that there was some return of the trouble in the chest scar as well as in the axilla. In the latter there was no appearance of the growth which at one time nearly filled it, but the cicatrix of the wound in the skin was raised somewhat, was scaly in places, and was the seat of three or four papular outgrowths, one of them as large as half a small cherry. The chest scar was in a similar condition, scaly and with several small protuberances on it. The patient, on being questioned, informed me that she noticed the chest scar getting red and scaly in the end of January or the beginning of February, and somewhat later the little inguinal tumour, then about the size of a pea. She had some return of the neuralgic pains in her chest, but her general health was not in any way impaired. I was considering the question of the advisability or not of excising the affected cicatrices, when the issue was settled by the patient herself. She told me she did not wish to have anything further done to them. I then put her on a course of thyroid extract, the dose varying periodically from 10 gr. to 30 gr. per diem, and I am glad to say that some improvement has taken place in the condition of the scars. They are less prominent, the colour is not so pronounced, and the nodules have diminished in size. She carries, for her, a fair amount of superficial fat, and she takes her food well. She rests well at night, and it might be said that she is now in tolerably good general health.

Let me for a moment glance back over the history of this case during the past eight or nine years, and compare the patient's condition at the beginning of that period and afterwards with her state at the present time. In 1890 she was thin and worn, but improved considerably after the removal of the cancerous breast. In March, 1893, there was a return of the disease in the breast scar.

A year subsequently, in 1894, I had to clear a cancerous mass out of her axilla, and to repeat the operation a year later on account of a re-growth. This was also unavailing, for in 1896 the disease reappeared in the axilla, and showed itself afresh in the scar of the breast wound, and the condition of her general health was then as bad as could well be.

It was just at this critical period when the case appeared to be hopeless, and when there was no ground for supposing that any operation could free her entirely from the disease, that the administration of thyroid extract was begun; and from this we may date the improvement which after a few months was noticeable not only in the general health of the patient, but also in the appearance of the affected localities. An arrest of the disease with resorption of its products most certainly occurred, and was at all events coincident with the administration of the thyroid extract. The only other medicine which was somewhat continuously taken at the same time was opium, but as she improved this was lessened in quantity and then entirely stopped. Operative measures failed to control the progress of the disease, but it is allowable to suppose that its course may have been influenced by the menopause which occurred in the early part of 1897. How great, or what influence, if any, the change of life may exercise on the retrogression of carcinoma of the breast is a question that has yet to be answered, but I am disposed to think that its influence has has been much exaggerated.

I am by no means enthusiastic about the beneficial effect of thyroid extract or any other medicament on so intractable a disease as cancer. I believe that our main hope of cure rests upon early and radical operation and re-operation. Still I cannot close my eyes to the fact that some evidence in favour of the usefulness of thyroid extract in mammary cancer has been brought forward during the past few years, and that in the case of my particular patient the disease was beneficially modified or controlled by the long and continuous administration of large doses of the extract.

SOME OBSERVATIONS ON RENAL SURGERY.

By L. E. Barnett, M.B. Edin., F.R.C.S. Eng.,
Dunedin.

As illustrating various points in renal surgery, I have jotted down a few notes of fourteen cases of operations on the kidney which have occurred in my practice during the last few years. I do not intend that you should suffer the infliction of a detailed account of these cases, but purpose making some observations more or less general and more or less scattered, which have arisen in my mind, partly from consideration of these cases and partly from the writings of the leaders in renal surgery, in particular Mr. Henry Morris, of Middlesex Hospital, whom I had the privilege of knowing during the time I was house surgeon at that institution, and whose fame in the domain of renal surgery is such that he is known far and wide by the soubriquet "Kidney" Morris.

I shall refer, in the first place, to that very common and sometimes troublesome condition,

MOVABLE KIDNEY.

The minor degree of mobility in a kidney, especially the right one, is a very common condition in females; indeed, all degrees occur much more commonly in women than in men. Edebohls examined 500 women, and found a movable kidney in ninety—that is, in 18 per cent. of all cases. But in most cases the condition gives no trouble, and I should like now to emphasise this observation: That it is *unwise* to call the attention of patients to a minor degree of renal mobility which they themselves *had no previous knowledge of*, and which had caused no symptoms. If you do so direct a patient's attention to the fact that a kidney is misplaced, there is a possibility—and in females a strong probability—that a train of neurotic symptoms will follow, which may develope so seriously and obstinately as actually to call for operative interference.

Before calling attention to a movable kidney, one should be careful, by thorough examination of the patient *in every detail*, that there is nothing else that will explain the symptoms complained of. Unduo mobility of the kidney may be brought about by the absorption of the peri-renal fat, such as occurs in the emaciation of a person previously obese, or by the stretching and separation of the peritoneal attachments of the kidney to the posterior abdominal wall, such as is noted in Glenard's disease, and also in cases of abdominal relaxation after pregnancy. Those instances of the major degree of mobility to which the strange term "floating kidney" is applied (I say strange term, because "sinking kidney" rather than floating kidney should surely be the expression used) are said to depend upon the existence as a congenital peculiarity of a peritoneal meso-nephron. Instances of the occurrence of a meso-nephron, according to anatomical research, are extremely rare, and *post-mortem* one very rarely indeed finds any reference to such a thing, and I believe that a range of mobility to the extent of allowing the kidney to sink into the iliac fossa can be brought about by simple laxity of the peritoneal attachments without the necessity of there being any congenital meso-nephron. With all due deference to the fair sex, I am strongly of opinion that the abuse of that apparently indispensable article of female attire, the stays, is really responsible for a large proportion of cases of movable kidney, and I do not think this point has been yet sufficiently emphasised. There can be no doubt that a woman's stays, especially if habitually tightly laced, must force the liver downwards, and the kidneys, at best poorly supported, against the posterior abdominal wall, and, being destitute of any efficient supporting ligaments, they must be pushed down with it; at any rate, the right one is likely to be so, and as a matter of fact it is the right kidney that is almost invariably displaced. In this connection Morris's recently published statistics are very interesting. He has operated fifty-seven times for movable kidney—forty-eight times on females and nine times on males. In the females the right kidney was concerned in forty-four cases, the left being solely affected in only four cases, whereas in males, who do not, as a rule, wear stays, out of nine cases the right kidney was concerned in four, the left in five instances, showing, you see, a much more even distribution on the two sides than occurs in females. In the treatment of movable kidney with wide range of movement and with marked symptoms, I have so far been dissatisfied with pads or trusses, but well satisfied with the operation of nephrorraphy, or nephropexy as it is better called, inasmuch as, other things being equal, it is easier to say and to write. I suppose that a carefully constructed mechanical appliance might suffice to keep a movable kidney in the loin in certain cases, and I have always endeavoured to see that patients made a fair trial of this mode of treatment, but so far without benefit, and I should be glad to hear the experience of others in regard to the use of mechanical appliances for this condition.

I note that recent literature on the operation of nephropexy condemns the use of sutures passing actually into the substance of the kidney. These sutures are said to quietly cut through the friable renal substance, and may cause urinary fistula or suppuration in the kidney, or relapso of the mobility or other troubles. We are accordingly advised to suture the capsule only, laying bare the kidney posteriorly by turning back flaps of this fibrous envelope after the manner of Jacobson. Dearer, of Philadelphia, goes so far as to say that no sutures should be used, but the wound left open and made to granulate from the bottom by careful gauze packing. Although I have not myself seen any ill-effects from Morris's method of passing sutures into the kidney cortex, yet I do appreciate the possibility of them, and I have thought it wiser latterly to pass the sutures through the capsule only. In my small experience, I have seen so far no relapses, but they are said to occur in a fair proportion of cases, so that it does not do to be too confident about success. I have observed that in many women it is practically impossible to fix a displaced kidney exactly in its normal situation on account of the low position of the ribs, and in thin subjects you can still easily feel the kidney that has been sutured, lower in the abdomen than it should be, but nevertheless immovable. One of my patients, a few weeks after nephropexy, was greatly disappointed on discovering that she could still feel her kidney out of its place, and she feared that the operation had been a failure. She was relieved to find, however, that the kidney was securely fixed, and gratified at the complete cure of her former distressing symptoms.

One of the most marked cases of floating kidney that I have ever seen occurred in a woman aged forty-three, whose kidneys were so movable that they could with ease be knocked together below the umbilicus. She suffered severely from aggravated and obstinate dyspepsia, and from a strain of neurotic symptoms amounting almost to melancholia. She was emaciated and anæmic, and altogether in a pitiable condition. On separate occasions, with three weeks' interval between, I sutured both kidneys in position, and the benefit has been most marked, although improvement at first was disappointingly slow. Now, however, the patient is well-nourished and hearty and cheerful, and doing her household duties as well as any of her friends.

HYDRONEPHROSIS.

The varieties of urinary obstruction are so numerous that it is only to be expected that hydronephrosis should be a fairly common condition. A moderate distension, especially if intermittent, may give little or no trouble, but marked enlargement causes distress, and calls for surgical intervention.

Fixation of a movable kidney can occasionally be regarded as a preventive of hydronephrosis, as I am sure many cases of this condition are brought about by kinks or twists in the ureter. Hydronephrosis is often mistaken for other conditions. One of the cases I operated upon this year was under the impression that he had got an enlarged spleen. He had been told so in youth by his doctor, and always accepted that explanation of the large swelling he could feel in his left side. It gave him no trouble until his admission into the Dunedin Hospital, suffering from acute bronchitis. He was a notoriously alcoholic subject, and the hydronephrosis became septic, secondary to, and coincidently with, the occurrence of septic cystitis. In the fluid, withdrawn by exploratory aspiration from the hydronephrotic kidney, were found myriads of actively moving bacteria, and the urine from the bladder contained the same micro-organisms. I removed the kidney, which really was nothing more than a huge distension cyst, with walls no thicker than a sixpenny piece, and the patient did uninterruptedly well.

In another case of nephrectomy I expected to find a renal calculus, as there were present all the classical symptoms of that condition, and I think it is well to remember that, amongst other things, intermittent pain and hæmaturia are not very uncommon in simple hydronephrosis. Exploration showed that the kidney, although not larger than a cocoanut at the time of operation, was reduced to the condition of a thin-walled cyst, and the cause of obstruction was found in the shape of a tight stricture just below the renal pelvis. In museum specimens of hydronephrosis, I have several times seen a stricture of the ureter in this position, and I believe the liability to partial occlusion of the ureter at this point is to be explained by the fact that the kidney and ureter are developed, to begin with, separately in the fœtus, union of the two structures taking place at a later date, and this fact also no doubt accounts for many cases of congenital hydronephrosis. Hydronephrosis must be treated in very different ways, according to the varying conditions present. In those cases where a cause is discoverable, an effort should, of course, be made to remove that cause. A rectal or vaginal examination may, for instance, disclose the existence of a calculus or other obstruction near the vesical orifice of the ureter; a cystoscopic examination may help in this direction, and should be systematically practised. An exploratory nephrotomy may show a stricture or kink in the ureter, and this may be remedied by one of those conservative plastic operations suggested and performed by Van Look, Pozzi, Morris, and others. The ureter may also be explored by means of a long probe of some kind passed down from the pelvis of the kidney to the bladder, although an unusually long probe must be used, for the ureter is 12 inches or more in length, and I must say I have found it extremely difficult to thoroughly sound the whole of the ureter in this way.

Kelly, of Baltimore, with others who have done much valuable work in the surgery of the ureter, have actually carried exploratory operations right down the whole course of this duct, making what is practically a retro-peritoneal dissection from the kidney to the bladder.

If the obstruction in the ureter cannot be removed, and yet the condition of the kidney is such as to make one unwilling to perform nephrectomy, drainage from the loin is the only treatment, and this, needless to say, often means a urinary fistula.

Where the kidney is distended into a thin-walled sac from which there cannot be any urinary secretion worth considering, and where one knows by previous analysis of the urine that the other kidney is secreting sufficiently, nephrectomy I take it is the proper procedure, and in these cases I believe the operation is usually easy and safe.

PYONEPHROSIS

Is most commonly the result of either calculous or tubercular disease. In the former case, if the kidney is not completely disorganised, nephrolithotomy may suffice. In all other cases of pyonephrosis, if the surgeon is satisfied that the opposite kidney can carry on the needs of the organism, then nephrectomy should be done.

I have twice performed nephrectomy for tubercular disease; in one case—a woman aged thirty-eight—excellent health was regained for three years, but tubercular mischief in the remaining kidney then supervened, and ultimately caused death. In the other case—a man operated upon more than three years ago—health has remained perfect ever since the operation. Nephrectomy is the only course open in many cases of tubercular disease of the kidney, but is not as a rule very satisfactory.

RENAL CALCULUS.

I have only two brief references to make to this subject. One concerns the position of the incision into the kidney for exploration or for removal of stone. One frequently reads in text-books that an incision into the renal pelvis should be avoided if possible on account of the greater risk of urinary fistula in such a case as compared with an incision into the renal cortex. I am not sure that there is any justification for this statement, and I believe that, if the pelvis of the kidney can be easily brought into view, then the incision should be made into it rather than into the renal cortex. The only other way in which I wish to refer to calculus is to deplore the failure of the X rays to help us to any appreciable extent in the diagnosis or localisation of stone, but it is to be hoped that the improvements in the apparatus which are continually being made will soon lead us to depend with some degree of certainty upon this marvellous process of electrical skiagraphy.

HYDATIDS OF THE KIDNEY.

I have had three cases of hydatids of the kidney, only one of which, however, was submitted to operation. This case presented all the appearances of malignant disease of the liver. The patient, a fisherman, most cachectic and jaundiced in appearance, came to me with a nodular mass, as large as his head, adherent to and continuous with the liver, and reaching far back into the right loin. There were absolutely no urinary symptoms. I sent him into the hospital for exploratory laparotomy, and found, as I expected, what appeared to be a mass of fibro-sarcomatous growth with dense adhesions on all sides. I explored from the front in several directions with an aspirating needle, and only withdrew blood. Finally, just as I was about to abandon further proceedings, I thought I would explore from the loin posteriorily, and here I struck pus. I then made a lumbar incision, and evacuated two or three pints of purulent hydatid material, which I found came from the right kidney. Hydatid disease is everlastingly leading surgeons and physicians into errors of diagnosis; its manifestations are so numerous and so varied.

This case was as unlike hydatid disease as any I ever saw, the dense inflammatory indurated exudation surrounding the kidney so completely altering the usual signs, and I was very fortunate finally in being able to satisfactorily deal with the condition, for under irrigation and drainage the patient made an excellent recovery. The other two cases of renal hydatids are females, and both are remarkable in that hydatid cysts in large quantities have been passed by the urethra. One of the cases has been spontaneously cured in this way—small cysts and cyst skins coming away at intervals for more than twelve months, with occasionally a little blood. She must have passed altogether fully a quart of hydatid material, and coincidently a renal tumour from which she suffered disappeared. She has passed no hydatid skins for three years now; there are no evidences of renal swelling, and no urinary symptoms; and I think the case may fairly be regarded as a spontaneous cure.

The other patient I still have under observation, and from time to time she passes small cysts and fragments of membrane, per urethram, accompanied occasionally by profuse hæmorrhage. She still has a huge mass in connection with the left kidney, but operation in her case is contra-indicated, owing to the fact that she is afflicted with very serious valvular disease of the heart.

RENAL CANCER.

I have operated on two cases of malignant tumour of the kidney, both failures. In the first case which was a very obscure one, the patient, a medical practitioner, aged forty-eight, was thought to be suffering from pyonephrosis with waxy enlargement of the liver, but nephrotomy and laparotomy revealed the presence of a large peri-renal sarcoma with secondary deposits in the liver, vertebral column, &c.

The other case was one of carcinoma growing in the left kidney of an old miner. I found dense infiltrating adhesions to the spleen, diaphragm, and peritoneum, and knew when I had completed the operation that I had been unsuccessful in removing the whole of the growth. This patient died next day.

From what I have seen and read of renal surgery, operations for malignant tumours of the kidney are always associated with a high mortality, whereas it seems to me that in most other conditions the kidney is wonderfully tolerant of surgical interference, the mortality is light, and wounds heal as a rule kindly and rapidly. An incision down to and into the kidney performed under antiseptic precautions is not a very risky procedure, and such an incision may prove the presence of a calculus in the kidney or ureter, or a stricture, or tubercular disease, or hydronephrosis, or tumour, or a dozen other lesions, all more or less favourable to operative treatment. Until recent years renal operations have been regarded as *dernier resorts*, but, while deprecating any needless or reckless surgical interference, I venture to say that such operations will be and should be carried out more frequently and at an earlier stage than is the rule at present, with one or both of these two objects in view—namely, the preservation of kidneys otherwise doomed to destruction, or the preservation of life of the unfortunate sufferers.

BRIEF NOTES OF CASES.

Nephropexy—Improvement in Health.

Case 1.—E.P., thirty-five, female, married. No children. Very neurotic. Knew that there was a swelling in the abdomen that moved about. Very dyspeptic, and has twice been for long periods laid up with ulceration of the stomach. Very much troubled with a feeling of dragging on the right side. Right kidney movable almost to the umbilicus. Pads, belts, and stays of various kinds gave no relief.

Nephropexy, 27th November, 1897.—Flaps of renal capsule reflected and stitched to lumbar fascia. Two kangaroo tendon sutures passed deeply through kidney substance. No drainage tube used. Patient recovered well, and expresses herself as greatly improved by the operation. The kidney can still be felt on abdominal palpation, but is fixed in position absolutely firmly.

Nephropexy—Male Subject.

Case 2.—W.S., thirty-six, male, engine-driver. Two and a-half years ago severely strained side lifting heavy bags of coal. Since then has suffered from almost constant pain in right loin; feeling of fulness and as if something were being squeezed when he stoops forward. Cannot lace his own boots. Frequent micturition. Right kidney decidedly enlarged and movable. Has suffered from ischio-rectal abscess.

Nephropexy, 16th June, 1896.—Kidney noted as enlarged but healthy. Patient recovered rapidly. Returned to work and kept well for six months. Then suffered from repeated attacks of appendicitis with alternate diarrhœa and constipation, and passage of much mucus at times.

Appendix removed, 13th February, 1897.—The appendix was much swollen and adherent, and on the peritoneum adjacent were little scattered nodules of tubercular deposit. Abdominal exploration showed both kidneys in good position and apparently normal. Patient has been in robust health ever since.

Nephropexy—Both Kidneys—Two Operations.

Cases 3 and 4.—J.D., forty-three, female; nine children. Very neurotic, nine years previously ovariotomy performed. Very old-looking, withered—anæmic. Both kidneys very movable; they could be made to knock together below the umbilicus; liver and spleen also seemed deficient in support. Complained of severe intermittent pains in back and both loins and down both thighs. Frequency of micturition.

30th July, 1898.—Nephropexy right kidney } No drainage tubes used.
19th August, 1898.—Nephropexy left kidney }

Both wounds healed by first intention. Patient gradually improved in health until she was able to resume ordinary household duties in comfort. Has become heavier than she ever was before. Both kidneys can now be felt in abdomen, firmly fixed.

Nephrotomy for Advanced Tubercular Disease.

CASE 5.—A.Y., twenty-one, female, single, very thin and anæmic; had pain in right side and backache for two years, and from that time noticed swelling in right flank; frequent micturition; occasionally blood and pus in urine. Quantity of urine very scanty, only about 20 oz., with 6½ gr. urea to the oz. Hectic temperature. Tubercular family history. A large fluctuating swelling in right renal region and extending right down to right iliac fossa.

Nephrotomy, 29th August, 1899.—Right kidney much enlarged and soft; contained much broken-down tubercular pus, &c. Below the kidney a secondary tubercular abscess could be felt—sub-peritoneal. This was opened into and also drained from the loin.

Nephrectomy for Tubercular Disease.

CASE 6.—P.N., twenty-eight, signalman, suffering from enlargement of the right kidney. Frequent micturition. Pain in right loin and at end of penis after micturition. Pus and tubercle bacilli in the urine. Urea normal. Tubercular family history. Has lost 2 stone in weight during year. Bladder carefully examined, with negative results.

Nephrectomy, 15th July, 1896.—Right kidney enlarged, cortex completely disorganised, and very friable. Much difficulty experienced in delivering kidney, partly owing to its rotten condition, partly owing to the position of the twelfth rib. Ureter ligatured separately, and stitched to edge of wound. Patient recovered well, wound healed speedily, and he has kept in excellent health ever since, regaining more than his former weight.

Nephrectomy for Tubercular Disease.

CASE 7.—M.F., thirty-eight, married female, suffered from enlargement of right kidney and almost constant pain in the right loin for seven years; very frequent micturition. No tubercular history, but has herself had several attacks of hæmoptysis, and lungs show indication of slight tubercular mischief at right apex and right base. Urea varies in amount from 250 to 300 gr. per diem.

Nephrectomy, 29th June, 1895.—Tubercular pyonephrosis. Ureter much enlarged, and full of a material like white paint. A probe could not be passed further than 4 inches down ureter. Ureter stitched to edges of wound. Fistula remained for four months, and then closed. Patient did well after operation, and was able to resume her domestic duties. Three years afterwards, however, she died from tubercular disease of the remaining kidney.

Nephrectomy for Calculous Pyonephrosis—Cure.

CASE 8.—A.G., thirty-seven, female, married. Suffered for five or six years from severe pains in left loin. Latterly has been getting much worse. Occasionally passes blood and pus in urine.

Urea, 370 gr. per diem. Left kidney distinctly enlarged and tender. Septic temperature.

Nephrectomy, 23rd March, 1895.—Left kidney found completely disorganised. A large branched calculus embedded in the pelvis, with prolongations into calices. Cortex thinned and friable, and in a condition of suppurative inflammation.

A suppurating sinus resulted, but completely dried up in two months.

Nephrectomy for Pyonephrosis following Hydronephrosis—Cure.

CASE 9.—F.O., fifty-seven, male, barrister. Had a large swelling in left side as long as he could remember. Had always been led to believe it was an enlarged spleen, and it gave him no trouble.

Came into hospital suffering from septic bronchitis, cystitis, and alcoholism. Bronchitis improved, but severe pains and tenderness in region of swelling developed with rigors and high temperature. An exploratory aspiration of swelling withdrew foul urinous fluid, swarming with actively moving bacteria similar to bacteria found in urine passed per urethram.

Nephrectomy, 26th June, 1898.—Left kidney distended into a thin-walled sac as large as patient's head. Removed without difficulty.

Convalescence uninterrupted, and patient now says he never felt better in his life.

Nephrectomy for Hydronephrosis—Cure.

CASE 10.—W.M., twenty-seven, male, photographer. Suffered from pain in left loin for thirteen years, sometimes very severe and associated with vomiting. Symptoms ascribed to blow on back from heavy mass of putty thrown at him by companion. Occasionally blood noticed in urine, and sometimes oxalate of lime crystals; left kidney feels about twice its normal size. Case diagnosed as renal calculus.

Nephrectomy, 23rd August, 1898.—Kidney found distended into a thin-walled cyst containing clear serous fluid. Renal pelvis especially dilated, and where it joined the ureter was a well-marked stricture, the ureter below this being of ordinary calibre. Ureter carefully explored below stricture. No calculus felt. Patient has kept in excellent health since the operation.

Peri-renal Sarcoma—Exploratory Operation.

CASE 11.—T.L., forty-eight, male, medical practitioner. Suffering from intermittent pyuria with rigors and high fever, cachexia. Early symptoms of spastic paraplegia. Right kidney markedly enlarged and tender. Liver greatly enlarged. Cystitis present.

Exploratory Nephrotomy and Laparotomy, 12th July, 1898.—Revealed inoperable peri-renal sarcoma with secondary growths in the liver.

Patient recovered well from the operation, but died in about four months' time from extension of the sarcoma growth to spine, &c.

Nephrectomy—Carcinoma of Kidney—Operation—Death.

CASE 12.—H.C., sixty-two, male, gold-miner. Complained of pain in left side of very acute jumping character. Occasionally passed blood in urine. Much emaciated. Hard tender mass in region of left kidney. Urine found to contain blood every time it was examined.

Nephrectomy, 10th September, 1896.—Kidney when exposed found to be much enlarged by deposit of new growth and formation of blood cysts. With much difficulty the organ was removed. The spleen, the diaphragm, and the descending colon were all adherent and infiltrated, and before the operation was concluded I knew it must be a failure. Patient died the following morning from shock.

Nephro-lithotomy—Cure.

CASE 13.—A.B., thirty-five, female, domestic duties. Suffered from well-marked symptoms of renal calculus for eighteen months; has lost nearly 2 stone in weight. One brother suffered from gall-stones.

Operation, 7th September, 1897.—Kidney exposed and drawn out on to the loin before the stone was discovered; a small prickly oxalate of lime calculus about the size of a cherry-stone was felt through the pelvis of the kidney, partly embedded in the cortex; stone easily removed through an incision into pelvis; wound healed well, but convalescence was a little retarded by a miscarriage. No fistula resulted, although the pelvis was opened. Patient has remained well ever since the operation.

Suppurating Hydatid Cysts of Kidney—Nephrotomy—Cure.

Case 14.—T.E., fifty-one, male, fisherman. Suffering from what appeared to be a malignant tumour of the right lobe of the liver. Patient was emaciated, jaundiced, cachectic, feverish, and suffered severe and constant pain in the right side. On examination, a hard, ill-defined nodular tender mass, as large as a cocoanut, could be felt evidently connected with the liver. There were no urinary symptoms whatever.

Exploratory Laparotomy, 27th October, 1897.—Dense adhesions were discovered all round the tumour, which still seemed hepatic in origin, but it was seen to extend far back into the right loin. Exploration with syringe from the front gave negative results, and confirmed the opinion that the case was malignant. Finally the needle was introduced posteriorly, and pus withdrawn. An incision in the loin was then made, the kidney exposed, opened, and fully a pint of pus and broken-down hydatid membranes were evacuated. Convalescence was slow, but ultimately patient made a complete recovery, and has remained well.

A CASE OF EXTENSIVE MALIGNANT DISEASE, WITH UNUSUAL PROLONGATION OF LIFE.

By C. E. Todd, M.D. (Brux.), M.R.C.S. Eng., L.R.C.P. Lond.,
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Malignant disease causes death, as a rule, so certainly and so rapidly that one is surprised to find a case in which life has been prolonged, in a fair degree of comfort, for many years. When one hears of such an instance one, not unnaturally, calls in question the original diagnosis. This, however, isn't always fair to the surgeon who first saw the case. It is safe to assume that any surgeon of experience who pronounces definitely a condition to be malignant will be right in far the greater number of instances. Unfortunately, it is not always possible to obtain a specimen for microscopical examination. The following case will, I think, prove interesting. The woman is alive and comparatively well, twelve years after her condition was pronounced to be the last stage of malignant disease by more than one skilled surgeon. Moreover, the case has lately undergone a fresh development, which has increased its interest, and which has gone far to prove that the original diagnosis was correct, inasmuch as it was at last possible to examine parts of a secondary growth with the microscope.

Mrs. T., at that time fifty-four, consulted me for progressive weakness, loss of flesh and appetite, jaundice, and a lump in her abdomen which she could feel quite plainly and had noticed about fifteen months. She was a thin, cachetic, jaundiced woman, whose appearance strongly suggested malignant disease. She was markedly jaundiced, and had slight œdema of her lower extremities. On examining her abdomen a swelling could be felt continuous with the liver and extending downwards to an inch below the umbilicus. It was quite painless, hard, and was studded with "potato"-like nodules. The examination brought on an attack of vomiting, which she said had been frequent of late. She had lost 2½ stone during six months. She was seen in consultation by the late Dr. Gardner, and he agreed with me that she had malignant disease of the liver. She left the private hospital apparently in a dying condition, and I saw no more of her for nine months. After leaving the hospital she had been in bed for two months, and had then begun to take nourishment, and until recently had been in fair health. Her old troubles—vomiting, jaundice, and loss of flesh—were now what she complained of. I lost sight of the case then for a year and three months—*i.e.*, two years from the time I first saw her. She came with the same old symptoms, and a request that some operation should be done. Thinking our original diagnosis could hardly be correct, and that possibly she had hydatid, I took her into the private hospital and did an abdominal section. The liver was found enormously enlarged with hard, nodulated, lightish-coloured masses scattered here and there. They varied in size from a cricket ball to a small pea. The two surgeons who assisted me at the operation felt certain that these masses in the liver were malignant, so that, after separating some adhesions between the transverse colon and the under surface of the liver, I closed the abdomen and the patient made a good recovery. After the operation she improved in health and was 2½ stone heavier in six months, although her liver continued gradually to enlarge. I saw the patient occasionally for the next six years, and, although she had occasional attacks of vomiting and jaundice and was much inconvenienced by the size of her liver, she had a fair degree of health. Three years ago, however, she began to lose flesh rapidly, and the œdema of her lower extremities increased. She came into hospital, and at her urgent request I did another abdominal section. The masses on her liver had undoubtedly much enlarged since I last had a chance of examining them; the liver itself seemed almost to fill the whole abdomen. I introduced a hollow needle into several of the masses and also in various directions in the liver substance, but obtained nothing but blood. There were numerous adhesions between the liver and the larger intestine. The omentum was adherent to the abdominal wall, and contained enormous veins which were a source of embarrassment during the operation. The abdomen was closed without any drainage, and the patient made an uninterrupted recovery. Her condition improved greatly after this operation, the œdema in her legs rapidly disappeared, her appetite improved, and she gained weight and strength, and went away to the seaside. Seven months ago she came to see me, complaining that for a year or more she had suffered from bleeding and copious purulent discharge from the vagina. On examination a large very vascular cauliflower mass was found, projecting into the vagina. Under ether this was removed, and the whole of the interior of the uterus was found to be hollowed out by ulceration. The uterus felt just like a shell, and, although I scraped the whole of the inside of the organ, I was afraid to use the curette at all vigorously in case it should perforate the uterine walls. Here was a condition of what proved by microscopic examination to be very advanced epithelioma of the os cervix and uterine body, without any great loss of health and flesh, and without any inconvenience except the annoyance of the constant discharge. The liver remained much the same as at the last abdominal section. This last development of malignant disease would, I thought, at last cause the patient's speedy loss of flesh and death. Up to now, however, seven months after the operation she remains in quite fair health, and only has slight vaginal discharge. Whether she will continue to show the truly remarkable power of resistance to this form of malignant disease that she has before, remains to be seen. I fear, however, that as this ulcerated mass is in contact with the external air the patient may get into a chronic septic condition. I add Professor Watson's account of the microscopical examination of the tumour removed from the cervix uterix:—

"Cannot say whether it is a secondary growth from liver. It is probably primary—viz, nodular form of carcinoma, in which the cervical mucosa in places is broken down into deep excavations around which the stroma consists of vascular (easily bleeding) connective tissue, infiltrated with inflammatory round cells, and showing some dilated lymphatic spaces stuffed with large polymorphous epithelial cells not at all of the usual glandular type of epithelioma of cervix."

One can hardly, perhaps, hazard an opinion as to why this ordinarily rather feeble woman has been able to resist the cancerous process for so many years. It is a curious and interesting fact, however, that she comes of a particularly long-lived family. Several of her relatives have lived to over eighty, and one or two to ninety years. She inherits, therefore, a remarkable tendency to live to old age, and nothing, as far as I have seen, influences one's prognosis in cases of serious disease so much, although one doesn't generally consider that this applies to malignant disease. I have been often struck by the fact that persons who come of a long-lived family will very constantly recover from conditions which by all the rules of medicine and surgery are hopeless. While I write I can call to mind the case of a stout man, ætat. fifty-five years. He had had diabetes and occasional albuminuria for many years, and when I saw him he was passing, in spite of 5 gr. of codeine daily, an enormous quantity of sugar. Both of his legs were largely œdematous, and he had constant cough from œdema of both lungs. He was, moreover, drowsy for days together, and both his medical attendant and I thought that the case would shortly end by coma. The most favourable part about his case, however, was that all his family had lived to a great old age, several of them to over ninety. After being ill with one complication of diabetes after another, the patient ultimately got better, and is now to all appearance in the best of health, although he still has abundance of sugar and occasionally albumen in his urine.

The very marked improvement which followed on the abdominal sections also requires some explanation. The only reason that I can assign for it is that the omentum became adherent to the abdominal wall low down, and also to the intestines in the lower part of the abdominal cavity. In this manner a collateral circulation was established, and a large portion of the blood was thus able to get back from the lower extremities and the abdominal contents without the necessity of passing through a hard and enlarged liver. This would explain the enormous vessels in the great omentum which, I have before mentioned, were a source of difficulty at each abdominal section, and particularly at the second.

5th September.—I have seen this patient to-day, eleven months after operation, and, although she is bleeding constantly, she is in good health, and has not lost very much flesh.

FOUR CASES OF VASECTOMY.

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The value of vasectomy in reducing the size of prostatic overgrowth being still a debateable point, I have thought that an unvarnished account of four cases in which I have recently divided the vasa deferentia might prove of some interest to other practitioners who may be confronted with a case of enlarged prostate in which prostatectomy appears too severe an operation to be undertaken.

The arguments against ligation and division of the vasa are :—

1. That the operation is useless.
2. That, if effective, the relief is only temporary.
3. That the operation carries with it a serious risk of inducing that mental condition known as "Old Brain."
4. That it is repulsive to "castrate" a man still sexually active.

1. With regard to the first point I would prefer to leave the answer to a more competent surgeon than myself. Statistics, we are often told, can be made to prove anything, but time and a careful study of a sufficient number of cases must eventually prove the truth.

2. After seeing a considerable number of cases I can only say that the "temporary" relief still continues after an interval of many months. But grant that it is only a temporary respite from retention with its manifold and miserable sequelæ—from constant micturition and sleepless nights, or even from the uncontrollable sexual excitement from which so many of these cases suffer—is not this so called "temporary relief" (and I hold this means a period of months) better than advising a very severe operation to patients of advanced age, who can so badly bear a long confinement in bed, or the very considerable loss of blood which the operation of prostatectomy often entails?

3. The average age at which I have seen and done vasectomy has been between sixty and seventy-five, and, though I have not had the opportunity of observing any case of mental decay following this operation, the experience of others is against the non-appearance of this complication. But why should "Post hoc" be "Propter hoc"? Do not many men suffer from mental decay between the ages of sixty-five and seventy-five? Or is the operator to regard the possessor of an enlarged prostate as one fortunately exempt from natural degeneration of his cerebral tissues, and to blame his surgical interference because his patient does not possess the mental power of a Gladstone?

4. This, the sentimental, but from a patient's point of view, often the most powerful argument, should not need much contradiction. I have always very carefully explained to each patient the effect that this apparently simple operation will have upon his procreative powers, and perhaps the philosophy of age has had something to do with the usual answer—that he is "past work," and any relief from his vesical worries is more attractive than sexual anticipations. The question naturally arises, Why does division of two tubes, only indirectly connected with the prostate, cause diminution in the size of the latter organ? The vas deferens is a continuation of the tube of the epididymis, and extends from the globus minor to the prostatic urethra, passing, after its twenty-inch journey, through the inguinal canal and pelvis, to the base of the prostate, where it effects a junction with the vesicula seminalis to form the ejaculatory duct, which runs behind the middle lobe of the prostate and enters the urethra. The prostate, on the other hand, is a mass of glandular and muscular tissue, the latter being chiefly of the unstriped variety, its physiological function being to increase the bulk of the seminal discharge, and to prevent any back-flow into the bladder. The fact that in eunuchs and children, whose prostates are undeveloped, there is no want of complete control over the bladder proves that this organ is not an assistant to the bladder itself.

There must be, therefore, some intimate nervous connection between the vasa and the prostate, and I think that this may be traced thus:—

The hypogastric plexus of the sympathetic system gives off two lateral plexi, the pelvic, which in turn supply the vesical plexus; from the latter on each side a fine network is given off which clothes the vas deferens and lies on that tube as far as the internal abdominal ring. Here the spermatic plexus which comes from the aortic and renal plexi enters the inguinal canal, and joins with the deferential plexus and runs in company with it on the vas. The prostate is richly supplied from the vesical plexus, and contains those large ganglionic bodies known as the ganglia prostatica of Muller. From the prostatic plexus spring the cavernous nerves which supply the erectile tissue of the penis. In this way a direct connection can be traced between the nerves of the vasa and those supplying the prostate gland through the parent vesical plexus, and also another connection between the vasal and renal nerve supplies through the aortic plexus. The sympathetic nerve fibres have merely the power of conduction, whilst the ganglia have the additional functions of reflection and transference; thus each ganglion with its allied nerves forms a minute but complete nervous system in itself. The varied powers of the sympathetic system have been amply demonstrated by many well-remembered experiments; its action on the blood-vessels being shown when sympathetic supply to the submaxillary gland is stimulated, the blood-vessels immediately contracting and the flow of saliva becoming scanty.

Its action on involuntary muscle can be recalled when we consider the rapid action of the heart when the spinal cord is stimulated after severance of all connection between them, except *viâ* the filaments derived from the inferior cervical ganglion; and a second example is found in the action of the radiating fibres of the iris, whose nerve-supply is derived from the sympathetic root of the lenticular ganglion.

Lastly, that the sympathetic system has a marked trophic influence is demonstrated by the experiments of Majendie, Mayer, and Longet, who showed that inflammation of the eye always resulted after ligature of the sympathetic in the neck, and that destruction of the eye occurs more rapidly when the fifth nerve is divided beyond the gasserian ganglion than when the nerve is cut between the ganglion and the brain, the trophic influences being apparently transmitted through the sympathetic fibres which join the fifth nerve at the ganglion.

When we consider all these actions of the particular nervous system from which the vasa and the prostate derive their supply, is it unreasonable to conjecture that when the vasa deferentia are divided a powerful stimulus should be reflected to the gland which is sympathetically so closely allied to the injured tubes, and an atrophic as well as vaso-constricting action thus induced; or is it merely that the functions of the latter being destroyed the prostate receives a reminder that its auxiliary assistance is no longer required, and so suffers from the decay of disuse?

The following are brief notes of four cases of double vasectomy performed under cocaine:—

Case 1.—Patient, aged sixty-six, has been seen by me for the last two years, suffering from enlarged prostate, chronic cystitis, and kidney disease. Cystoscopy showed an extreme state of atony of the bladder walls, whilst the constant passage of a casually cleaned catheter has rendered his urethra a study in urethroscopy. Prostatectomy being out of court in this case, and as perineal section had already been performed without relief, I offered him the last alternative of vasectomy. Both vasa were divided under cocaine. The catheter was used for some days after, and then, according to his own account, he discarded the use of this dangerous instrument except once daily. I have seen him at intervals for the last six months, and lately he told me that his urine passes freely, and that his only trouble has been slight scalding during micturition. The urethral mucous membrane, though much injured by his previous instrumentation, seems much healthier than when I first examined it many months ago.

Case 2.—Patient, aged seventy-four, was seized with sudden retention. He stated that he had never had gonorrhœa or any previous attacks of retention. On rectal examination the prostate is enormous. A catheter passes right up to the hilt before tapping the bladder, showing how elongated and tortuous the canal has become. Both vasa were ligatured. He still needed the catheter for some days after the operation. On the fifth day he passed water naturally. He was discharged four weeks after the vasectomy, and was taught how to use a catheter. I cannot consider the result of this case absolutely unsatisfactory, as the patient has, at any rate, gained the power to enter on catheter life without impediment, and I trust that the prostate will eventually atrophy.

Case 3.—Patient, aged seventy-four, complains of constant micturition. He passes his water every few minutes. Cystoscopy showed a large circular collar of prostatic overgrowth. Vasa ligatured and divided. Ten days later the frequency had markedly diminished. The patient states that he has been able to sleep comfortably lately, as the calls to empty his bladder have been much less urgent. This patient complained of pain which ran from the site of the incision on the left side to the left kidney. This case, I think, was at least satisfactory in that it certainly gave relief from the distressing frequency of urination.

Case 4.—Patient, aged sixty-seven. He was admitted with retention. He is a prematurely-aged man, who has had a very hard out-door life with scanty feeding. The prostate was huge. The kidneys were diseased. A cystoscopic examination showed an extreme degree of atony of the bladder, the walls being simply a network of fibrous cords. The vasa were divided. Next day the urine was blood-stained. Urine was passed per urethram eleven days after operation, but catheterisation was still continued. Five weeks later the urine became foul, and perineal section was performed to enable the bladder to be washed out continually. Eleven weeks after operation the patient died.

I was extremely doubtful about performing vasectomy on this case, but the patient was desirous of returning to his residence (a hut up country), and, as it was hard work to pass a catheter on him, I operated in the hope that sufficient shrinkage of the prostate would take place to allow him to use a catheter on himself with comfort.

To briefly analyse the above cases.

Age.—Between sixty-six and seventy-four.

Condition of Life.—All were labouring men, whose tissues showed the effects of hard work and scant living. In two of the cases kidney disease was present, and therefore prostatectomy seemed too severe a procedure to suggest. In one case, the frequency of micturition, with its resulting sleeplessness, was certainly improved. In one case the cystitis has certainly been ameliorated. In one, catheter life has been rendered easy, and I still hope to hear the gland has diminished in size. In two cases I noticed that for two or three days after the operation the urethral canal seemed more blocked when the catheter was passed. In one case the urine was blood-stained. The explanation may be that the vaso-motor constriction has occurred, and hæmorrhage had taken place from the vesical portion of the gland, also that universal contraction had already begun, and the prostrate urethra was consequently narrowed temporarily.

In conclusion, I trust that these brief notes may be of some interest, and if I have unconsciously committed plagiarism I must ask the forgiveness of men more fortunate than the writer in possessing the latest literature on this subject.

THE PREVALENCE AND ÆTIOLOGY OF CANCER OF THE STOMACH.

By C. L. Wolfgang Hunt, M.B. et Ch. M. Syd.,
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My excuse for touching on such a well-known disease as cancer of the stomach is that unfortunately it seems to be on the increase if one may judge from statistics and personal experience, and I would have an expression of opinion in regard to its ætiology, diagnosis, and treatment, with a view to prophylaxis, should prophylaxis be possible.

Within a period of five years I have witnessed the closing scenes in the life history of nine cases of apparent primary carcinomata of the stomach (often certified merely as malignant disease). An examination of the district and town register gives seven other cases as having occurred during about the same period within a radius of about 30 miles. During the preceding five years I did not find more than three cases notified as having died from the same cause. These figures as far as they go show a decided increase in the mortality from this disease.

Next I give a tabulated list of cases, including four cases of cancer of liver, stomach, and omentum, with the brief information available from the death certificates as to length of time ill, final cause of death, age, &c., and occupation.

TABULATED LIST OF CASES FROM REGISTER.

Name	Age.	Occupation.	Special Remarks.	Cause of Death mostly as Entered in Certificate.	Time Ill.
G.B.	77	Viticulturist & labourer	...	Cancer of pylorus	Two years
G.B.	39	Farmer ...	Son of above ...	Cancerous growth in region of pylorus	Some months
C.R.	42	Miner ...	...	Cancer of pylorus and perforation and peritonitis	Some months
R.N.	55	Carpenter ...	Some idea of habit of fixing wood against region of stomach while planing it	Malignant disease of stomach, liver, and omentum	Seven months
J.F.	48	Labourer ...	...	Cancer of stomach, asthenia ...	Some months
M.O'B.	41	Carpenter ...	Some idea of habit of fixing wood against region of stomach while planing it	Cancer of pylorus, marasmus, asthenia	Twelve months
W.T.	53	Storekeeper	...	Cancer of pylorus and morbus cordis	Six months
W.C.	69	Telegraph line repairer	Definite history of injury some years previously	Cancer of stomach, dropsy, asthenia	Some months
J.Mc.	73	Pastoralist...	...	Cancer of stomach, icterus, asthenia	Four months
P.N.	64	Farmer ...	...	Cancer of pylorus	Some months
W.P.	74	Farmer ..	...	Malignant disease of stomach ...	Some months
A.G.	57	Farmer ...	...	Carcinoma of stomach	One year
C.D.	63	Housewife ...	...	Cancer of stomach, omentum, and liver, and dropsy	Two years
A.S.	68	Housewife ...	...	Carcinoma of liver and stomach	Some months
S.M.	52	Housewife ...	...	Carcinoma of stomach and liver	Eight months
J.H.	59	Labourer ...	...	Cancer of stomach and liver ...	Two months
H.T.	53	Farmer ...	...	Cancer of liver and stomach ...	Two years
A.D.	56	Housewife ...	...	Malignant disease of liver, and probably stomach	Some months
M.D.	41	Housewife ..	...	Abdominal cancer and dropsy ...	Some months
G.B.	67	Selector and drover	Hard rough diet in early life	Malignant disease of stomach and colon	Eighteen months

Average age—57·3.

The nine cases under my care presented themselves for treatment unhappily too late for the possibility of either error of diagnosis or relief by surgical interference; for, in most instances, when diagnosis by means available is definite, the disease has made progress which prohibits more than palliative measures at the surgeon's hand, and often not even that.

CLINICAL AND PATHOLOGICAL NOTES.

The time at our disposal will not allow me to describe seriatim each case; it is sufficient for my purpose to say that they all presented a marked similarity. The primary symptoms complained of were flatulence, eructations of gas and fluid, feeling of distension and abdominal malaise, at first after taking solid food, then after taking nourishment of any kind.

A wasting and general cachexia far in excess of that due merely to malnutrition was soon established in the course of these symptoms; finally, vomiting of food at varying intervals after ingestion, but becoming more frequent, set in, generally the final stage in obstructive conditions of malignant disease of the stomach, the beginning of the end.

Pain is a variable quantity, often most severe in the early stages when the delicate solar plexus is first involved.

Dropsy, or more correctly, ascites, depends largely on the involvement of the gastro-hepatic omentum, with pressure on the portal vein.

The wasting in lingering cases is frightful to behold, the patient becoming literally a " living skeleton," every vestige of adipose tissue having disappeared, while the muscles atrophy.

The Age.—Majority were in the middle period of life, the twenty cases giving an average of 57·3 years.

Occupation.—Of the labouring class, for the most part; hard-working, energetic people; farmers, carpenters, miners, and housewives, taking life hard and their pleasures rarely.

Family History.—Uniformly good; hereditary taint possible in one family, father and son dying the same manner of death, from the same disease, within three years of each other, the progress of the disease being more rapid in the case of the son.

Personal Health.—Uniformly good, but with vague history of transient dyspepsia.

Traumatism.—Definite history of injury in one case, a telegraph line repairer, who was struck over the region of the stomach a few years previous to the onset of his symptoms. Two carpenters also complained of injury in holding timber against abdomen while working it.

Diet.—Plain, hard food, indifferently cooked, more especially when " roughing it " in early life on a dietary of salt-junk, damper, and ration tea.

P.M. Notes on Two Cases.—W.T., fifty-three, and Dak Fou (Chinaman), fifty-four. As conditions were so similar in both these cases, one description will suffice for both :—

Primary Seat of Lesion.—The primary seat of lesion was apparently the pylorus, extending thence towards the cardiac end of stomach; the duodenum was not affected.

Prolongations of the disease have spread centrifugally into the adjacent tissues and organs, especially along the gastro-hepatic omentum, matting everything closely together in a hard atrophying or contracting mass, by which the natural relation of the parts was seriously disturbed; in one case there had been such drawing up and obliteration of the stomach that only a small portion of the greater curvature at the cardiac end was remaining to represent that organ; in both cases there was active ulceration of the mucous lining, the source of hæmatemesis that had occurred during illness; there was thickening and cancerous growth in the fissure of liver, with metastatic deposits and nodules in the left lobe of liver.

The conditions found demonstrated only too clearly the utter hopelessness of radical cure by operation late in the course of the disease, once it has spread beyond the confines of the pylorus and its immediate vicinity.

The microscopic appearance was, in both cases, that of typical columnar-celled epithelioma or so-called adenoid cancer.

REVIEW OF PATHOLOGY AND ÆTIOLOGY OF CANCER OF THE STOMACH.

As regards the existence of a bacillus of cancer, such has been reported as isolated, and even verified by bacteriological proof, but confirmation is lacking.

So-called cancer cells or corpuscles have been from time to time described as the actual potential agent in cancer-causation and metastatic infection, but if such exist their exact relation to the disease is still unknown.

On the other hand, the pathology of cancer tissue is clearly demonstrated, both macro- and micro- scopically; and until our knowledge on other points is greater we must reason largely on pathological facts in order to arrive at any definite theory in regard to the ætiology of the disease; for it is only by a thorough grasp of the ætiology of any malady that we can successfully guard against it, and prophylaxis is the goal of our profession.

We can bring strong evidence to prove that friction and irritation of tissues, by causing increased vascularity chiefly, tend to produce hypertrophy of their intrinsic structure; this is especially true of epithelial tissues. Moreover, towards the middle period of life, such hypertrophy, if the irritation is continued, is prone to take on malignant aspect—*i.e.*, the tissues involved, while becoming more elemental in structure, overstep their normal limits, invading other organs, at the same time that by pressure, or by pirating their blood supply, or by exercising some intrinsic atrophying power, it absorbs them piecemeal; simultaneously the contraction of its own fibrous stroma destroys its epithelial elements and blood vessels; consequently the growth degenerates, breaks down, and ulcerates in its own older and more central parts, thus causing typical malignant ulceration, a condition so often met with in the late stages of adenoid cancer or columnar-celled epithelioma of the stomach.

Are we then justified, in the absence of reliable evidence of bacteriological causation, in taking a source of continued irritation as a potent factor in this disease? If so, what conditions are liable to produce this noxious influence on the stomach, and how can we avoid them?

May not tardily digested food, by undue irritation, cause a pathological state of the delicate mucous coat, a benign hypertrophy with proliferation of peptic cells? Thence arises an acid dyspepsia, with more irritation, and finally a malignant hypertrophy with contraction, ulceration, obstruction. The intervals may be long, the process slow; the end is the same.

The conditions may start in early life and remain quiescent; may cease and disappear for ever; or, on the other hand, under unfavourable circumstances, they may increase and eventuate in malignancy.

If, then, there is the faintest reason for supposing that delayed digestion, or indigestion, or dyspepsia—call it what you will—can have this morbid tendency, we must give most earnest consideration to the question of foodstuffs as possible factors in the causation of cancer of the stomach, more especially in regard to the following points:—

(*a*) Quality of food, especially meat and tea;
(*b*) Cooking and preparation of same;
(*c*) Mastication of same;
(*d*) Period of rest after meals.

(*a*) With regard to meat and the quality of the flesh of beasts suffering from—

Actinomycosis,
Tuberculosis,
Fluke,

Has such meat direct infective action, or does it only act from its inferior quality affecting its assimilation and digestibility; or does it possess some peculiar toxin prone to cause indigestion?

The public are misled by the term "cancerous" beasts, and fear a direct infection; this is most improbable from the fact that beasts apparently do not suffer from "human" cancer, but from *actinomycosis*, so-called "lumpy-jaw," or "lumpies": the latter term is also applied by cattle-dealers and butchers to beasts with glandular enlargements of the throat and neck, which are rather of tubercular causation. However, with regard to all these diseases, including the liver-fluke of sheep, the degree of heat to which meat is exposed in process of cooking would effectually destroy all the specific bacilli and their spores, and so render all fear of direct infection groundless, to say nothing of their having no pathological affinity to cancer.

On the other hand, indirectly such meat may exercise a pernicious influence from its being essentially of a more indigestible nature, and personally I am inclined to attach considerable importance to it for that reason; the action of the digestive juices may be resisted—

(*a*) By its poor quality
(*b*) By some intrinsic toxic property yet to be discovered and isolated.

Tea may likewise be described as having a deleterious action in two ways:—

(*a*) Its action *directly* on the stomach.
(*b*) Its action *indirectly* on ingested matter.

Tea, as is generally drunk among the lower classes, is virtually a "brew," rather than an "infusion," of leaves which, though sold as tea, is so largely adulterated that it contains less of the essential oils of tea than of tannin, and the more the tannin is extracted the better relished is the resultant "brew" for its colour and its taste; the labourer has a large "billy" full of it, from which he periodically slakes his thirst during his working hours; the busy housewife has her pot of tea on the hob, of which she partakes scarcely less frequently. This powerful tannate is swallowed between meals and at meals, and the strength of the last draught is awful to contemplate. I have been frequently told, and have verified the statements, that the economically disposed often boil up the residuum and the leaves for a fresh decoction again and yet again.

Little wonder, then, if these misguided individuals, after a time, possess a stomach whose mucous membrane and secreting cells are so degenerated by this slow tanning process as to be functionally useless. Nor does the mischief end here; every particle of ingested animal food is brought under the same tanning process, doubling the digestive labours of the stomach.

(*b*) Other foods, damper, indifferently-made bread, all play an active part in the trouble of digestion, and doubtless in many instances, where the food staples are good, the cooking and preparation thereof is proportionately bad.

(*c*) Lastly, the early decay of the teeth, especially the molars, on which depend the proper mastication of solid food, has a large share in the establishment of morbid conditions of the stomach.

Early dental decay is a marked feature in Australia, and may be due to the largely meat diet allowed children, and also to the complete neglect of cleaning and care of the teeth; it is the exception to find a child whose sixth-year molars are not completely decayed within two years after their eruption; and they are left in that condition, filled with bacteria and putrefying *débris* of food which taint every mouthful ingested, while working their pernicious effects on the other permanent teeth, as they in turn erupt, by the corrosive action of the acid products of fermentation on the delicate young enamel and dentine.

(*d*) Another circumstance is worthy of consideration in its relation to digestion:—The rush and hurry to finish a meal and afterwards start work on a full stomach, thereby diverting the blood supply and nerve energy that should be concentrated on digestion into such other channels as the particular form of work engaged in may require. To this is attributed the great prevalence of dyspeptic troubles among the people of the United States, essentially a nation of festination and hurry. I have seen men resuming such work as ploughing, stumping, drafting stock, mounted on an exceedingly rough horse, immediately after a heavy mid-day meal of hard salt-junk, potatoes, and black tea! Fancy the bruising and injury to the delicate lining of the stomach in such cases, often forging, link by link, the chain which begins with dyspepsia and may be ends with cancer!

The importance of correct diagnosis early in the course of the disease is only equalled by its difficulties with the ordinary means at our disposal: the protean symptoms of dyspepsia may be simultaneously and independently the far graver ones of incipient malignancy. A comparison of symptoms complained of and noticed early in their course in cases that have afterwards proved themselves malignant will show that they might stand equally well for those of simple dyspepsia.

Again, considering the progress of cancer of the stomach chronologically, is the disease:

(*A.*) Of comparatively recent formation, with short remissions, steadily progressing?

(*B.*) Of old standing—
- (*a*) Continuously progressing?
- (*b*) With long intermissions of perfect health?

Are the symptoms felt in the first—
- (*a*) Purely dyspeptic manifestations?
- (*b*) Purely malignant manifestations?
- (*c*) Do they partake of the nature of each?

Consequently the location of a tumour or cancerous mass or thickening in the region of the pylorus is the only diagnostic sign of certain value; to do this by palpation is most difficult, and even when felt one day it may elude discovery the next.

Is it not the proper course then to strongly urge the patient to submit to the risk of an exploratory laparotomy in these doubtful cases where the surgeon has grave suspicions of malignancy, even though he believes, and the symptoms indicate, nothing more serious than dyspepsia? If nothing is found, all is well, and anxiety at an end; if cancerous tissue, there may be then the chance of its successful removal: either gives the best result.

Treatment.—This brings us to the subject of treatment. In early stages, after definite diagnosis, when the patient is willing and his constitution permits, free removal by operative measures, grave as is the risk and difficult the performance thereof, holds out the only hope of saving life.

In the latter stages, when elimination is no longer feasible and permanent cure hopeless, we are brought face to face with the alternative—

(*a*) A palliative gastro-enterostomy, only postponing, as it were, the bitterness of death; or

(*b*) The unimpeded onset and establishment of malignant cachexia, wasting, starvation, asthenia, a death of agony.

In either case death ends his troubles; but a gastro-enterostomy may spare him the tortures of obstructive starvation, with indifferent relief from morphia, though it may prolong his journey through the valley of the shadow of death.

Will not the day soon come when what is ordained by law to end the sufferings of a beast lingering in agony be allowed in the hopeless mortal sickness of a human being, and the poor victim of cancer may by anæsthesia find euthanasia, drifting away from life's stormy shores on the calm sea of oblivion?

In conclusion, if the factors I have endeavoured to make prominent have influence, partial though it be, in the causation of cancer of the stomach, surely there exists a self-imposed duty to endeavour by words of caution and advice to warn those whose knowledge is less than our own to have more caution in the future, that by any means in their power they may avoid all probable risks of such a dire disease.

Let them be more careful, I say—

(1) In selecting pure and wholesome food;

(2) In the cooking and preparation of the same, and most especially in the preparation and use of tea;

(3) In maintaining a sound condition of the teeth by every care in childhood, and after, and a healthy condition of the mouth;

(4) In taking due time over their meals, and a proper period of rest after partaking thereof;

and they will find that in this instance, as in others, prevention is better than cure, both now and in the future.

A CASE OF ALLANTOIC CYST.

By R. D. Pinnock, M.D., C.M. Glasgow,
Hon. Surgeon to the Ballarat Hospital.

Cysts of this variety are admittedly rare. The one now described is the only one that has come under my notice at the Ballarat Hospital during the twenty-one years I have been a member of its honorary staff, and I have never encountered one in private practice.

The patient, R.P., a lad of twelve, was admitted on the 18th November, 1898, complaining of acute pain in the abdomen, below the umbilicus, accompanied by constipation. He first complained of pain about the umbilicus twelve days ago. This came on three hours after eating a great quantity of cherries. There was no vomiting, but his bowels were at the time constipated.

A few days prior to admission, he noticed a swelling below the umbilicus, painful on pressure, and preventing him from holding himself erect. This pain has increased up to time of admission. He has had no trouble with micturition. His temperature on admission was 100°.

On examination, there was a hard mass below the umbilicus, apparently superficial, dull to percussion, the dulness being continuous to the pubes. There was great tenderness to the touch, and both recti were on guard. The catheter was passed, and a few ounces of urine drawn off without causing any diminution in the size of the tumour. The bowels were thoroughly emptied by enemata, and the rectum examined, but nothing new was detected.

On the 22nd the umbilicus was noticed to be protruding, and becoming retracted in the centre, and there was also a sensation of fluctuation, but this was not well marked. Since admission his bowels have acted freely every day after aperients. He has slept fairly well, but the temperature has been not less than 100° all the time, and he has required hot foments to the abdomen to ease the pain. The majority of those who saw the case thought it was a parietal hydatid cyst beginning to suppurate.

On the 23rd I made a free incision over the tumour, passing to one side of the umbilicus, and opened up a large cavity in the abdominal parietes. Just below the umbilicus I could pass my finger from the cavity into the umbilicus, causing it to protrude. The contents of the cavity were granular grumous-looking substance, not unlike omental tissue in some parts. The cavity was thoroughly cleansed and well scraped, and no connection was to be found with the peritoneum. A large drain tube (rubber) was inserted. After the operation the discharge continued for a fortnight, gradually lessening as the cavity contracted and healed up. He went home quite well on the 6th January, 1899. The discharge and some scrapings from the wall of the cavity were examined by Mr. Treloar (honorary bacteriologist to the hospital) with the following result:—No hooklets; no scolices; no tubercular *débris;* pus cells mixed up with columnar epithelial cells, and unstriped muscle tissue *débris* were found. I should here say that the amount of cyst contents from eye measurement (for the actual measurement was omitted) would be about fifteen fluid ounces.

Bland Sutton says: "Allantoic cysts arise from dilatation of a urachus which is occluded at the umbilicus and at the summit of the bladder. In the adult the urachus lies in the subperitoneal tissue exactly in the middle line of the anterior abdominal wall between the summit of the bladder and the umbilicus. When the urachus becomes dilated it forms a cyst lying outside the peritoneum and in close relation to the bladder. Such cysts are usually the size of a ripe cherry. In rarer cases the urachus may dilate into a cyst as large as a distended bladder. The late Lawson Tait recorded a case in which the cyst had a capacity of ten (10) pints. The structure of these large cysts is identical with that of the bladder, and consists of unstriped muscle fibre lined on the inner side with epithelium similar to that covering the vesical mucous membrane. Allantoic cysts are found in many mammals, such as the pig, horse, ox, mole," &c.

Greig Smith says: "Small cysts of urachus containing a few drachms of fluid are sometimes found at abdominal operations. They are exactly median, rise a little higher than ovarian tumours of the same size, have no connection with the uterus, and if not large may leave a resonant area between the lower margin and the pubes. They fluctuate freely in all directions, and sometimes there is a history of urinary trouble."

In reference to treatment Sutton says that "at present so little is known about the cysts that it is impossible to decide as to the best method of dealing with them."

The treatment adopted by me in the case recorded resulted satisfactorily, and I should be inclined to follow it out again should it be my fate to meet with another similar case.

A SUCCESSFUL CASE OF DIRECT INJECTION OF STRYCHNINE INTO THE WALL OF THE LEFT VENTRICLE OF THE HEART OF A PATIENT APPARENTLY MORIBUND DURING ANÆSTHESIA.

By R. D. Pinnock, M.D., C.M., Univ. Glasgow,
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On the 27th July, 1899, whilst operating on a Chinaman, æt. fifty-seven, for recurrent sarcoma of the glands of the left side of the neck, it was observed that the patient took the anæsthetic (chloroform) badly from the first half-dozen inhalations, becoming rapidly cyanosed. The respiration failed on two occasions during the early portion of the operation, and therefore the chloroform was changed for A C.E., which was being administered at the time of the third failure. On this occasion the action of the heart also failed. Artificial respiration was practised for over twenty minutes. Two separate hypodermic injections of $\frac{1}{30}$ gr. of strychnine in each, and one of m. 20 of ether, were administered during this period. He was also treated by rapid intermittent pressure over the heart. At the end of the period named, as there was no visible or palpable pulsation in the heart or in any of the arteries, it was considered by the five members of the honorary staff present that it was useless to continue the artificial respiration any longer, the patient being regarded as dead. I then, as a last resource, injected the wall of the left ventricle of the heart at the ordinary point of visible pulsation between the fifth and sixth ribs, with ten minims of solution of strychnia, equal to $\frac{1}{15}$ gr. The effect was most striking as well as most gratifying. In seven to eight seconds pulsation could be felt in the femoral artery, and almost simultaneously the patient gave a convulsive gasp, and in a few minutes more, under renewed artificial respiration, he began to breathe. The remaining portion of one of the affected glands was then removed as rapidly as possible, and the edges of the wound united, and patient put to bed. The wound healed by first intention, and he had no further bad symptom. A large portion of the sarcomatous glands was necessarily left untouched, so that the result will ultimately be death. When I saw him about ten days ago, he was well and cheerful, and only complaining of a very stiff neck. This case seemed to me worthy of bringing under the notice of those assembled here, for, although I had previously read of acupuncture of the cardiac wall being advocated as the most powerful stimulant to its contractile efforts, I do not remember any case having been recorded where strychnine was either suggested or used. It may reasonably be argued that the result in the above case was due to the acupuncture, minus the strychnine; and to those who prefer this view, it may with equal reason be pointed out that the result clearly proves that $\frac{1}{15}$ gr. of strychnia can be injected into the wall of the heart without doing harm. Having proved this, I should, if necessity arose on any future occasion, again inject the strychnia, on the principle of having two strings to one's bow.

NOTES OF A CASE OF "TRAUMATIC SEPARATION OF THE LOWER EPIPHYSIS OF THE TIBIA."

By Robert Scott, M.D.,
Hon. Surgeon, Ballarat Hospital.

The clinical notes of the case were kindly supplied to me by Mr. A. G. McGowan, M.B., the resident medical officer.

J.A., *ætat.* sixteen, was admitted to the Ballarat Hospital on the 13th October, 1898.

Whilst wrestling, the patient, catching his toe in a hole, fell backwards with his foot and leg doubled under him. From his description, he fell with the foot and ankle joint in the position of extreme plantar-flexion. On examination, a marked deformity presents on the anterior surface of the ankle joint. The deformity is hard, and apparently bony, with a sharp projection at the outer extremity. The outlines of the ankle are obscured by considerable effusion, which patient states came on at once after the accident. The relative lengths of the malleoli were normal, and no irregularity could be detected about them. No true crepitus, but pseudo crepitus, can be obtained by making extension on the foot. The astragalus is not displaced, and patient can flex and extend his ankle, but not to the full extent. He does not complain of severe pain on handling the ankle. The foot is held in the mesial line in the position midway between complete flexion and extension. Tendo Achillis is tense. Deformity does not disappear on making extension on the foot. Put up in Cline's splints between sandbags.

October 14.—Swelling less. Back splints and foments. Skin ecchymosed.

October 16.—Swelling much decreased. Sharp ridge running across the front of the ankle joint, just above the level of the malleoli, easily made out. Bony point is felt at the outer extremity of this ridge. No lower fragment palpable. Skiagram shows this ridge to be the anterior border of the lower end of the diaphysis of the tibia, the epiphysis of the tibia being displaced *backwards*.

October 19.—Still swelling about the ankle. No visible deformity, but the tibial ridge still evident on palpation. Forcible extension and manipulation brought the parts into better apposition. Patient complains of no pain. Has had evening temperatures of from 99 degrees to 100 degrees for the last four evenings. Tongue clean, bowels open. No rigors nor sweatings.

November 8.—Splint removed. No marked deformity. Skin still ecchymosed in places. No pain on manipulation about the seat of injury. Good union. Put up in starch bandages.

Discharged, 11th November, 1898.

Patient came to the hospital a fortnight later, and bandages were removed. Perfectly firm union. Can put his weight on the leg without discomfort. Massage advised to regain complete use of the joint.

The patient was seen by me a few weeks ago, and presented no deformity whatever, and the measurements as compared with the other leg were normal.

On looking up the literature on this subject, one cannot but be struck by the small amount of space devoted to disjunctions of the epiphysis in all the surgical works. Ashhurst quotes only two cases of this particular injury, and other surgical authors dismiss it in a few lines, so that, until the publication of Poland's work on "Traumatic Separation of the Epiphyses," but little information could be gained from existing literature.

R. W. Smith (*B.M.J.*, 1867) states that this is among the rarest of this class of epiphysial injuries.

J. Hutchinson, junior, has collected seventeen cases.

Age.—Poland gives the period between eleven and eighteen years as the time in which this separation is most likely to occur.

Sex.—The injury mostly occurs in boys; only two cases have been recorded of simple separation in the female sex among forty-six cases, and these both occurred at ten years of age, and were associated with solution of continuity of tho fibula.

Cause.—Indirect violence is the most common cause of this injury; usually it is caused, as in this case, by the foot being forcibly doubled backwards while in a flexed position. Direct violence has caused this injury in a few recorded cases; in two cases by the passage of a cartwheel over the ankle, and in one case by the fall of some blocks of wood on the leg—the foot being turned outwards (Poland).

Diagnosis.—In some cases where the foot is displaced outwards or backwards diagnosis is easy. Swelling probably is present almost immediately after the accident. Crepitus, as in this case, is usually absent. Mobility is a more reliable sign, being about 1 inch above the flexure of the ankle joint. Deformity is the most prominent sign—Poland states that in all the four recorded cases of simple separation the displacement of the foot with the tibial epiphysis was backwards — and so it was in this case. The internal malleolus preserves its relation to the foot, but not with the rest of the leg nor the external malleolus. Separation of the epiphysis will allow but slight displacement of the fragments indicated by some amount of eversion of the foot, unless the fibula be fractured or its lower epiphysis be separated. In cases where separation occurs without deformity (as in this case) the injury might be taken for dislocation of the ankle. Crepitus (if present), mobility of the ankle, and the unaltered relations of the internal malleolus to the foot, together with the shortened distance from the projecting end of the diaphysis to the great toe, would be the points to aid in arriving at a correct diagnosis; Pott's fracture, dislocation of the astragalus, and subastragaloid dislocation could hardly be mistaken for this injury. The crepitus in the one case and the altered relation of the bony prominences to the foot, with fixation of the ankle joint in the other cases, would help in distinguishing these injuries.

Prognosis.—The prognosis is favourable. In cases where the injury is seen early and reduction effected, as a rule the growth of the bone is not seriously retarded. Even in compound separation good results have been obtained, little or no deformity resulting and the ankle joint movements being perfect.

Treatment.—Poland recommends the replacement of the displaced bone by extension and counter-extension, and then applying a starch or Croft's plaster of paris splint. In my case a back and side splints were found most serviceable for the first three weeks until all swelling had subsided, after which an ambulatory starch bandage was applied.

In compound separations, if necessary, resection of the diaphysis should be performed when it is impossible to reduce the deformity. Amputation, even when most extensive injuries to the soft parts are present, should only rarely be resorted to.

My reason for bringing this uncomplicated case before you is on account of the extreme rarity of such injuries, and also as showing the great value of the Röntgen rays as an aid to diagnosis.

The two skiagrams [exhibited] were taken by Mr. T. R. Treloar, hon. demonstrator of Röntgen rays to the Ballarat Hospital. The one was taken two days after the injury, and the other about two months ago, or nine months subsequent to the accident.

ANATOMY AND TREATMENT OF CONGENITAL TALIPES EQUINOVARUS.

By W. Kent Hughes, M.B. Lond., M.R.C.S. Eng.,
Melbourne.

With respect to the bones, we have to consider deviation from the normal both as regards displacement and distortion. All the bones in front of the medio-tarsal joint are displaced inwards, and more or less rotated.

The os calcis and astragalus are not only displaced, but are also distorted. This is an important point to notice, for while we can restore a displaced bone to its proper position by attending to the soft structures which are retaining it, a distorted bone that is displaced must be itself operated upon before it can be induced to resume its normal function.

The scaphoid, the cuboid, and the cuneiform bones pretty closely resemble the normal. In severe cases they are stunted, and the cuboid and scaphoid have new facets.

The astragalus and os calcis, however, show marked deformity. Taking, first, the astragalus in a severe case :—

Astragalus.—Some force has been exerted upon the anterior portion of the astragalus in a downward and inward direction. The effect of this is to depress and deflect the anterior portion and bring the posterior part of the trochlear surface in contact with the articular surface of the tibia. In advanced cases, this movement being increased, the trochlear surface is shifted posteriorly till it reaches the posterior edge of the upper surface, and is even continued on to the os calcis. Owing to the continuous pressure to which it is subjected, the depth of the posterior portion of the bone is much diminished.

The portion of the bone in front of the ankle-joint is deflected downwards and inwards, the outer surface of the neck is lengthened, and the inner shortened, whereas in normal bones the two edges or surfaces are equal or almost so; in talipes the outer is generally about double the length of the inner.

The inward obliquity was first brought forward prominently by Mr. Adams ("Club-foot," p. 135), in 1866, and was further worked out by Mr. R. W. Parker, in 1887 ("Club-foot," p. 15).

Mr. Parker fixed the mean angle of inward obliquity of normal adult astragali at 10·65°, Scudder at 12·32°, and we found it to be 11·5°, so we may take 11·49° to be a fair average, although I should not be surprised if the angle was found to be smaller, as in several instances it was almost impossible to measure it. In normal fœtal astragali, the mean angle is about 39°. In talipedic astragali (adult and children) the angle may be from 50° to 60°; it may be as large as 68°.

We will now direct our attention to the declination of the neck. This fact has been strangely overlooked by writers on talipes, and Mr. Walsham and I were the first, I believe, to bring it prominently forward It is to be found in all cases of talipes, and is more marked than the inward obliquity, as a reference to the table will prove. A portion of this declination may be apparently due to the defective development of the head, but by far the greater portion is "real" and due to downward pressure and development of the bone in this position :—

Bone.	Inward Inclination of Neck.	Downward Inclination of Neck.	Outer Edge of Neck.	Inner Edge of Neck.
Normal, fœtal	39°	0°	$\frac{1}{3}$ inch	$\frac{1}{4}$ inch
Normal, adult	11·49°	0°	1 ,,	$\frac{13}{16}$,,
Talipedic, adult (severe)	48°	57°	$1\frac{1}{4}$,,	$\frac{1}{2}$,,
,, child	18°	49°	$1\frac{1}{8}$,,	1 ,,
,, child	19°	28°	$\frac{13}{16}$,,	$\frac{6}{16}$,,
,, fœtal	·29°	69°	$\frac{5}{12}$,,	$\frac{5}{24}$,,
,, child	21°	41°	1 ,,	$\frac{1}{2}$,,
,, child	24°	45°	$1\frac{3}{8}$,,	$\frac{3}{8}$,,
,, child	31°	28°	$1\frac{1}{4}$,,	$\frac{5}{8}$,,
,, child	31°	54°	1 ,,	$\frac{1}{2}$,,

The obliquity of the neck in congenital talipes eq. V. is in marked contrast to the conditions found in paralytic cases. Even in those cases where the paralysis dates from infancy, there is hardly any deviation from the normal, even though the deformity may be of the most marked character. This fact should impress upon us the necessity for beginning treatment before ossification makes much headway, as it shows us how resistent the astragalus will soon become.

The relations of the astragalus to other bones are somewhat altered. The scaphoid is drawn inwards on to the inner edge of the neck, and the os calcis anteriorly is bent inwards, so as to lie vertically underneath the astragalus instead of being to its outer side.

Os Calcis.—In a severe case, the os calcis is very complexly distorted. The posterior portion is twisted upwards and inwards upon its horizontal axis, and the interior slightly downwards and inwards, so that the external surface is convex, and the inner strongly concave. The posterior portion of the bone is, as a rule, ill-developed. In a normal bone, the distance from the superior edge of the posterior surface to the posterior articular facet for the astragalus occupies about one-third of the whole bone; in a talipedic fœtal bone, the articular facet runs quite close up to the posterior surface. In the fœtus, it has not developed much of that oblique twist so marked in older bones. In position it is almost vertical, forming a marked contrast to the position of the boue in paralytic talipes equinus, as Mr. Adams pointed out. The articular facets, as in the astragalus, show a gradual displacement in a series of cases. The anterior facet for the astragalus is generally absent, or blended with the posterior facet; the long axis of the posterior facet in a normal bone is directed from behind forwards and outwards; this direction is altered gradually to forward—forward and inward; and lastly, in the most severe cases, it is almost transverse, encroaching upon the posterior part of the sustentaculum tali, and the upper portion of the inner surface of the bone. The facet for the cuboid, likewise, is transferred from the anterior surface to the inner, and a new facet may be found in severe cases upon the anterior part of the sustentaculum tali for the original internal postero-inferior angle of the cuboid.

A new articular facet may be occasionally found upon the outer surface of the bone for the tip of the external malleolus—a fact to be remembered in dealing with the re-position of the os calcis.

The tendons and ligaments may be shortly described as lengthened on the outer and contracted on the inner side of the foot, not forgetting the contracted tendo Achillis and the plantar fascia.

I have dwelt at some length upon the anatomy, because I consider that much of the treatment that has been from time to time suggested, and is, as a rule, practised, is based upon an imperfect conception of the more important points of the anatomy of the deformity. The numerous bone operations (over seventy) that have been devised for the relief of the deformity, and the countless ingenious appliances that have been invented, are themselves more than suggestive of the truth of my remark.

There is no doubt that there are but few cases that cannot be cured by manipulation and plaster of paris if sent early enough for treatment ; but that is the difficulty. The general opinion in Australia is that nothing can be done till the child is six months of age and older, but I would earnestly impress upon all the necessity for placing the patient under treatment at once. "As soon as the baby is washed," said Sayre, when asked how soon he would commence. Lately I had under my care at the Children's Hospital, Melbourne, a most severe case of double talipes eq. V. in a baby one day old with a sloughing spina bifida. In less than a fortnight the feet were absolutely normal ; it is astonishing how quickly the feet came round. This is the first case that I have been able to treat under three weeks' old, and the difference in the density of the bones at the latter age is most marked.

To perform the manipulations properly, taking a left foot for example, the surgeon should press the heel outwards with the left hand ; grasp the anterior portion of the foot between the thumb and fingers of the right hand, the thumb being uppermost and to the inner side, and press upon the neck of the astragalus and the anterior portion of the os calcis with the tips of the fingers. It is difficult to give definite rules for manipulations, for both children's feet and surgeon's hands differ so very much that directions that are suitable to one person are equally unsuitable for another. It should be remembered, however, that the anterior portion of the foot is to be everted upon the posterior, that pressure is to be exerted upon the outer surface of the neck of the astragalus, and in our efforts we must endeavour to carry the scaphoid and head of astragalus outwards, and also make play upon the cuboid in a direction upwards and outwards. In some instances a good deal of force must be used, and in such it is well to interpose a piece of cotton wool between the fingers and the neck of astragalus, as the skin of the infant may be easily damaged. After manipulating for a few minutes, a plaster of paris bandage should be applied, or a flexible metal splint. I myself infinitely prefer the plaster bandage, and have a horror of complicated mechanisms of any kind. They are always expensive and very often inefficient, and in none of them can you so well overcome both the equinus and varus at the same time as you can with plaster. Sayre long ago said that the hand was the best orthopedic instrument, and I shudder to read of the awful appliances and strong force exerted by some Continental and American surgeons upon little infants' feet. Before using plaster cover the leg and foot with a thick layer of cotton wool. It is best used in the form of a bandage.

In applying the plaster bandage from without inwards, bringing the foot well round as you pass over the dorsal surface to the plantar and up to the leg again ; this, as Mr. Willett pointed out, is a most important point, and should never be neglected. Take care to keep the bandage flat so as to avoid creases. After the leg and foot have been covered with double layer of plaster bandage, manipulate the foot again as above for a little while and continue the bandage again. The foot should be held in the corrected position until the plaster sets. In order to delay setting, I always use cold water so as to obtain plenty of time for manipulation.

Treatment by manipulation and plaster has not received the attention it deserves, and if the case is taken in hand early enough I am convinced there are very few, if any, that could not be cured by its means alone. The treatment should be repeated twice a week. The objections to it are:—1. The danger of pressure sloughs. 2. Its tediousness. 3. Its inconveniences. The first objection holds good for all mechanical appliances, and plaster of paris is less liable to produce such than any other. The second objection ought to be no obstacle to a sincere surgeon. The third objection can be minimised if the surgeon instructs the mother to remove the bandages before leaving home, and takes care to " vaseline" his hands before applying the plaster.

After restoration, it is necessary that the child should wear a night shoe and a splint with an universal joint at the heel during the day until the child can walk, and then a boot, with restraining irons carried up to the waist if necessary, should be worn for one or two years. Attention to this point will prevent relapses and disappointments.

In most cases division of the tendo Achillis will be called for, and in a smaller number division of the astragalo-scaphoid capsule and tibialis posticus. After tenotomies, strong pressure should be used to overcome the obstructions as far as possible, and the foot put up in plaster of paris. I prefer to divide the tibialis posticus at its insertion at the same time that I divide the ligaments of the scaphoid and astragalus. If the plantar fascia needs division, divide far back about the level of the internal malleolus. I would caution you against early division of tendons and fascia. The main obstacles to restitution exist in the bones, and we should endeavour to rectify these by manipulation before using a tenotome. When it is considered necessary to divide tendons, &c., I divide all as above mentioned at one sitting.

Some surgeons delay dividing the tendo Achillis because it affords a " point d'appui" by which the varus can be overcome. Apart from the fact that the "varus" is always comparatively easy to overcome, division of the tendo Achillis does not render the foot "limp"; it only removes one of the "influences" that retain the foot in an equinus position, the principal factor being the deformed astragalus and os calcis.

The majority of the cases that have come under my notice since I have been in Melbourne have been too old to cure by such means as the above. In spite of a great weight of opinion to the contrary, I do not think that *often* we can produce a perfect result without a bone operation in children over six months. I grant you that you can produce straight feet by sufficiently dividing the soft parts, and by using enough force to wrench the foot round into position; but I hesitate to call such results "perfect." A plantigrade foot is far from perfect. The bones are still deformed, and a real cure is not effected.

If manipulation and plaster treatment fail, what courses are open to us—including under the above treatment subcutaneous division of the tendo Achillis and astragalo-scaphoid capsule with tibialis posticus tendon and plantar fascia? We have a large number of operations to choose from—some of which deal only with the soft parts and some practically only upon the bones, and lastly those that include both.

Operations upon the bones other than the os calcis and astragalus must be condemned, as I have shown you that no other bones are distorted, but are merely displaced. This reduces the number to be considered, and we have now to turn our attention to the astragalus and os calcis.

As regards the astragalus, the inward inclination of the neck and the lengthening of its outer surface suggest the removal of a wedge with its base outwards; this would overcome the inward inclination, and if the bone were thoroughly divided the downward deflection would, of course, be easily remedied by raising the head and articular surface to a level with the body of the astragalus.

The anterior end of the os calcis is so much bent inwards that a similar process seemed the natural one to overcome the defect. This would leave the scaphoid and cuboid in their old positions with the astragalus and os calcis, and it is better that we should do so; their articular surfaces correspond and are adapted to the requirements of the joints; whereas, if we disturbed their relative positions, the bones would have to form new articular surfaces—so it is far better to attack the os calcis and astragalus at some distance behind the displaced medio-tarsal joint, and leave the joint alone, though we replace it.

The operation which I have performed since 2nd March, 1894, is as follows: The foot, being placed on its plantar surface on a sandbag, an incision is made just in front of the ankle joint, on the upper surface of the neck of the astragalus. This is continued downwards with a slight curve below the level of the external malleolus; the anterior incision commences at the same point above, and is then continued downwards with a slight curve to meet the posterior incision. From this point a single incision is carried on almost to the plantar surface. The amount of skin removed must depend upon the case in question. After removal of the oval piece of skin, dissect out the peroneus brevis and longus tendons. They are easily found just below the level of the tip of the external malleolus. Pass a stout silk ligature round them both.

Crossing the upper part of the wound there will be generally seen the external branch of the musculo-cutaneous nerve and the outermost tendon of the long extensor. Pass a ligature under these and upwards through the skin on either side; these can be used for retraction at the same time that the nerve is guarded from injury. Cut down upon the neck of the astragalus in the direction of its long axis; separate the periosteum carefully for some distance, being careful not to disturb it too far anteriorly, and pass well over to the inner surface of the bone. I consider it important to preserve the upper part of the periosteum intact, as it is a considerable factor in the firm and rapid union of the bone; below it must be sacrificed.

When the neck is well exposed pass the chisel (choosing one about the depth of the neck) from without in a direction inwards and forwards, commencing as near to the ankle joint as possible—in young children this can be done without a mallet, but in any case a finger should be kept on the inner side of the foot to give warning of the position of the chisel. The anterior incision should not be made nearer the head than necessary, for the anterior portion of the bone may be so loose that there is a danger of losing it altogether—from without the direction of the cut should be backwards and inwards to leave as large a surface as possible to be opposed to the raw surface of the posterior portion. Carefully remove the wedge and see that the bone is thoroughly divided through the whole depth. Expose then the upper part of the os calcis, and get an assistant to pull down and out the peroneal tendons, and with a chisel commence the incisions in the upper part of the os calcis—the posterior incision corresponding with that of the astragalus—the anterior being situated more forward, and the direction being more obliquely backwards. Pulling the tendons upwards, incise the periosteum at the lower edge of the os calcis, and lift it off the outer and under surfaces of the bone with an elevator, leaving the latter in position to guard the vessels, and with a chisel incise the lower portion of the bone. To completely divide the plantar surface is the only difficult part of the operation, and care must be taken not to divide the external plantar artery. If the vessel is injured, divide completely, and arrest hæmorrhage by stuffing with sterilised gauze temporarily. Having removed a wedge from both bones of sufficient width, which matter can be determined roughly by attempting to replace the foot, proceed to the division of the astragalo-scaphoid capsule, dividing it subcutaneously according to Parker's method (which also includes the tendon of the tibialis posticus) as well as the tendo Achillis and the plantar fascia. The plantar fascia should be divided as far back as the level of the lower end of the malleolus, thus avoiding the implication of the cutaneous nerves in the scar tissue; and I prefer to divide everything between the skin and the bone—that is to say, the plantar fascia, the flexor brevis, and the abductor hallucis.

If the foot cannot now be over-corrected, remove more bone posteriorly to the cut surface till the result is quite satisfactory. I have occasionally stopped short of over-correction, but would urge you not to do so, as the case will require much more after-treatment, and will not always give a perfect result. The point where resistance is most likely to be found is generally at the junction of the two bones owing to imperfect removal of wedge. When the operation is finished, the foot should be put up in over-corrected position on a back splint, with plenty of dressings, as the oozing is sometimes considerable. At first I used to put the foot up in plaster, but, acting upon Dr. Clubbe's advice, I use the splint first, and find it a much better plan. I prefer to operate without an Esmarch's bandage. I carefully arrest hæmorrhage at every step, and although there are no important vessels severed, yet there are many small ones which ought to be secured.

It is as well to pack the space between the divided bone with gauze directly the wedge is removed, in order to avoid infection and to arrest hæmorrhage. After thorough irrigation with sterilised water, the wound should be completely closed, and the dressings left undisturbed for at least three days. The temperature may rise to 100° Fah., or even 101°, without giving just cause for alarm; the toes will generally give warning of any mischief. If, however, the temperature keeps up through the second day and remains up, it is as well to dress it. Sometimes superficial death of the skin distal to the incision has been the cause owing to deprivation of blood supply.

I dress the case on the third or fourth day and put the foot up in plaster, over-correcting still more, but, however good the result, I think it is better to change the plaster twice a week, as the tissues have an inherent tendency towards equino-varus, and the plaster bandage yields, and thus the good effect runs a chance of being minimised, and also if the foot be not constantly moved the astragalus and os calcis may unite. I found this to occur in one of my earlier cases where I used to leave them in plaster for a fortnight at a time. I do not leave off plaster till I am satisfied that I have overcome this tendency: it may be a few weeks or it may not be for a month or two. I cannot impress upon you too strongly the necessity for assiduous attention to after-treatment. When plaster is left off I order the patient boots with steel supports above knee, the outer iron being placed in advance of the inner to over-correct any attempt at inversion. At night the patient should wear a right-angled night shoe; these should be worn for at least a year. In some instances the fibula will give rise to trouble either from being rotated inwards, as Scarpa first pointed out, or from articulation of its lower end with the os calcis preventing proper replacement of the os calcis In the latter instance it is advisable occasionally to excise the lower end of the malleolus carefully—for the former twist, transverse division of the bone will be called for, and occasionally a similar division of the tibia.

In only two cases have I had any serious trouble with the skin on the inner side—viz , in one (*vide* photo. of W.K., æt. nineteen) after removal of the wedge I found the skin so tight that reposition was impossible without incising the skin. His great toe had to sweep over an arc of 10 inches, so after operating as above I incised the inner border of the foot over the region of the first tarso-metatarsal joint, cutting all ligaments and muscles that offered any obstruction when the foot was replaced. The incision became a gap of nearly 2½ inches; this I left to granulate up and to graft at a subsequent date. The result is excellent, but there is not good movement at the ankle joint—there was but little there before the operation. He had a large slough at the under surface of his great toe, and two smaller ones on the dorsal surfaces of the fourth and fifth. This happened in two other cases, and must be due to some trophic lesion from division of the nerves. There was no question of pressure. I have performed the operation 37 times during the past five and a-half years with the following results to date: 28 perfect, 3 good, 4 moderate, and

1 death, and one case which I operated on at the Children's Hospital, when Dr. Lendon very kindly gave up one of his beds to me; the result after the operation was very good, though I do not know the state of the foot now. The death occurred in a boy of seven, on the third day, from, I suppose, acute septic poisoning. I had previously operated with success on the other foot. I prepared the foot and all the dressings most carefully myself, as I imagined, and my assistants were very careful also, but somehow the septic matter was introduced—the wound looked well, and there was never a sign of pus.

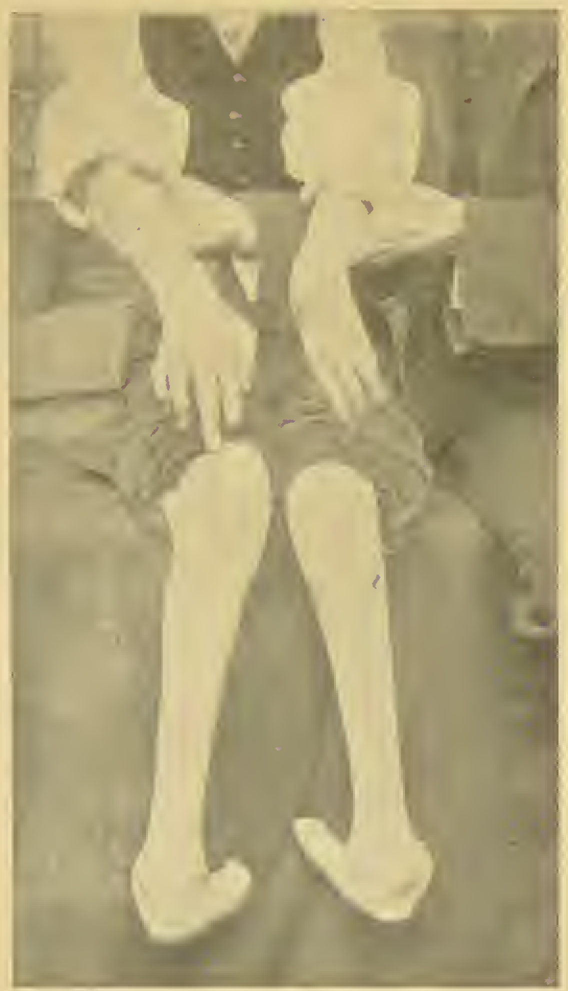
Of the three marked good (*a*) there was a local death of skin and subcutaneous tissue, and the head of the astragalus and anterior portion of os calcis were eventually lost. (*b*) Very extreme case of W.K.; a perfect result could not be expected: that is to say, a foot with perfect movement at ankle-joint. (*c*) Jas. K., æt. fifteen months, developed bronchitis and chickenpox; sixteen days after operation a collection of pus found at back of heel in position of poxmark; cellulitis of foot followed, and for a time the case looked serious. Nearly two years after operated on again with very good result.

Of the four marked moderate (*a*) is the case of F.C., æt. two years and five months; tarsectomies had been twice performed by other surgeons, and attendance was most irregular; finally treatment was refused. (*b*) E.M., æt. sixteen, had previously undergone several crushing operations and tarsectomies, and in consequence the foot was much "knocked about." (*c*) E.A., eight years: After operation he ran a high temperature for several weeks, reaching as high as 105°. There was no sepsis apparently, and anti-streptococcic serum had no effect upon the temperature. The foot, eighteen months afterwards, is still discharging slightly through two small sinuses. I have cut down and scraped the wound once. Very small pieces of bone keep coming away at intervals. There is no definite history of tuberculosis, but the mother has the scar of a tubercular gland in her neck, and the boy ran a very anomalous chart when he had typhoid fever, and was finally sent away to the country with a temperature of 102°. The position of the foot is fair. (Healed nine months later.) (*d*) J.W., fifteen months of age, after operation was taken to a very dirty house in Carlton, and wound became septic. Cellulitis of foot set in, which spread up to knee. It was well incised, and healed well except for a slough on inner side of foot. The case passed out of my hands at this stage, and the wound was allowed to contract. It would have been an excellent result if a graft had been put in. Patient was syphilitic, and died a year afterwards.

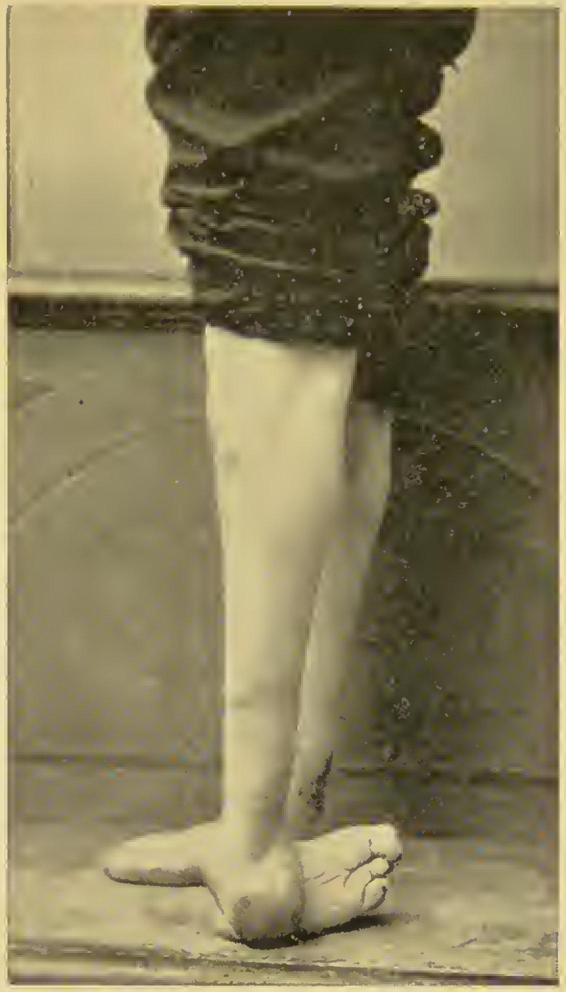
The two operations most generally performed in Europe and the States are excision of the astragalus, and Phelps'. Excision of the astragalus is a confession of failure, and does not alter the deformed os calcis and the very much displaced cuboid, and in my opinion is a sorry refuge.

Phelps' operation is generally understood to be an heroic incision on the inner side halfway across the foot, vigorously forcing the foot round at each step—that was his first method. He later advocated in some cases an incision through the neck of the astragalus, and later the removal of a wedge from the os calcis, and failing replacement he attacked the cuboid and scaphoid.

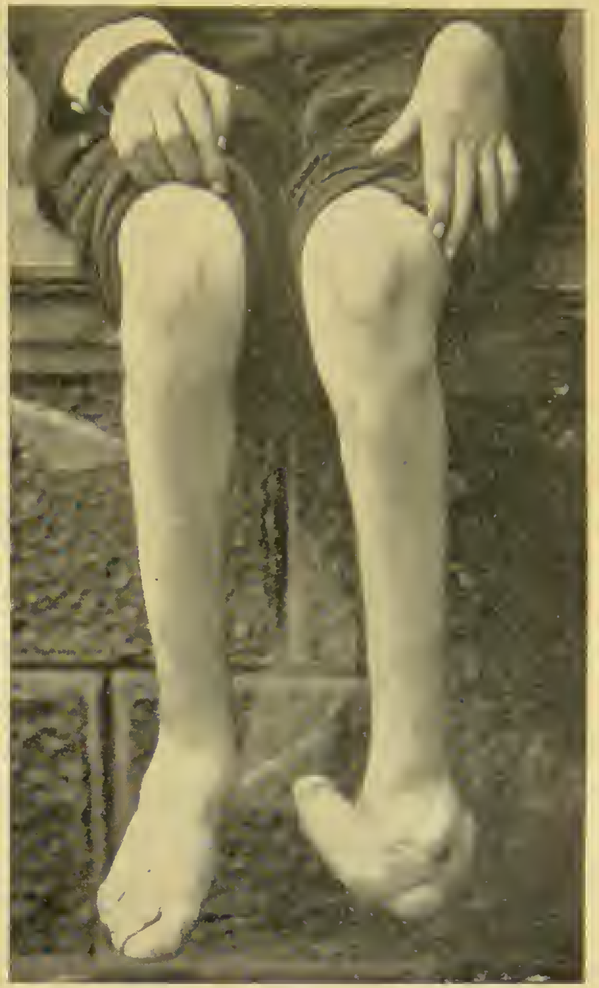
Cases that could be cured by the open incision could be equally well cured by careful subcutaneous division of the resisting structures, and linear osteotomy of the astragalus is quite, in my opinion, insufficient to get really rid of the deformity in the neck. Little, *British Medical Journal*, 19th October, 1895, advocated the removal of a wedge base upwards from the astragalus and calcis, after the varus had been overcome by slow orthopedic instrumentation; whereas both varus and equinus can be speedily overcome by my operation at one sitting.



W.K., aged 19 years—Before Operation.

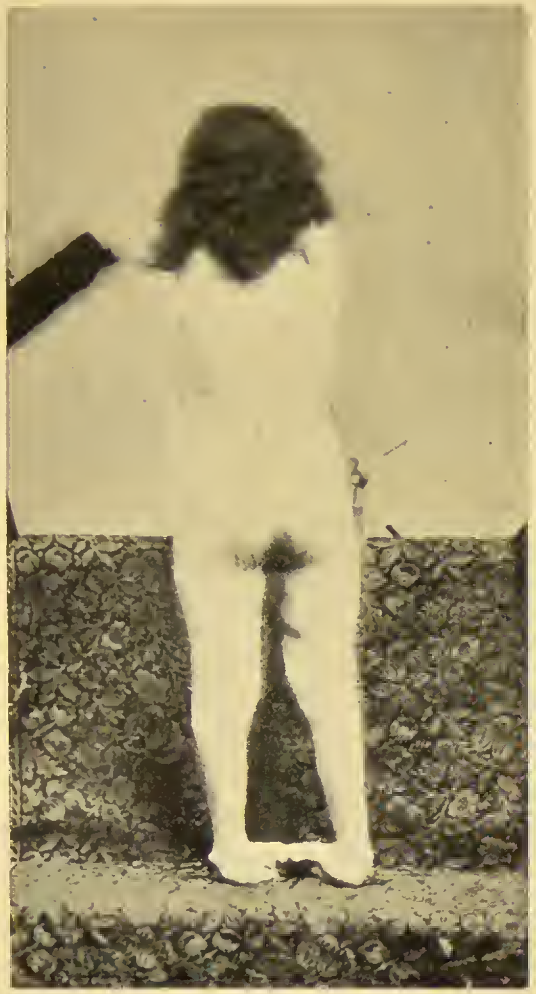


W.K., Side View—One Year after Operation.

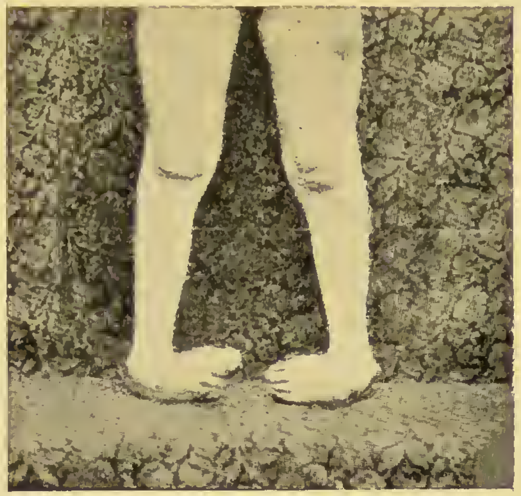


W.K., Front View—One Year after Operation.

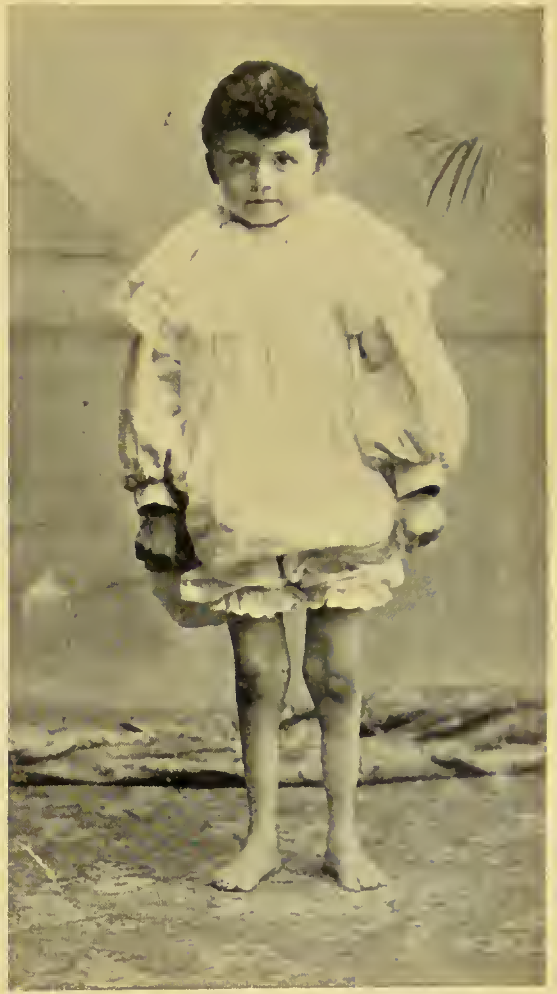
To illustrate Dr. Kent Hughes' Paper on Congenital Talipes Equino-varus.



M.A., aged 2½ years—Before Operation.

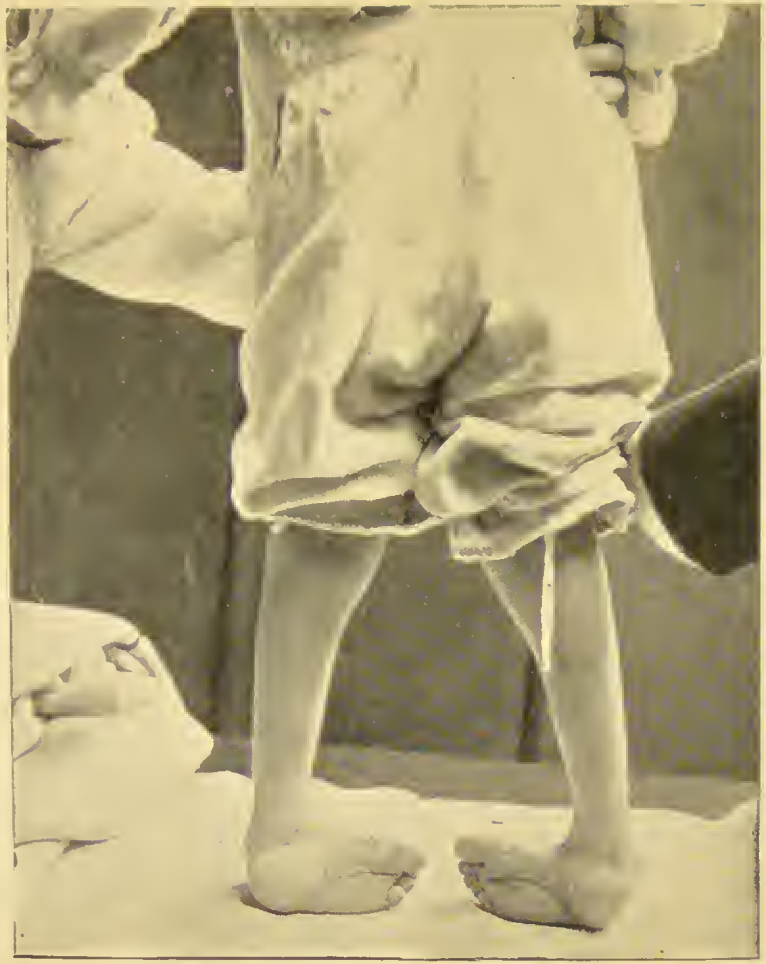


M.A., Front View—Before Operation.

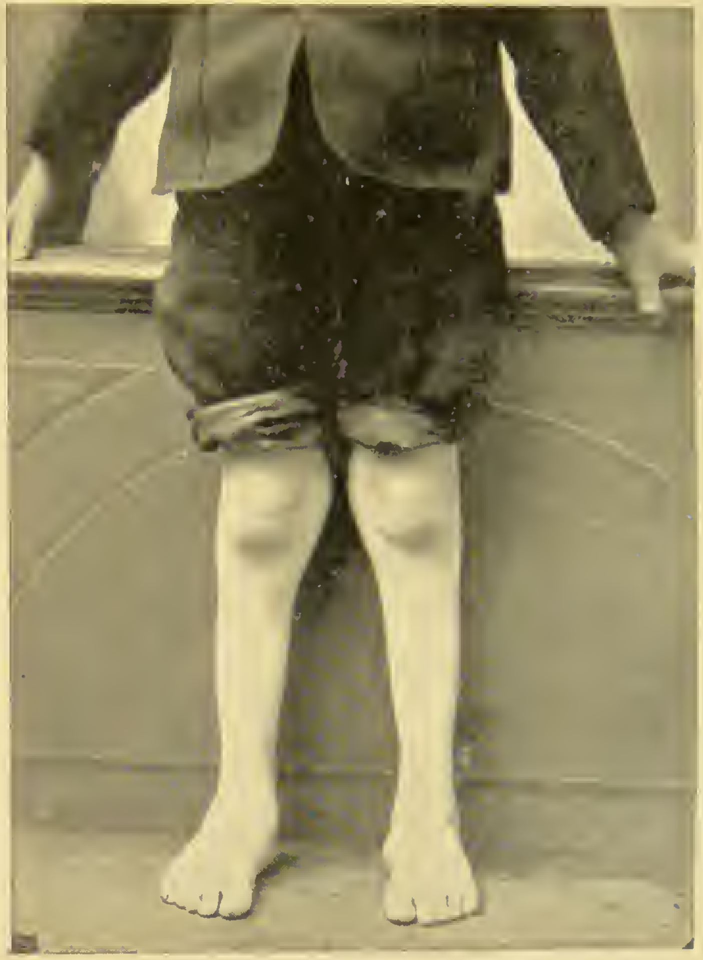


M.A., One Year after Operation.

To illustrate Dr. Kent Hughes' Paper on Congenital Talipes Equino-varus.

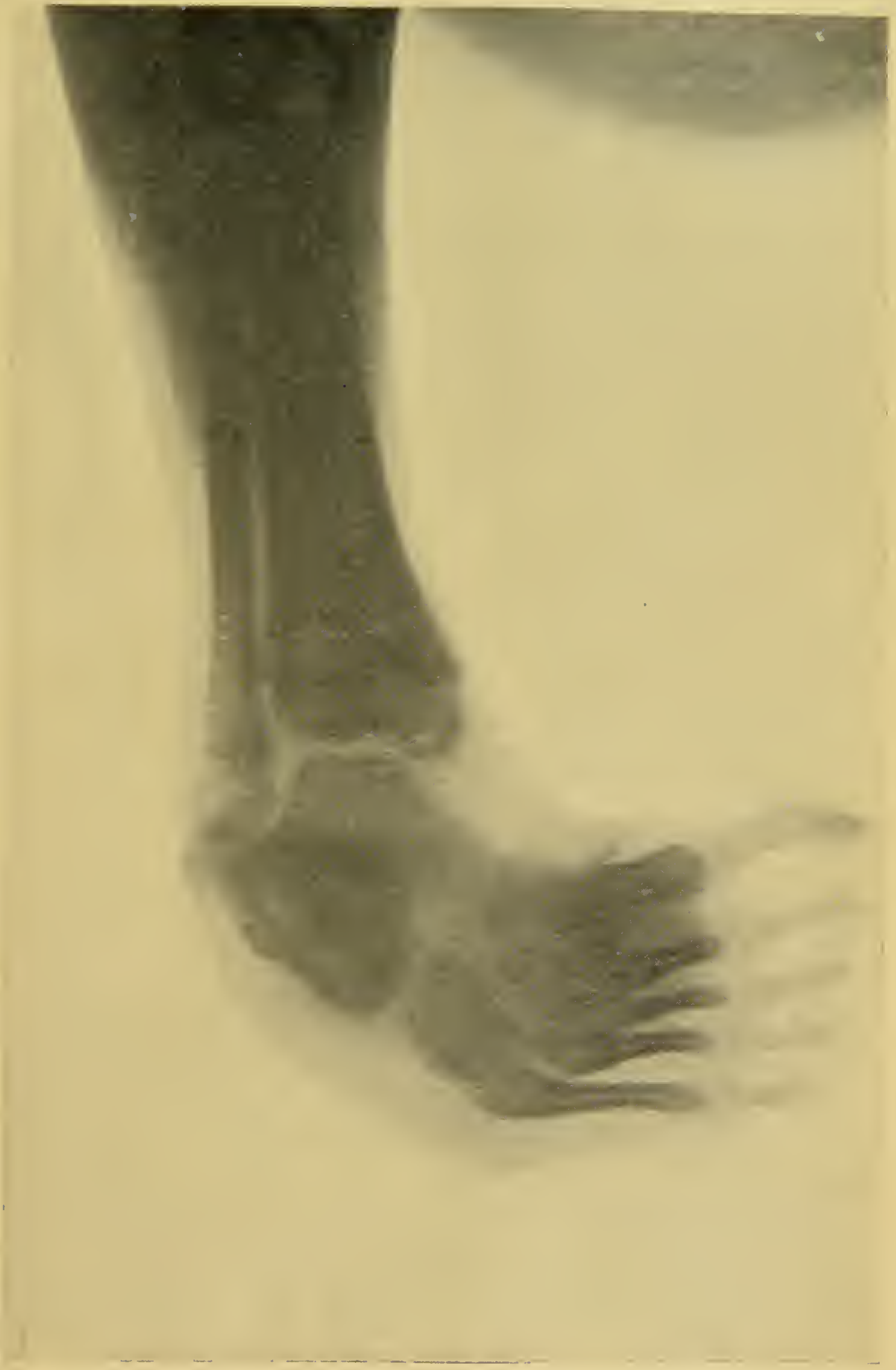


H.S., aged 3½ years—Before Operation.

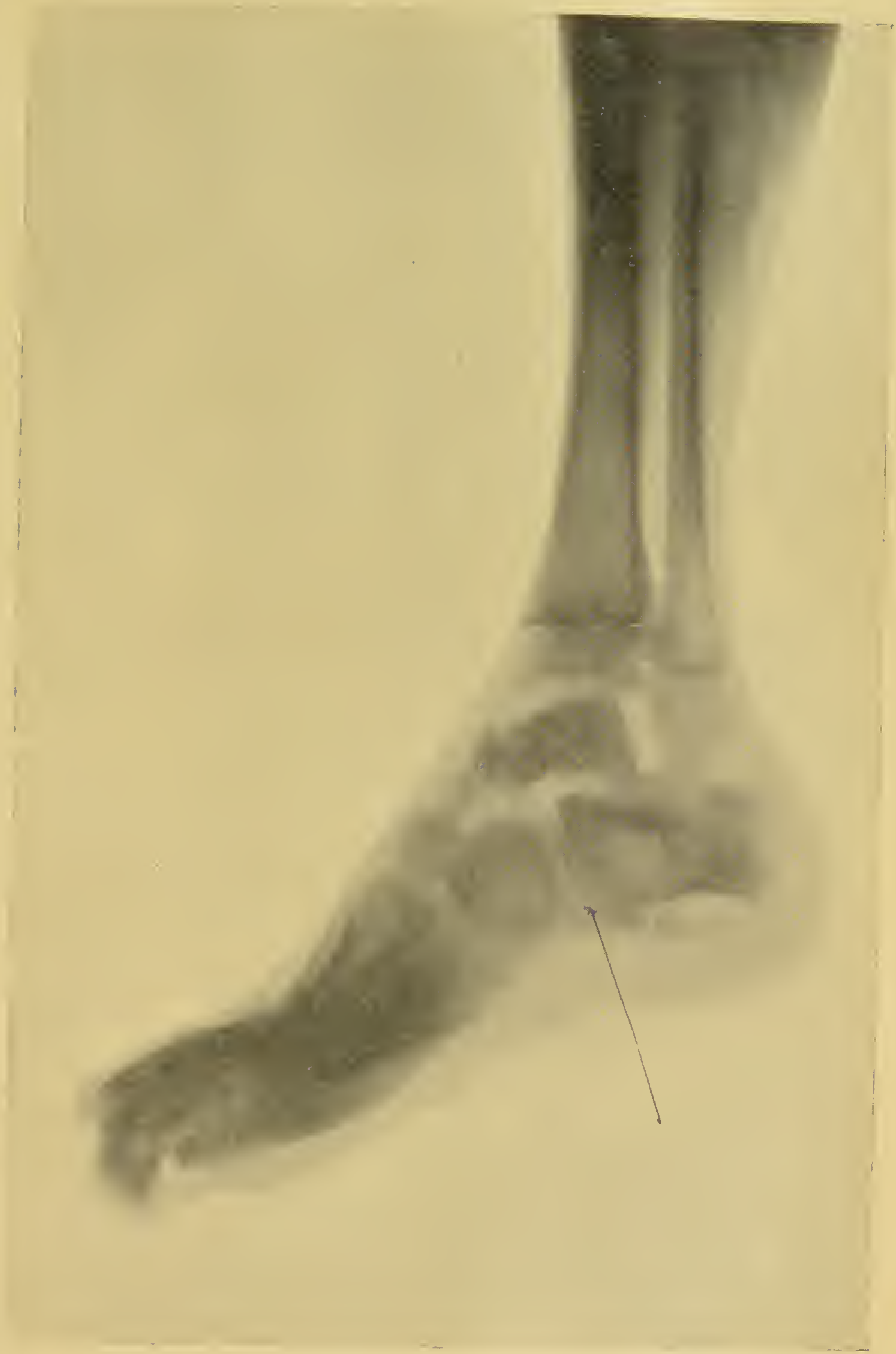


H.S.—Three Years after Operation.

To illustrate Dr. Kent Hughes' Paper on Congenital Talipes Equino-varus.



To illustrate Dr. Kent Hughes' Paper on Congenital Talipes Equino-varus.



To illustrate Dr. Kent Hughes' Paper on Congenital Talipes Equino-varus.

Dr. Bradford, in 1897, wrote an article in the transactions of the American Orthopedic Association advocating an almost similar method to the one I have just described, but he there mentions the employment of Phelps' open incision, and the forcing of the unfortunate scaphoid on to the rough non-articular part of the head of the astragalus. He also describes the anterior part of the os calcis as pointing outwards—overlooking the real nature of the distortion as it is twisted inwards. The incision he advocates is not so conveniently placed as mine, and it is certainly dangerous to remove the wedge so far forward that only the cartilaginous plate is left as he advocates.

A large number of the cases have been treated practically as out-patients, though occasionally, through the kindness and courtesy of Doctors Snowball and Cole, I have been given the use of a bed at the Children's Hospital.

I must also express my thanks to Dr. Clubbe, who during the last two and a-half years has helped me in many ways by suggestions and by his kindly appreciation of the value of the operation.

Finally, I would say that all cases should be treated as soon after birth as possible by manipulation and plaster; that if cure is not effected or promised within a month by these means, proceed to subcutaneous division of fibrous tissues and tendons above mentioned; that if the child is not cured when twelve months old, provided that it has been six months previously under treatment, proceed to tarsectomy as above advocated.

Dr. Clubbe said he had been brought in contact with such deformities for a great many years. He was very pleased with the operation which had been described by Dr. Hughes. He had practised the operation himself, and the twenty or thirty cases he had had turned out satisfactorily. In cases of such deformities, they must keep the patients under observation for a considerable length of time. He did not put the limbs in plaster at all for five or six weeks, but if at the end of that time he had to send them from the hospital, he put the limbs in plaster. He also made them wear a boot with a side-support for fully a year afterwards. If they followed the patients up, and impressed upon the parents the necessity of watching the cases, and that there was a tendency to relapse, then, and only then, would they get good results.

A CASE OF AN ODONTOME.

By D. R. Eden,
Brisbane.

The specimen of an odontome which I have the pleasure to submit for your consideration, being of somewhat rare occurrence, is interesting from the nature of its growth. It is similar to those described by Professor Broca in his "Traité des Tumeurs," 1864, under the head of "*Odontomes odontoplastique*," consisting as it does of irregular and alternate confused layers of enamel dentine and cementum. The name "odontome" is applied to those masses of dental tissues which result from morbid conditions of the formative pulp; and in order to understand the growth of these pathological conditions, Tomes says it is necessary to keep in view the normal process of tooth formation in the early stage. Represented by the submucous tissue (*mesoblastic*) and the other process of oral epithelium (*epiblastic*) which forms the enamel organ, this papilliform mass, assuming the dimensions and form of the tooth, becomes covered on its surface by a layer of cells known as *membrana eboris*, whose special office is the formation of dentine, which once formed is unalterable; and it is presumed that at the time of the origin of an odontome of this nature the dentine germ is covered by a layer of odontoblasts more or less completed, but dentine has not yet been formed; consequently, when the bulb has become the seat of an irregular growth, a mass is produced containing dentine, and perhaps also enamel, the enamel organ having followed, as it tends to do, the wanderings of the dentine germ; but no part of a tooth having been formed the mass may be a confused heap of dental tissues.

Again, "The product of the calcification of a dentine pulp is not always true dentine. So long as the layer of odontoblasts coats its surface, true dentine is produced; but this layer is easily destroyed, and any calcification which takes place after the destruction of the odontoblasts will assume the form of secondary dentine or of confused bone-like tissue."

Before describing the tumour, I may be permitted to say something of the history of the case. The patient had suffered considerable pain at intervals, and to give relief had the first molar and both bicuspids extracted at different times, the mass enlarging in the direction of these teeth as room was made for it. A surgeon had then been consulted, who, upon examination, concluded it was a tumour of such a nature that it would be necessary to excise part of the jaw adjoining the growth to obtain permanent relief; but being unwilling to undergo the operation, she again consulted the dentist who had taken out her teeth, and who, then observing a brown mass of hard substance growing above the gum, cut a piece off with a pair of bone forceps at different times; ultimately reversing the forceps, he broke the mass in two, one-half of which was loose, but which he was unable to remove. At this stage the patient was brought to me (under the impression that the mandible was fractured). The patient, a woman of about twenty-two years of age, had the left side of the face considerably swollen with two sinuses—one at the angle of jaw, the other in the region of the dental foramen—discharging a considerable quantity of pus.

Upon examining the mouth, a large irregular mass was exposed above the gum, the mucous membrane being congested round about it. Upon applying an instrument to it, it was movable, although it appeared jammed in its position as if it were dovetailed between two hard substances. Upon applying a sharp-pointed gouge to it, the instrument slipped upon it as if it were ivory or secondary dentine, and it was impossible to make an impression on the mass, and it struck me that it might be an odontome, more especially as, although the mass itself was slightly movable, the mandible was quite solid; so that I ventured to raise it with an elevator, and making a fulcrum of the cuspid with some force it suddenly gave way, revealing the larger half of the mass. An examination of it confirmed my diagnosis, and with a pair of forceps I removed the other half.

Upon examining the cavity, although highly congested, the mucous membrane was otherwise not affected, and at the posterior part the crown of the wisdom tooth was revealed—a perfect tooth apparently—around one side of which the tumour had extended itself.

The specimen is about 1¾ inches long, ⅞-inch broad, and ⅞-inch deep. I have no doubt it was originally as smooth on the top as beneath the centre part, the roughness arising from the top having at different times had pieces cut off its surface, while the fracture marks the spot where the forceps split it; and I venture to suggest that some considerable force must have been exercised upon it.

Upon the posterior end and on the lingual side you will see the smooth part that partly encircled the wisdom tooth, and at each end a cauliflower excrescence seems to grow out from the remainder, covered with nodules of enamel-like little white beads. The peculiar strata radiating from the centre of a spot near the attachment to the wisdom tooth suggest to one's mind the probability that the trouble may have arisen from some abnormal condition of that tooth.

On the other hand, the absence of the second molar, which could not be accounted for, and the ultimate eruption of the wisdom tooth, in a perfectly normal condition, leads one to suppose, after examining the mass, that it is a second molar whose odontoblasts have in some way been injured or destroyed before calcification set in, the irregular mass exhibited being the result. The patient's photograph, taken after the parts had healed and the wisdom tooth had taken its position, shows two distinct scars where the sinus were situated. The section which I have prepared confirms the diagnosis, and shows clearly that it is composed principally of dentine, intermixed with layers of enamel and cementum, irregularly formed, apparently radiating from a centre, near the part adjoining the wisdom tooth, and from numerous small canals. There are also numerous lacunae where the layers of cementum are situated, and little nodules of enamel, being of a brownish colour. At other places large vascular canals are exhibited suggesting osteo-dentine.

A number of beautiful wax models by Dr. P. Clennel Fenwick, of Christchurch, were exhibited in the Library. These models were representations of various bladder and urethral diseases, some derived from cystoscopic inspection, and others from *post-mortem* examinations.

The CHAIRMAN said the Secretary had had an arduous time; but he had conducted his work splendidly. At the same time the people of Brisbane were very, very kind. They not only gave them intellectual food, but every other kind of food—too much, perhaps. He proposed a vote of thanks to Dr. Hopkins for acting as secretary, which was carried.

Dr. HOPKINS briefly replied, expressing his only regret that it would be a long time before the Congress met in Brisbane again.

SECTION OF EYE, EAR, NOSE AND THROAT.

EYESIGHT IN SCHOOL CHILDREN UNDER COLONIAL CONDITIONS.

By H. Lindo Ferguson, M.D. Dubl., F.R.C.S.I.,
Dunedin,
PRESIDENT OF THE SECTION.

Gentlemen,—

My first duty is to express my thanks for the very great honour which has been done me in placing me in the position of President of this Section, a step which I feel is more a compliment to New Zealand than in consequence of any claims of my own to the chair. It is, however, a position which I am very proud to occupy, and which I shall endeavour to fill to the best of my ability. In our individual capacity as members of a great profession, we are, from the very nature of our work and the conditions under which it is performed, rather apt to get into grooves, and, becoming engrossed in the occupation which each day brings, to lose the true sense of proportion as regards our own work and that of our fellow-workers. It is the most valuable feature about meetings such as that in which we now have the privilege to be taking part, that we meet our professional brethren, discuss our methods of practice, amend views which were becoming narrow, and regain our power of perspective. The gain to us personally, and to our patients, is very great; but when we are here in our corporate capacity we have to remember that the public look to congresses such as these to speak with an authority such as no individual can exercise on any question which affects the public welfare, and to give such warnings or advice as may help the lay reformer to attack the various abuses which are a menace to that welfare, or are likely to prejudicially affect the health or development of the nation.

Good work has been done in this direction at previous Congresses, notably by the Section of Public Health, and I am pleased to say that the result of the discussion on the eyesight of sailors and railway employees at the last Congress has been a very real increase in the safeguards of the travelling public.

The subject we are about to discuss is one of the greatest importance. The eyesight of school children under colonial conditions—though it has, of course, been studied by all of us individually in our practices—has never, so far as I am aware, been dealt with by a committee of experts; and if, in the opinion of this Section, there are any conditions in school life in the colonies which are calculated to interfere with the future well-being of our colonial youth, it is our duty to speak out clearly on the subject, and indicate to the different Education Boards the manner in which we think the abuse may best be met.

In a matter of the sort, when we have the views of men who have seen the result of education on the eyes of children in different colonies, under systems, and in different climates, we have a good chance of getting at general principles of reform, and of avoiding confusion of essentials with small details of management, which would be far more difficult to do in dealing with the system of one colony alone.

I do not propose to inflict a statistical paper upon you. I had never thought of such a thing; I have had neither the time nor the opportunity to collect the figures since the selection of the subject for discussion here, and such data as I shall depend upon in my remarks are gleaned from my private case-books of such cases as I have seen in New Zealand during the past sixteen years. Before going through my case-books, although I knew the amount of school myopia to be small, I was prepared, in view of German statistics, to find it much greater than it really proves to be. I found, however, that even by including all cases up to the age of twenty-five or twenty-six, in which there was a distinct history of the vision having failed during school life, only 2 per cent. of the patients who consulted me in my study could by any possibility be put down as suffering from what we know as school myopia. This is indeed far above the real mark, as from my own knowledge some 25 per cent. of those were cases that might more fairly be termed family or hereditary myopia (of which one family alone furnished seven examples who inherited the tendency from the mother).

If, in addition to excluding this class, we limit our observations to those who come within the school age proper, I am very near the facts in saying that not over 1 per cent. of the cases that seek advice in the study do so for school myopia in New Zealand.

Failure to see the blackboard is readily noticed, and calls for relief. Probably nearly all the myopes find their way at some time into somebody's consulting-room; but my experience is that for every case of true school myopia that comes to me I see more than ten hypermetropes whose eyes have, for one cause or another, failed during school life. If they only form, say, 9 per cent. of the pathological conditions, the percentage of myopia must fall to a very small figure when the vast majority of children whose eyes do not break down, and who never come to the study, is taken into account. To check this conclusion, I applied to the lady principal of the Otago Girls' High School, and she has ascertained for me that, out of a total of 135 girls, there are only eight who have appreciably defective distant vision, as manifested by wearing glasses or being obliged to approach the blackboard in class. It does not follow that these are all cases of myopia, but, assuming them to be so, the percentage is under 6, while in the tables given in "Cohn's Hygiene of the Eye" we find that in schools of a similar grade in Germany the average percentage runs from 22 in the lowest form to 58 in the highest.

This freedom from myopia in New Zealand is not in any way due to special precautions on the part of the authorities to prevent it; the lighting of the schoolrooms is not always perfect, there are no desks graduated to fit pupils of different sizes, no particular care seems to be taken to prevent undue approximation of the work, the paper of the school books is fair, but the print is certainly smaller than an adult would read for pleasure: I can only consider that the small percentage of myopia is due to racial immunity from the condition.

This conclusion is borne out by Mr. Brudenell Carter's statement in his report to the Education Department at home in 1896, that he failed to find evidence of any extended prevalence of myopia in over 8,000 school children examined in the Board schools.

Mr. Brudenell Carter found that the ordinary refractive condition among the children was hypermetropia, and, if I may draw an inference from the children who have for one cause or another been under my observation in New Zealand, I fully endorse his result, for, though the total number of cases which may fairly be spoken of as having school myopia was only about fifty out of a total of over 5,000 eye cases of all sorts, the number of children of the school age who had hypermetropia and who sought relief for conditions which might have arisen more or less directly from it was over 500; it so happens that in making up my lists I omitted all cases of high degrees of hypermetropia, as I was studying specially the point of accommodative failure and purposely excluded all cases of 4 D and upwards, as their accommodative distress was independent of school conditions. There were 82 cases of hypermetropic astigmatism, and 132 cases of concomitant strabismus; ulcers, nebulæ, phlyctenular conditions and lid troubles with hypermetropia account for 116, but by far the largest class was that which included accommodative troubles with hypermetropia, such as swimming of print, pain on accommodation, frowning, blepharospasm, and similar distresses which were due to accommodative effort in hypermetropic eyes, and required treatment in most cases with convex lenses for reading, either temporarily or permanently. This class totalled 175, and from it were excluded all cases of high degrees of hypermetropia in which such distress would naturally be expected, and all cases in which from the history there was any reason to suppose that the asthenopia was the result of measles or other illness.

The question immediately arises, Why should such a large number of children show evidence of insufficient nerve force for the accommodative effort? In the great majority the refractive error was small, in some of the most marked cases not more than 0·5 D, and sometimes even this trifling amount was manifest.

The conclusion pointed to by the figures is clearly not that our children are in danger of breaking down during their school life in the direction of myopia, as is the case in Germany, though when it does occur it is just as difficult to control, but that the tendency is towards the neurotic type of failure which forms so large an element in the work of our American *confrères.*

On attempting to analyse the figures in this class, I found that the total number of boys was 69, and of the girls 107, showing that the nervous strain was felt much more severely by the girls than by the boys. The most interesting point was brought out in the comparison of the ages at which the nervous breakdown made itself felt in the two sexes.

The ages were—

	Boys.	Girls.
5 to 6	3	2
6 to 7	4	2
7 to 8	5	5
8 to 9	7	3
9 to 10	8	10
10 to 11½	10	13
11½ to 13	9	20
13 to 14	8	18
14 to 15½	5	19
15½ to 17	5	7
17 to 18	5	8
Totals	69	107

We see from this list that in boys the incidence of the breakdown increases steadily from the beginning of school life up to the age of from nine to ten. It then remains stationary till fourteen, and then is steady at a little more than two-thirds of the highest rate, up to the end of the school career. In girls, up to nine years of age, the incidence is less than in boys, but from that age it increases very rapidly, and almost one-third of all the cases in both sexes occurs in girls between the ages of eleven and a-half and fifteen and a-half years.

The reason why girls between these limits of age should suffer more readily from symptoms of nervous exhaustion than their older or younger schoolfellows is very obvious; but that our present system entails such exhaustion must give us food for much thought as to what the result will be not only to these children in their adult life, but also to their children and the generations to follow. In the large class of cases in which there is in girls, past the school age, evidence of nervous instability present with anæmia, I have often obtained a history of the trouble having been more or less continuous from the years which these figures point to as being the critical age, and I have often doubted as to whether our educational system was not to blame for the prevalence of that anæmia which is so universal among colonial girls.

Inquiry as to why children should feel the strain of their work so much as they seem to do, at once brings us face to face with two important factors in our educational system—the first is the promotion of children from standard to standard by examination; the second, the system of educational scholarships given in the primary schools.

As long as teachers find that their reputation as successful teachers depends on the percentage of their pupils who pass the examination from one standard into the next at the annual inspection, there will of necessity be a strong temptation for them to cram the pupils for the examination, instead of studying the effect which the high pressure has on individuals. I am glad to say that at present there is a movement on foot to do away with this system in part, and to allow a child to pass to a higher standard if it has only failed in one subject, so that it will not be necessary to spend another year over the same English and geography because the child does not come up to the pass standard in arithmetic, but, even if the strain of cramming for the annual pass examination is reduced to a minimum, the evil effects of the scholarship system will still remain. There are every year a number of scholarships given to pupils of the primary schools—the limit of age being thirteen—which carry free education and maintenance money for two years in connection with the High schools. These are known as the junior scholarships. In addition to these the Otago High School Board give free education without maintenance money to all such children who at the scholarship examinations fail to secure scholarships but receive more than 50 per cent. of marks at the examination. A second series of scholarships, known as the senior scholarships, are open, with a maximum age of fifteen, which carry the children on at the High schools for three years longer, when they are of an age to compete for the university scholarships.

Theoretically, this system is all very well, but its effects are far-reaching, and in many ways undesirable. That the children of the settlers in remote parts should be able to obtain a university education, if they show ability for it, is a very excellent thing, but the system as it works out in practice means that in each school there are one or more children who are going to compete for the coveted scholarship at the age of less than thirteen, and they set the level of attainments which is expected of the rest of the class. A child of exceptional ability, such as the scholarships are intended to benefit, may be able to do the necessary work without suffering for it afterwards, but the penalty is paid by the rest of the class who are doing more work than they are able to do without suffering from it in some way. Quite recently a mother brought me a girl who had broken down at school, and said that her son, a boy of about thirteen, was in much the same condition, and that his eyes were troubling him very much at night, that he had an extra hour in school, making six hours, and between five and six hours' home work, because there was one boy in his class who was preparing for the scholarship examination, and the work of the class had to be set accordingly. The evil does not end here, for the next class below has to be prepared to take up the work of the scholarship class the following year, and the standard of the whole school, down to the infant class, is regulated by an examination for which the great majority of the children will never enter.

Failure at the junior scholarship examinations does not debar from entry for the senior ones; so that, while all the children who have gained a junior scholarship are for the next two years working to obtain a senior one, with the three years' more education that it represents, those who are just below the standard, and have missed the junior prize, are straining every nerve to get the senior scholarship, which is probably their last chance of a university career.

I have several times discussed the question of home lessons with teachers, and one teacher of great experience said to me, "I am sure all my children take five hours over their home lessons, and if they had less to do their parents would complain that they were not being given enough work, and that their chances of scholarships were being sacrificed." What is to be said to the parent who thinks an eight-hour day for his own work is one of the laws of the universe, but who insists that his growing children should work ten hours?

I have shown that the nervous stress is felt more particularly by the girls, and that the age for the senior scholarship examination keeps up the strain to the age of fifteen. But in the High schools, as in the primary ones, the standard is set by the requirements of the scholarships for which the senior class have to compete. In this case (the university scholarships) the girls from thirteen to sixteen are being prepared to enter the sixth form, from which they compete for these scholarships, and, in order that you may more clearly understand what this represents, I will give you the course of study for the sixth form of a girls' High school for last year:—

COURSE OF STUDY, 1898.

Sixth Form (Upper).

English.—Chaucer—"The Knightes Tale," "Truth," "Pity," "Parlement of Birds." Shakspeare—"Macbeth." Spenser—"Fairie Queene," Book I., Cantos VII.-X. Milton—"L'Allegro," "Il Penseroso," "Comus." Literature—From Elizabethan to Victorian Period. Historical English Grammar. Composition, &c.

Latin.—Virgil, "Aeneid," Book I., 110 lines. Livy—Book XXI., 24 chapters. Horace—Odes, Book I.—Book II., 7 Odes, and Selected Satires. Cicero—"In Catilinam," I. and II. Cæsar—"Gallic War," Book I., 8 chapters. Tacitus—"Agricola," 36 chapters. Reid's Translation at Sight—Selections. Composition, Grammar, &c. Roman History.

French.—Chardenal's Advanced Exercises. Erckmann-Chatrian—"Waterloo." Boielle—Poetry. Grammar, Composition, &c. Ninet's Readings for Middle and Upper Forms.

German.—Schiller—"Maria Stuart" (part). Otto's German Grammar. De La Motte Fouque—"Undine." Goethe—"Faust." Buchheim's German Prose Composition.

Mathematics.—Arithmetic—The whole subject. Algebra—To Permutations and Combinations, inclusive. Geometry—Euclid, Books I., II., III., IV., VI. Trigonometry—Lock's Trigonometry.

Science.—Botany—The Morphology and Physiology of the Botanical Types specified in the Junior Scholarship Schedule. Chemistry—The Metallic Elements—Revision of the Non-metallic Elements.

This means practically that when the girls have passed through this form they have been educated to all intents and purposes up to the standard of a pass B.A. degree, which is the level they are supposed to attain after a further three years at the university. The result of this work for the school is that from one to three university scholarships are won by girls from this form, and appear in the list of school prize-winners; the private schools which are competing with the High schools have to educate their girls to the same standard, and, seeing that the preparatory work for the last year's course has to fall on the children when my observations show nervous breakdown to be most common, I think you will see that the explanation of the breakdown lies not so much in any inability of healthy children to face a physiological change naturally under normal conditions as in the undue nervous effort during that change caused by the standard of education which the scholarship system entails. The matter is one which affects the future of the race chiefly as it touches the girls, and it is our duty to educate the public to see that between the ages of twelve and fifteen years girls undergo developmental changes which make it exceptionally hard for them to face the call of extra educational work, and yet this is the period at which the New Zealand scholarship system taxes them most severely. It is no answer to such a criticism to say that many children take their scholarships and get through their education apparently without harm; we have, in looking broadly at the question as affecting the public health, to consider the amount of harm done to those who do not get the prizes by having to work alongside those who do. Colonial parents have a very laudable desire to so educate their girls that they may be able to support themselves. Those who have not the means to prepare them for a profession as a rule wish them to qualify themselves as teachers; in either case they are anxious that the child's education should proceed as fast as possible. To ask that for three years, from twelve to fifteen, their education should proceed on very much modified lines is to invite an outcry such as would be raised if one urged three years' military service for every boy from eighteen to twenty-one, but the slow process of evolution which has during countless ages made girls what they are has done so not to make them teachers of backblock schools, but the mothers of generations to come. Injudicious education may very easily render them unfit to fill the part which Nature destined them for, but it is our place to utter a note of warning as to what the future result will be. I am quite prepared to accept the statements which we hear, that girls up to ten or eleven are much quicker to learn than boys. I am not anxious to enter into any argument with those who hold that women can hold their own in any intellectual field with men, but I am convinced that, during the time which covers the developmental changes which convert a child into a woman, girls cannot compete with boys in the matter of education without serious detriment to their own health and without serious risk of transmitting to the children some neurotic disability which will prejudicially affect the stamina of the race. It is the custom in New Zealand to send children to school at a very early age—five years or even less. The teachers say that the parents send their children as soon as they can walk to school to get rid of them for so many hours in the day. Personally I am of opinion that at the age of nine they are no further advanced in their studies than if they had run wild till the age of seven, but I have not satisfied myself that any ill-effects either to the eyes or nervous system results from this custom, and the parents have a certain amount of reason in the contention that they are kept out of mischief. The main blot on our system is that to which I have referred at such length, and I should especially like suggestions for its removal. My own view is that as scholarships will continue to be granted there should be only one class given, with a currency of five years, and that the age limit should be eleven, the standard being proportionally reduced. The scholarships might be deferred so that the children should not go to the High schools till they were twelve or even thirteen, although the examination had been passed one or two years previously. This would avoid competitive examinations during the critical period of the children's lives. For the three years from twelve to fifteen I would much reduce the mental work, confining it to English subjects, and devoting the time to instruction in sewing, cookery, dressmaking, knitting, and other such feminine accomplishments as do not come into a university curriculum but are, nevertheless, of much value if the girl, instead of becoming a teacher, becomes a wife. I should also during this three years insist on a large amount of outdoor exercise, tennis, hockey, baseball, cricket, swimming, golf, gymnastics when outdoor exercise was impossible—anything that would help to promote the physical development at the time when it is most in need of the healthy stimulus of those physical recreations which boys have, but which girls have not. At fifteen the amount of studies might be increased, and, if by eighteen they are not to enter the university, I have no hesitation in saying that they are making a mistake in trying any longer to do so. It may be that such a change in the system as I have outlined would result in some being excluded who now become teachers, but I am convinced that great good would result to the majority.

The statement that the battle of Waterloo was won on the playgrounds of Eton and Harrow may not be strictly accurate, but it contains a great truth; in the great battle of survival of the fittest, whether our race is to figure as a strong, healthy, vigorous nation, or as a weedy, neurotic, decadent one, depends very largely on the stamina of the mothers of the future, and it may be that the battle will be won on the playgrounds of our girls' schools.

It may, perhaps, be thought that I have travelled in my remarks beyond the subject which we are to discuss to-day, but I do not think that this is the case. It is in the ordinary routine of our day's work to be consulted by people who say they want glasses for reading, and after examining their eyes we find ourselves confronted with the task of breaking to them the news that their failing sight is due to Bright's disease or diabetes, to poisoning of the nervous system by alcohol, by tobacco, by lead in their water supply; that it is due to early locomotor ataxy or other serious form of nervous disease. Our work is inseparably bound up with the study of nervous derangements and degenerations; if we find that a man's failing sight is due to early general paralysis, we do not hesitate to take what steps seem best calculated to safeguard the interests of the patient and his family; if we had a number of cases of saturnism from one district we should feel bound to inform the sanitary authorities, and protect the public health; if we find that the stress of our educational system is pressing unduly on the nervous system of the mothers of the next generation, it is our duty not to content ourselves with ordering a pair of glasses for reading and a tonic, but to enter a protest against the perpetuation of methods which menace the future of our children, and to an unascertainable degree the nervous balance of their heirs.

DISCUSSION ON EYESIGHT IN SCHOOL CHILDREN.

Dr. F. A. Pockley (Sydney) said he did not think the author of the paper had gone beyond the province of the specialist in the treatment of his subject. Indeed, he (the speaker) agreed with the sentiment expressed by the President of the Congress in his inaugural address—that any branch of special study "neither is nor ever can be a separate entity, but is only an offshoot of the great tree of medical knowledge." With regard to the matter under discussion, his experience, extending over a period of ten years (first in general practice and subsequently in connection with eye diseases exclusively), enabled him to endorse the main points of what Dr. Ferguson had put forward in his paper. As the result of his experience and observation, he was of opinion that school myopia did not represent more than 1 per cent. of the eye diseases treated in New South Wales. He was unacquainted with the school system of Queensland, but he had long protested against the system of cramming which obtained in New South Wales, and which often gave rise to serious neurotic influences. At most of the schools, children were set tasks which they had to perform at night, frequently during hours when they should be building up tissues by sleep. In connection with the subject under notice, the great influence for harm was the constant cramming for examination. Among the evils of this practice, children were induced to be dishonest because they were enjoined not to seek help in tasks which were often too difficult for them to master themselves. The result was that rather than fail to do their lessons and thus appear behind others in their class they sought the assistance of parents or someone else. The amount of work set forth on the curriculum read by Dr. Ferguson, to show what an excessive and unreasonable strain is put upon pupils in some schools in order to keep up to a competitive standard for the sake of scholarship students, was simply astonishing. The constitutional after-effects of such unnecessary over-study was a serious matter. There was far too much bookwork, as was shown by the breakdown of so many students just before examinations. Dr. Muskett (of Sydney) had recently shown in a very able way what a great and injudicious excess of expenditure there had been on theoretical education in New South Wales as against the amount expended on technical education; and he (Dr. Pockley) considered that if they could make any suggestions at the Congress with a view to promoting technical tuition they should certainly do so. Theoretical and technical instruction should, in his opinion, be so alternated as to provide for the judicious exercise and rest of hand and brain, so that both would be employed to the best advantage. How often did they find that mere theoretical education was in a large measure useless to the learner in after-life, when a trade or profession was adopted; so it not infrequently happened that students, including those who wished to qualify for lawyers, architects, or doctors, had to begin *de novo*. There should be a better balance between theoretical and technical education with a view to practical utility, and if they could do anything towards lessening the evils attending the system of cramming an undoubted good would be accomplished.

Dr. J. J. Miller (Brunswick) was thoroughly in accord with both the author of the paper and with Dr. Pockley with regard to the dangers of the universal and unnecessary system of cramming to which reference had been made—a system which he regretted to say prevailed in Victoria in much the same way as was stated to be the case in New Zealand and New South Wales. He had often noticed the unmistakable and injurious physiological effects of over-study as exhibited on the visages and in the frames of Victorian school children. Moreover, there was not a properly constructed school building in Victoria, while the print in the books used was of a decidedly bad type for its purpose. He certainly thought the Congress would do a great deal of good if it called attention to the urgent desirableness of introducing suitable systems of hygiene in connection with the construction and conduct of schools.

Dr. Kenny (Melbourne) admired the President's paper for the great thought it showed, and he quite concurred with what had already been said in support of it. He thought it was well within the province of the Section to discuss the subject. Speaking from his own experience as a practitioner, he considered it was time that an attempt was made to effect a very considerable and very radical change in their educational systems. There was no great difference in the system which prevailed in Victoria and that which Dr. Ferguson had described as existing in New Zealand. With regard to the straining caused by the scholarship competition, he recognised that there was a great necessity for reform; so that teachers should have discretion to prevent children from studying to a degree for which they were unfitted. In his opinion the most effectual way of dealing with the evil would be by commencing in the teachers' training schools, where a member of the medical profession should be on the staff as a lecturer to impart such information as was necessary. By this means the teachers would be made a very strong set of allies, who would be capable of detecting defects amongst their pupils, and having them seasonably attended to. A practical step to take—and, perhaps, the only one which would have much effect—would be to pass a resolution and submit it for the consideration of the various education departments throughout the colonies with a view to urging the appointment of medical lecturers for the purpose he had indicated. He felt confident that in every colony they would find competent medical men willing to give their services gratuitously if the Governments were not prepared to pay. With regard to the suggestion that a standard should be drawn up for the proper lighting of schools, arrangement of desks, &c., he thought that was rather too large a subject for them to deal with, unless they appointed a sub-committee to attend to it before the next Congress. Altogether he considered that Dr. Ferguson's paper had introduced a most important subject for discussion, and one which demanded earnest attention.

Dr. Kent Hughes (Melbourne) favoured Dr. Kenny's suggestion that an effort should be made to have medical lecturers appointed to give instruction at training schools for teachers. Ignorance in this respect on the part of teachers often resulted in the most unmerited punishment of children, sometimes with regrettable results. He had known several instances in which school children who were suffering from eye diseases had been beaten for inability to do their work, when, as a matter of fact, they were physically unable to do it. In a similar way children suffering from a degree of deafness were frequently ill-treated. There was much boasting about the Victorian educational system, and it entailed a large expenditure of money, but the practical results were in many respects deplorable. He was convinced that a large amount of good would result from the appointment of medical lecturers to instruct teachers in the testing of both sight and hearing. As Dr. Scot Skirving had said, there might be an element of danger in partial teaching of the kind, but nevertheless he (Dr. Hughes) was of opinion that it would be a good thing to give teachers some instruction in this direction.

Dr. Lovegrove (Petersham), in support of the suggestion for the appointment of medical lecturers to instruct school teachers, instanced a case in which a boy suffering from diabetes had been repeatedly thrashed with very injurious effects, at a time when his condition was very critical, as was confirmed by the fact that he succumbed to the disease shortly afterwards.

Dr. Kirkland (Sydney) stated that as the result of his experience he was inclined to regard anæmia as the chief cause of eye strain among schoolgirls. But anæmia was not confined to schoolgirls, and was also developed after the period of fifteen years of age; so that he thought they were rather relying too much on the pressure of over-study as the sole cause of eye strain. He had had under his close observation the pupils of a boarding school, and had not noted an instance in which myopia could be conclusively attributed to over-study. Although some of the parents complained of over-straining, he believed that the pupils, owing to the fresh air they enjoyed, were in better health than they would be away from the school.

Dr. J. Lockhart Gibson (Brisbane), in some complimentary references to the author of the paper, characterised it as one which had been most instructive, suggestive, and valuable in every respect. It was the kind of paper which tended to confirm Dr. Rossi's definition of a modern specialist—"a man who has a special knowledge in his own department, and who is a complete clinician in the whole field of medicine and surgery." He cordially agreed with Dr. Ferguson with regard to the dangers of placing any educational strain on girls between the ages of twelve and fifteen years. Besides the dangerous and serious aspect of the practice, it was quite unnecessary to force girls to close study at such a period of their lives. Where it was desired that girls should go in for higher education it should, in his opinion, be delayed till they had reached the age of fifteen or seventeen years. To subject girls between the ages of eleven and fifteen years to a course of study such as that indicated by Dr. Ferguson was not only constitutionally dangerous, but in many cases was a cruelty. Moreover, what was learnt under a system of overstrain was of little, if any, use. The same thing applied to cramming. Knowledge might be gained and retained for a while, but it was not wisdom and could not last. A friend of his once wittily described the ultimate or examination result of cramming girls and boys with book lore as "a process of cerebral vomiting," which was appropriate, inasmuch as knowledge so ingested was invariably got rid of as effectually as if it had never been acquired. Concerning the age at which school studies should commence, he did not think children should be sent before seven years, unless to get them out of their mothers' way. Unthinking pupils thus gaining knowledge grew up with a habit of looking to books for everything, and consequently became mere book machines who did not think for themselves. Myopia in young Queensland ametropes did not amount to 2 per cent., excluding, of course, hypermetropic astigmatism.

Dr. Ferguson, in reply, said he was sorry that there had not been a greater expression of the views of the general practitioner on the paper which had been discussed, as he had hoped that by such means it would have been dealt with from a broader standpoint. However, he was glad to find that he had a considerable amount of support in the remarks which had fallen from the several speakers. Dr. Pockley had strongly urged that technical training should be promoted, and the reasons advanced commended themselves. So far as the question of formulating a system of hygienic rules for schools was concerned, he thought, with Dr. Kenny, that it would be rather premature to carry out the suggestion. Many recognised American and other authorities had published works on the subject, and doubtless their publications would furnish all that was at present required for guidance in this direction. He certainly thought that Dr. Kenny's suggestion for the appointment of medical lecturers to instruct school teachers was a very valuable one. Considering that as specialists they had no opportunity of examining school children unless they had some special object, this would decidedly be a step in the right direction. The experience related by Dr. Kirkland regarding the boarding-school pupils under his observation confirmed what he (Dr. Ferguson) maintained—that with fresh air, care, and judicious exercise the danger of eye diseases amongst school children was considerably diminished. Dr. Kirkland was of opinion that anæmia was the chief cause of asthenopia amongst school pupils, but this was a very open question. With a view to giving some practical issue to their discussion, he thought the best course they could take would be to refer the question to a committee, which could draft any propositions which might be deemed desirable to submit to Congress as a whole during the week. By that means they might possibly be able to do something towards bringing about a change in the undesirable conditions under which school children now study. The standard of their system of education was set by the competitive work in the higher schools where pupils were seeking scholarships. Private schools competed with the public schools in this matter; and it meant that there was a general strain in all the classes in order to keep up to the highest possible competitive standard. He instanced, as a case of over-study, that a girl fifteen and a-half years of age, who suffered from intolerable headaches, was recently brought to him for treatment; and upon inquiry he ascertained that her school hours, lessons at home, music lessons, &c., occupied no less than twelve and a-half hours each day, and that all the exercise she had, as a general rule, was the walk to and from the school. If they could do anything in the way of bringing home to the parents, education boards, and others interested the iniquity of placing such undue physical and mental strain on a girl of that age (when she was just passing through the developmental stages that transform from girlhood to womanhood, and when it must not only react on herself, but on her children), they would be rendering a most important service. They must do something; such a state of affairs could not be permitted to continue without protest and effort at reform. If they could only get the first cut into the Upas tree, they would do well. He therefore moved that a committee be appointed for the purpose he had already indicated, and that propositions be embodied in a resolution to be submitted first to another meeting of the Section, and subsequently to the Congress as a whole.

Dr. Kenny seconded the motion, which was carried unanimously, and the following committee was appointed for the purpose:—The President of the Section (Dr. Ferguson, New Zealand), and Drs. Pockley (Sydney), Symons (Adelaide), Gibson (Brisbane), Kenny (Melbourne).

NOTES ON A CASE OF SEGMENTAL HEMI-ACHROMATOPSIA.

By F. Antill Pockley, M.B., C.M. Edin., M.R.C.S.,
Hon. Ophthalmic Surgeon to the Prince Alfred Hospital, Lecturer in Ophthalmic Medicine and Surgery in the University of Sydney.

P.H.W., farmer and grazier, aged fifty-eight, was sent to me by Dr. Scot Skirving on 23rd April, 1897.

History.—A fortnight ago, when going out of his house to speak to a friend in the road, was suddenly seized with dizziness and great pain in top of head, and shortly afterwards noticed he could not see clearly anything on his right-hand side. The dizziness and pain, which was very severe, lasted a week, and then gradually diminished. Has now slight pain, but objects seem to dance about before the sight.

Present Condition.—Patient is a fresh-complexioned, healthy-looking man. Dr. Scot Skirving reports that he is of uric acid type. The heart sounds impure; no other neural trouble save the ocular.

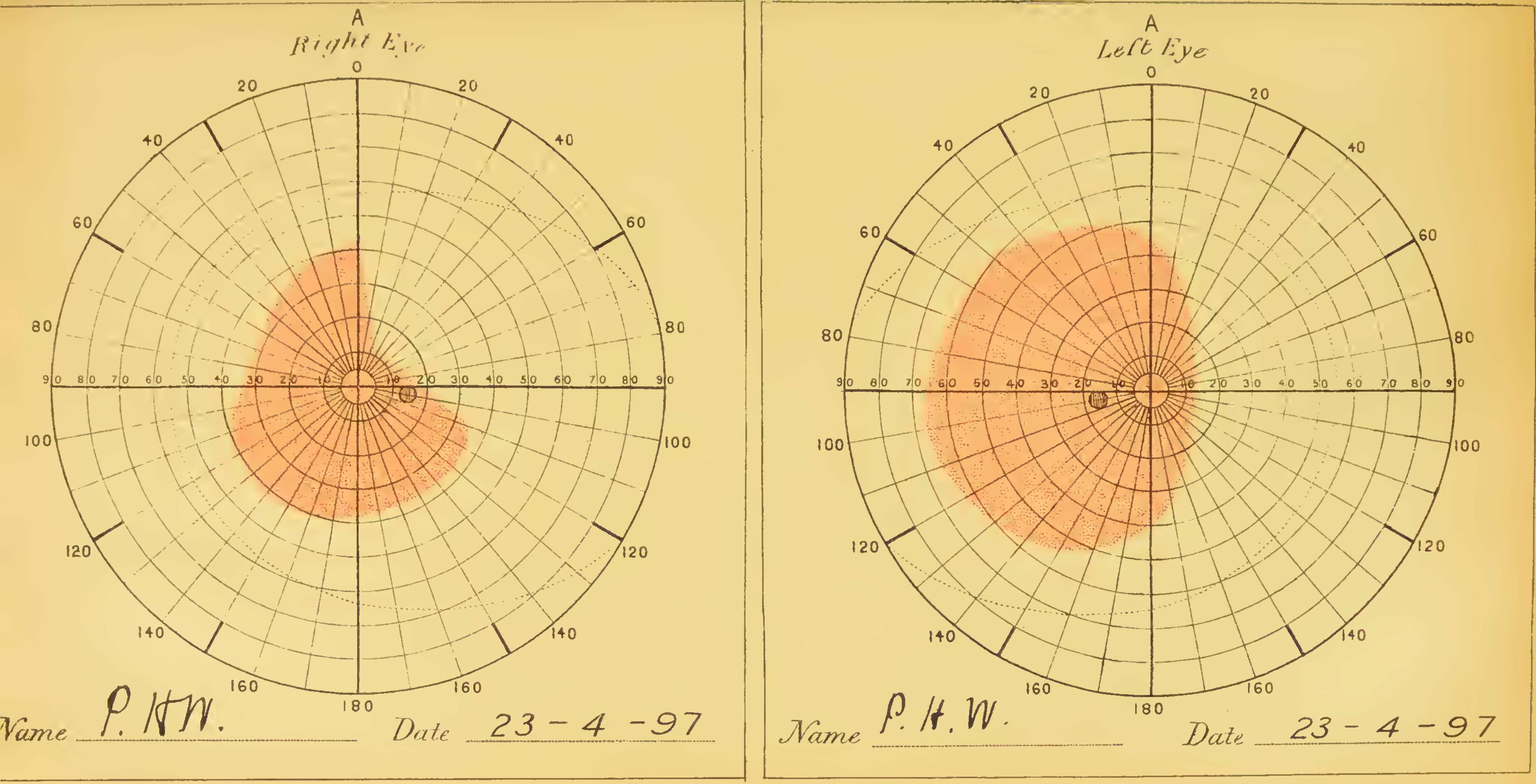
V. in either eye with + 1·5 D. is $\frac{5}{5}$, and with + 4·5 D. reads J. No. 1 at 8 inches.

Perimeter showed form field to be intact for white (test object 5 mm.), but for red and green has no perception in left eye in nearly all the right half of the field, and in right eye in a sector comprising about the upper fourth of right field. *Vide* charts Nos. 1 and 2, 3rd May. After using trinitrine tablets for a few days, and subsequently iodide of potassium, the pain in head quite left him, and limits of colour field are slightly different, the defect in both eyes being now over the upper right quarter of both fields, and there is in addition a negative scotoma for white, also near the apex of the quadrant. Limits of fields for white otherwise normal. *Vide* charts 3 and 4. On 14th May he went home, the fields being then precisely as last noted. Wernicke's sign not elicited.

On 15th August of present year, he came to Sydney and saw me again. Now, two years and four months after attack, the fields are unaltered, except that there is a slight encroachment for white on the outer limit of the colour-blind quadrant. The small absolute negative scotoma is still there. *Vide* charts 5 and 6.

Remarks.—Cases of hemi-achromatopsia are rare (perhaps they would be more frequent if more often tested). Reported cases of segmental colour defect are extremely rare. I have not been able to find a record of one precisely like this. My diagnosis, in which Dr. Scot Skirving concurred, was a lesion, embolism, or hæmorrhage—probably the latter—in a portion of the cortical centre. This opinion was arrived at by a consideration of the character and mode of onset of the attack, and the absence of any other sensory or motor disturbance.

The exact pathological significance of these cases appears doubtful. Heuschen's localisation of the visual centre in the cortex of the calcarine fissure is now, I think, generally accepted; the former localisations of Ferrier (in monkeys) and others having been almost conclusively proved to be wrong. Heuschen also states that the cortex of the upper surface of the fissure corresponds to the dorsal half of the retina, that of the lower surface to the ventral half. Some writers have quoted these portions reversed, but this is probably an error in transcription. A case is reported by Hun, in which the left lower quadrant in each field was blind, and where the *post-mortem* showed an atrophy limited to the lower half of the right cuneus—*i.e.*, the upper surface of

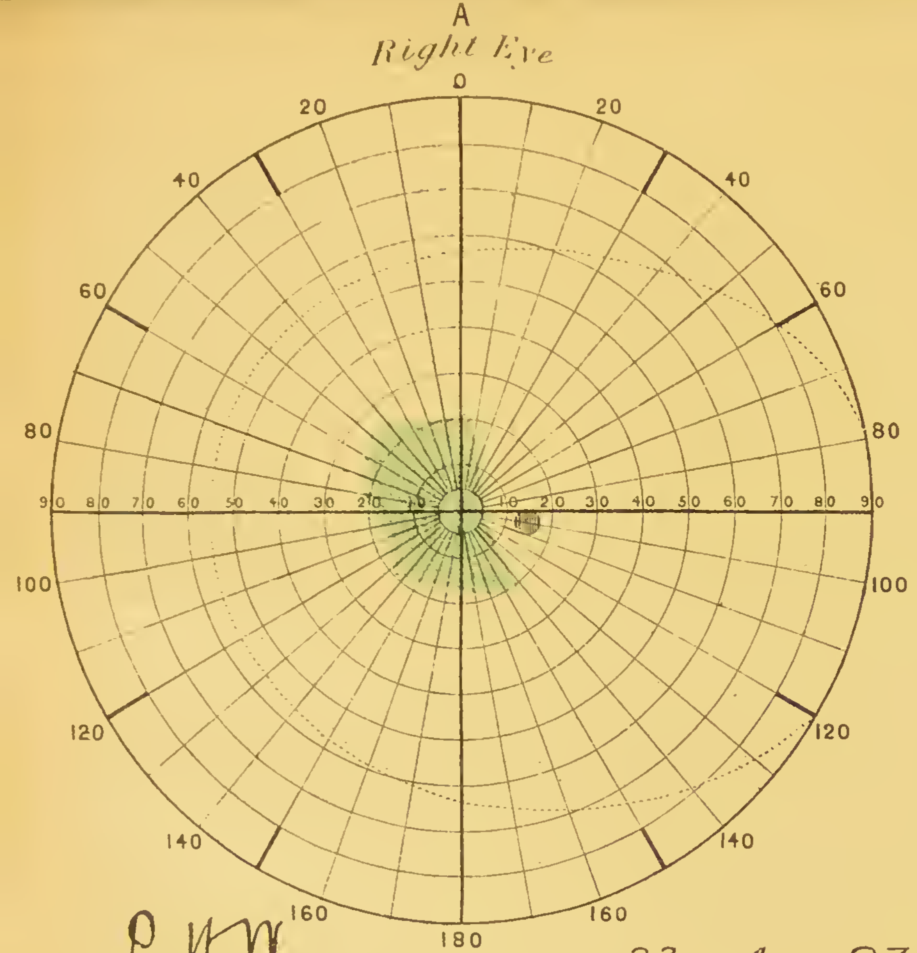


Name P. H.W. Date 23 - 4 -97

Name P. H.W. Date 23 - 4 -97

To illustrate Dr Pockley's case of Segmental Hemichromatopsia.

Right Eye



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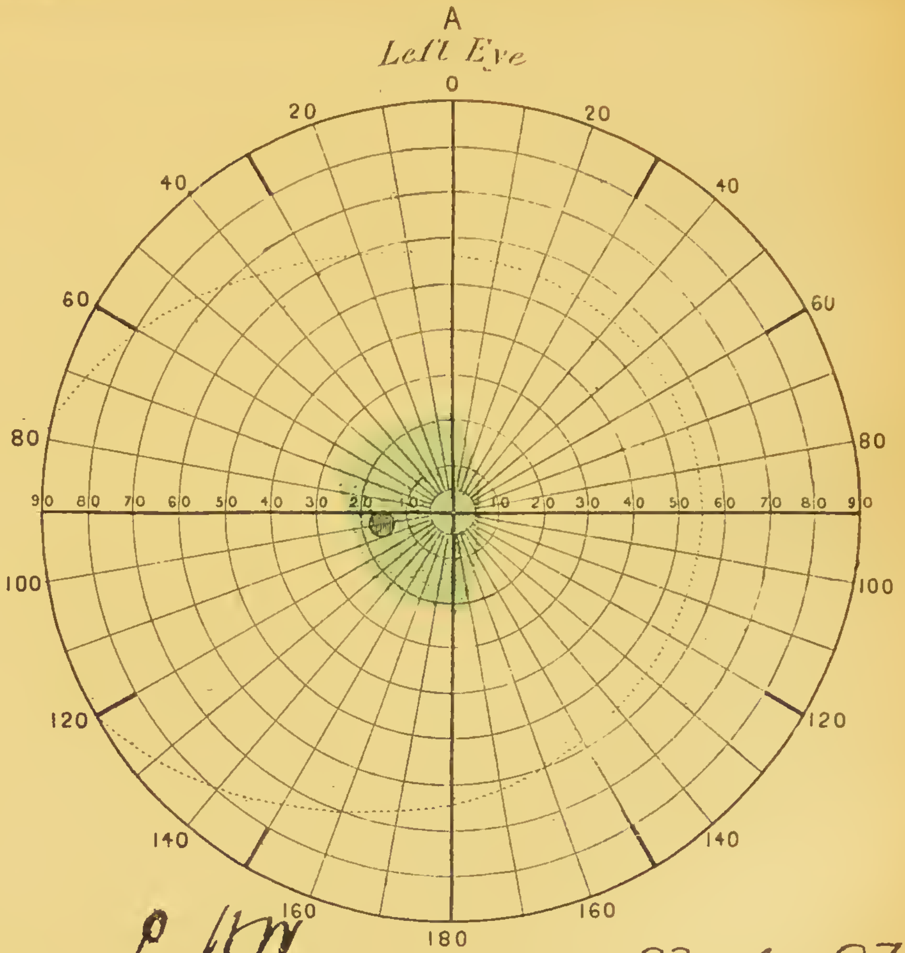
Left Eye



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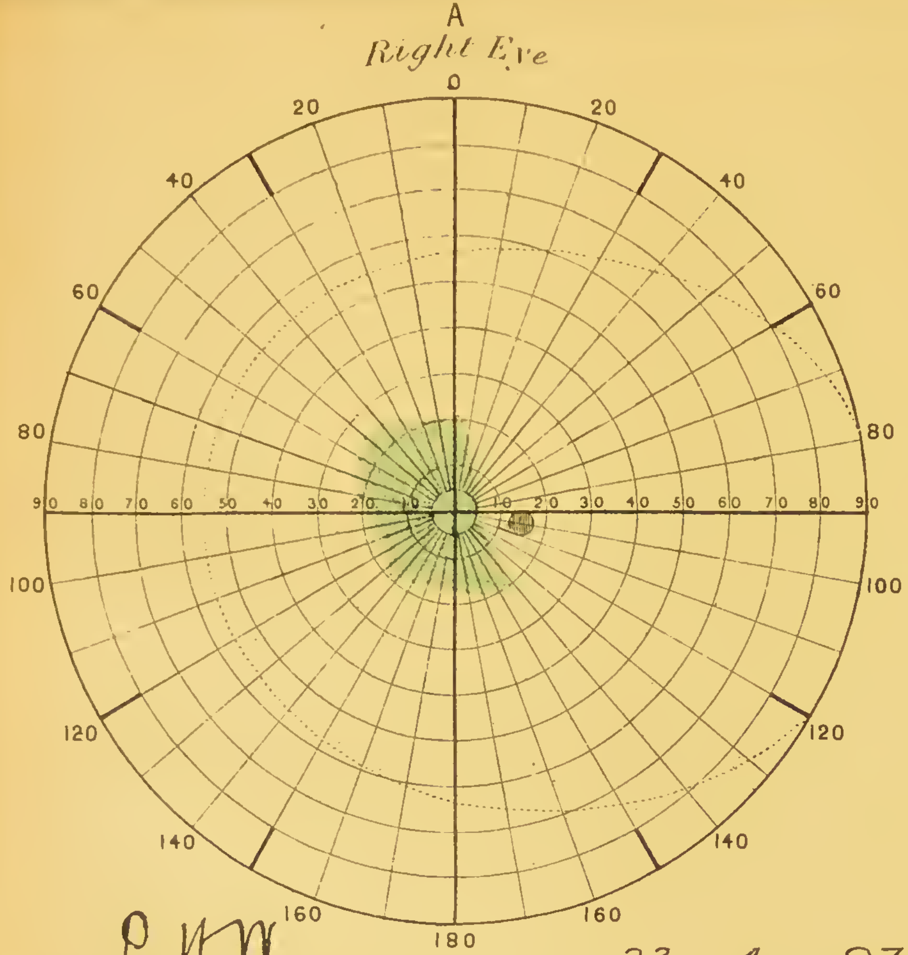

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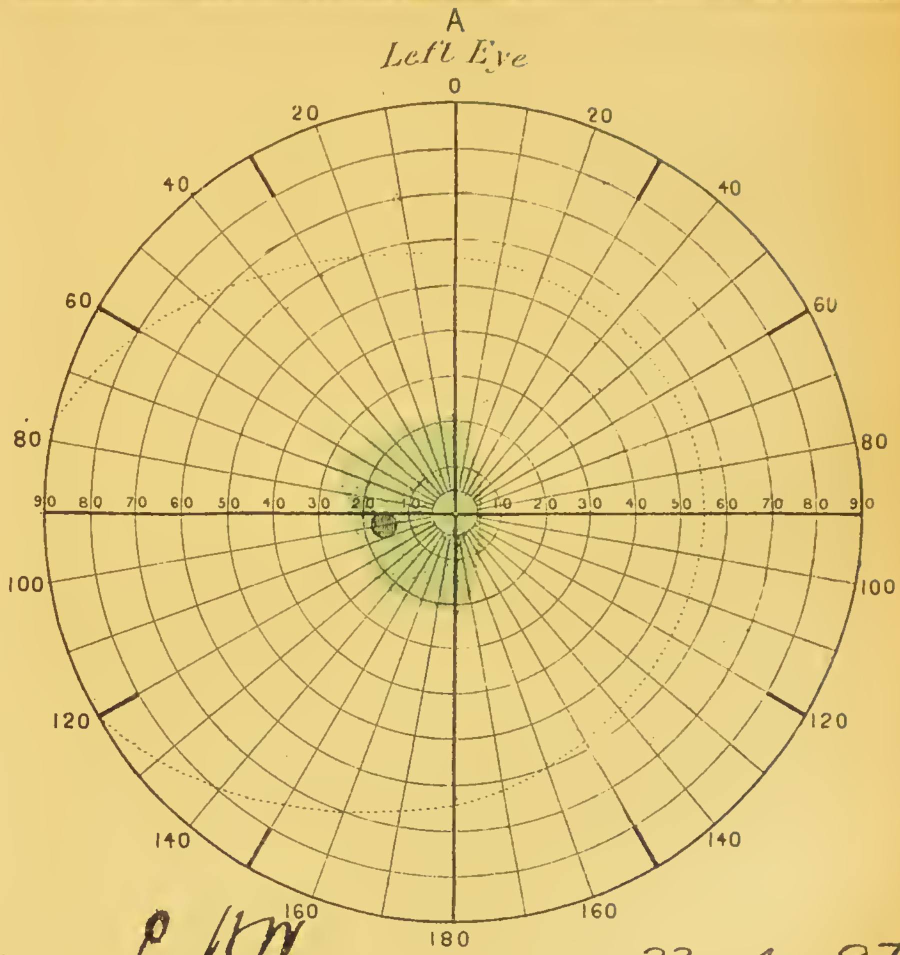

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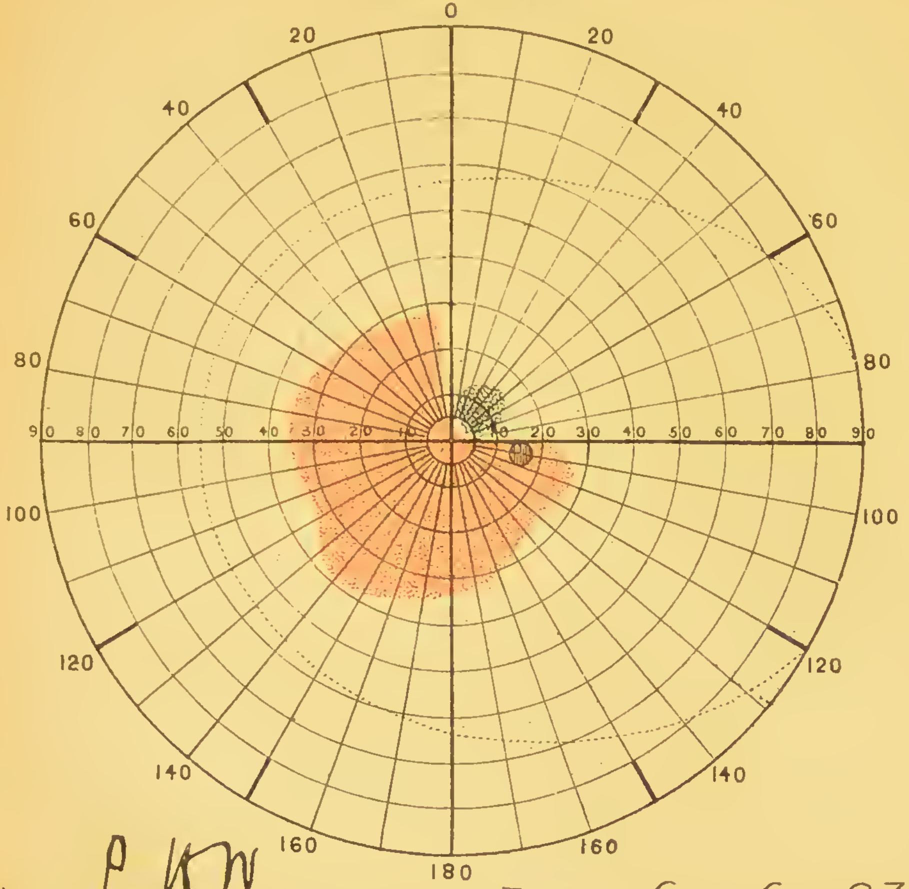
Right Eye



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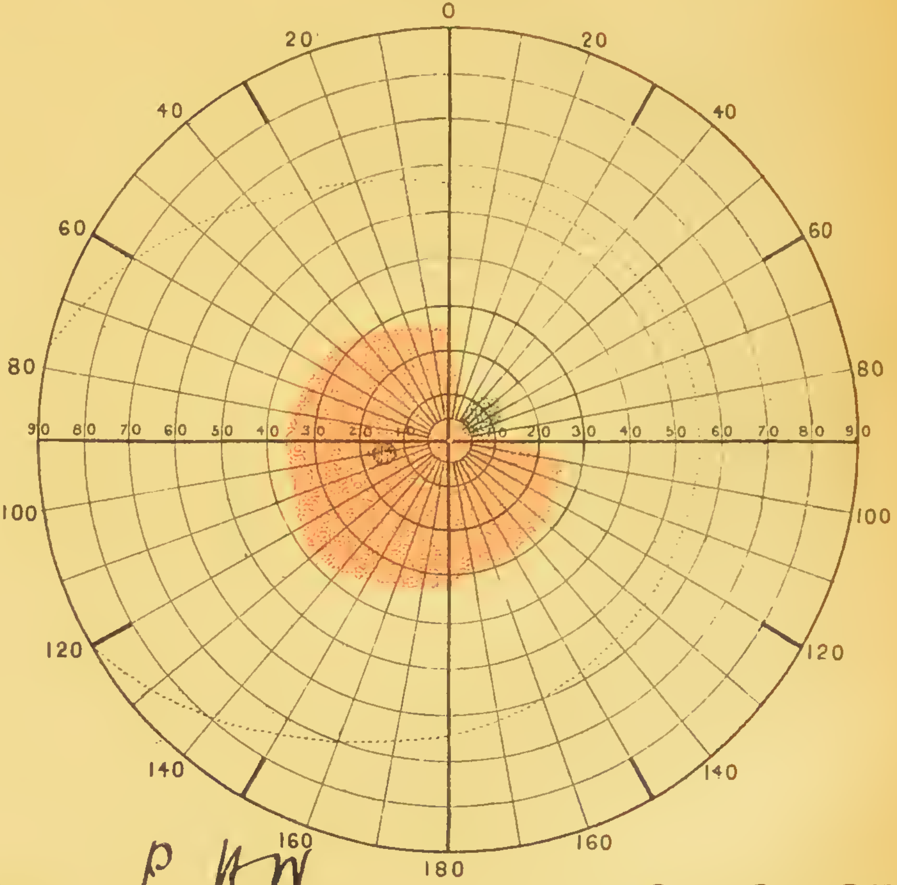

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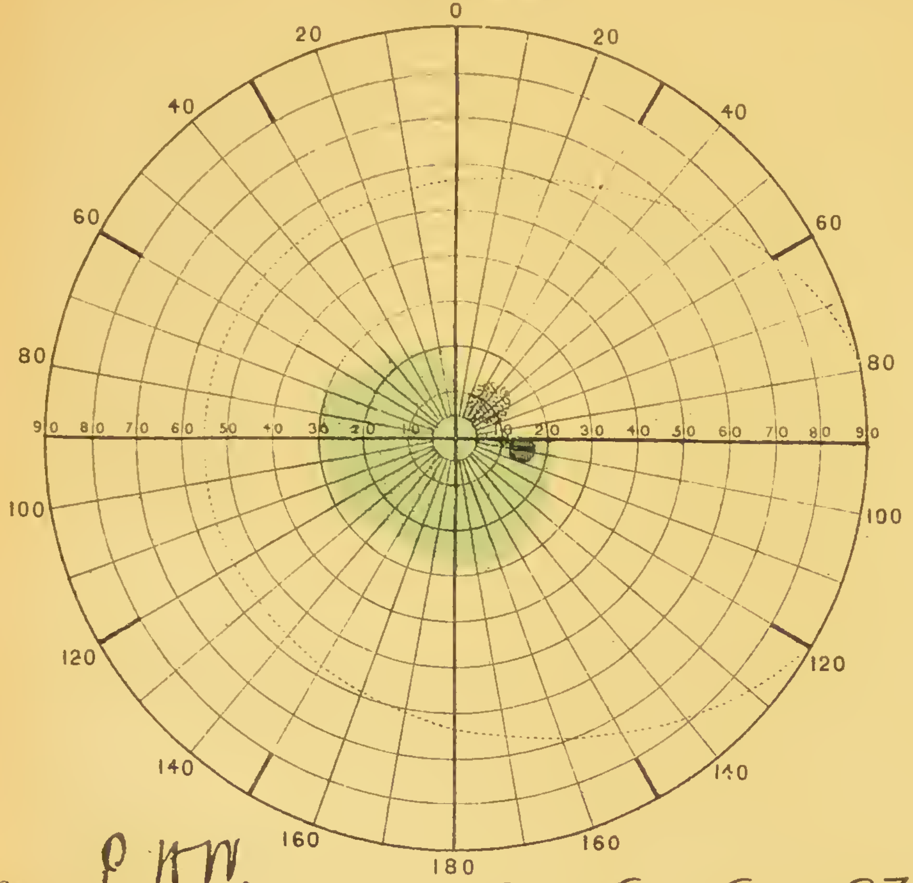

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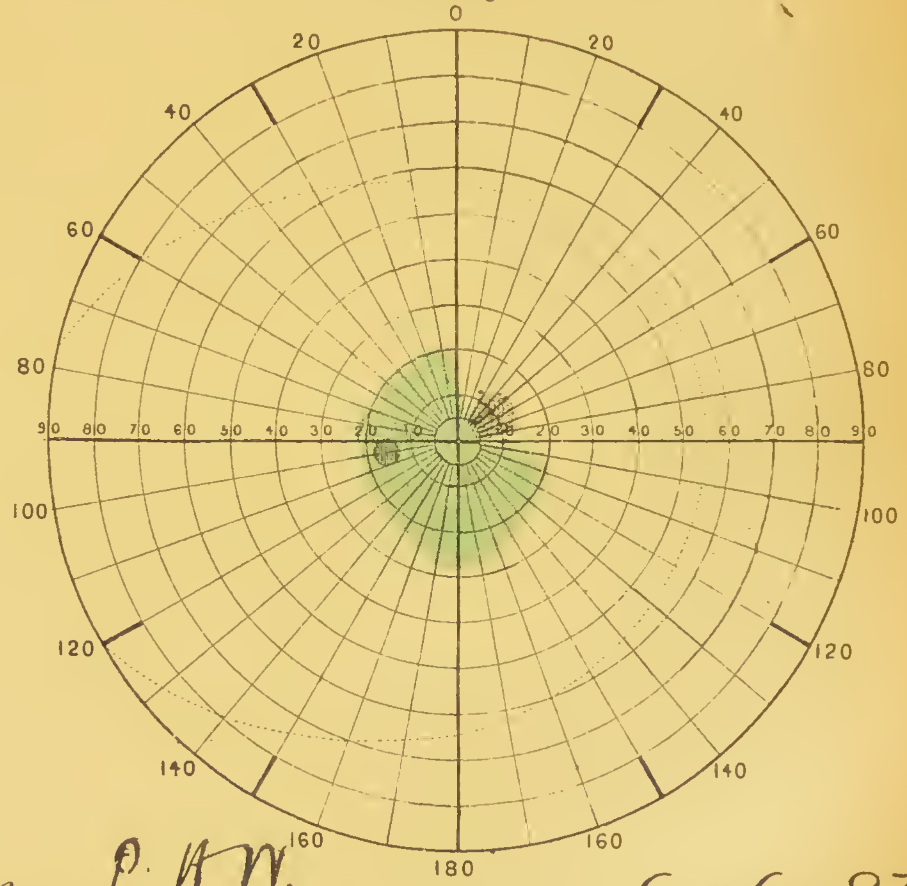
Left Eye



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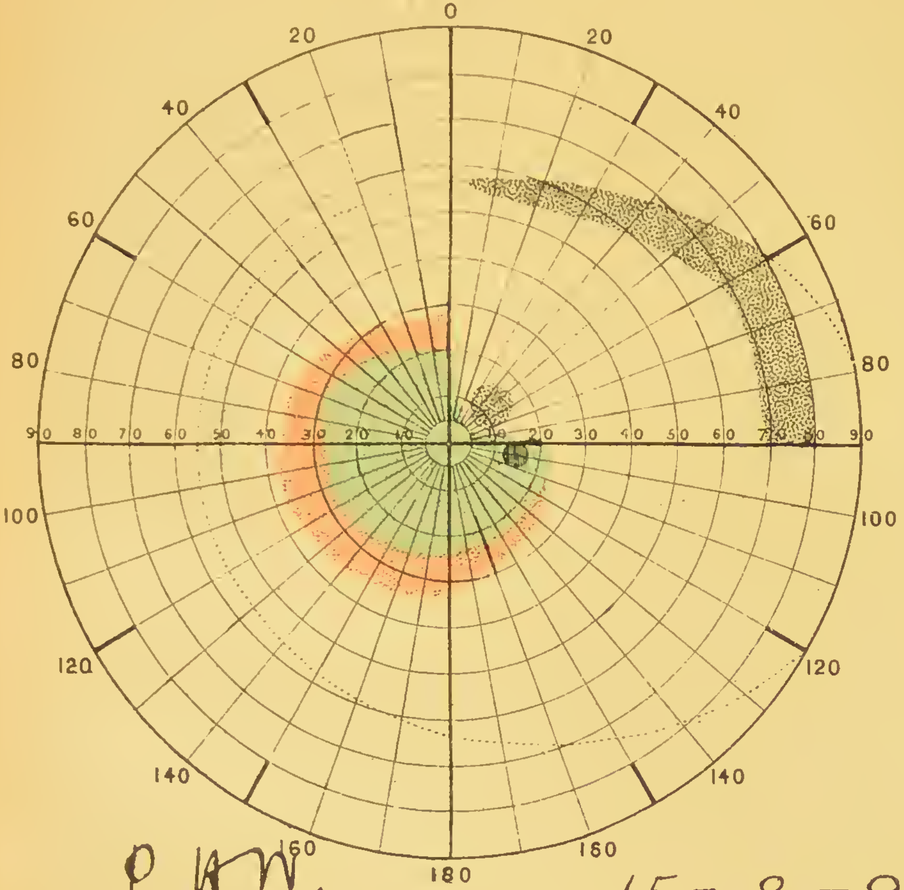
Right Eye



Name P. K.W. *Date* 15- 8 -99

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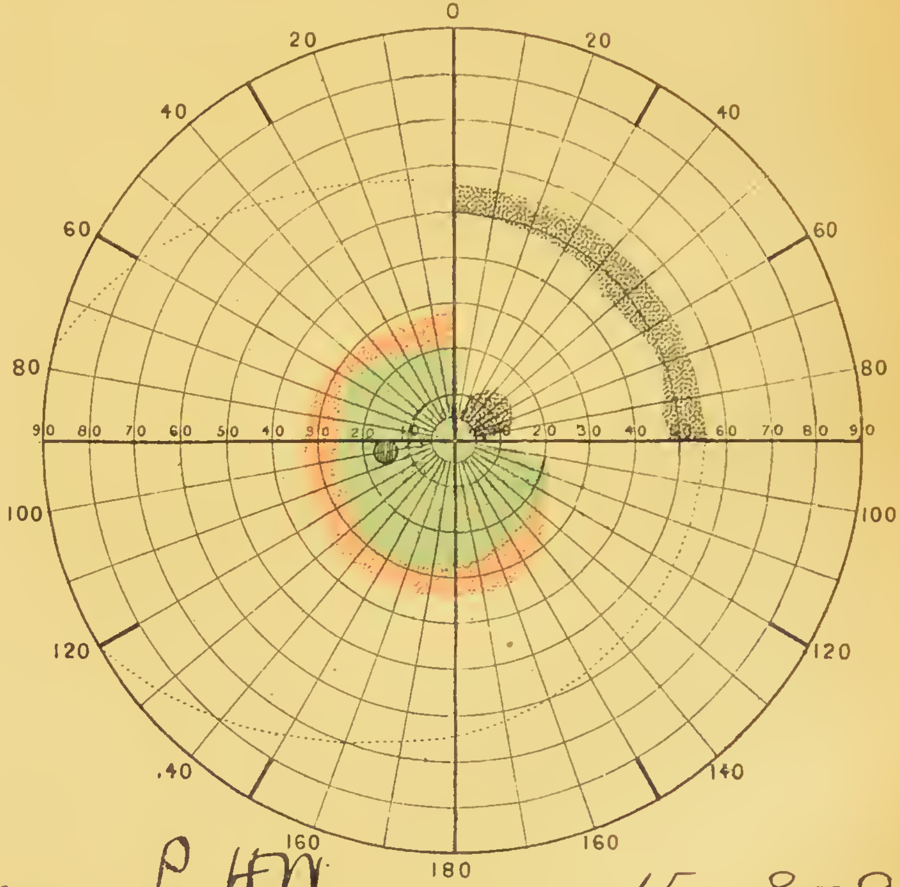
Left Eye



Name P. H.W. *Date* 15- 8 -99

the calcarine fissure. As the upper quadrant of the retina corresponds with the lower quadrant of the field, this case confirms Henschen's localisation. In the case I have recorded, therefore, it appears probable that the lesion was situated in the lower surface of the left calcarine fissure.

As to the significance of only colour vision being affected (excepting the small absolute scotoma), opinions vary, and, as no autopsy has been recorded, they are largely speculative. Wilbrand, of Hamburg, who has made a deep study of hemianopsia and perimetry generally, and who is a recognised authority, has gone a step further than Henschen, Munk, and others, and has formulated an elaborate scheme in which he places separate centres for light, form, and colour perception. He asserts that the latter is situated in the outer cortical layers of the visual centre. Berry quotes him as saying "in the most central position in the brain," which is not quite the same thing. Probably the word "hemisphere" should have appeared instead of "brain." Wilbrand places the light sense centre most superficial, the form sense centre intermediate.

On the other hand, Ward Holden maintains that the assumption of the involvement of a particular cortical centre in cases of hemi-achromatopsia is not only unnecessary, but palpably wrong (Knapp's Archives, October 1895), and that the recognition of colour varies with the light sense. Berry appears to be of the same opinion, for he says that in all the cases he has seen of so-called hemi-achromatopsia he has been able by testing with very small white objects at a distance, according to Bjerrum's method, to demonstrate that the form sense has also been defective over the same area. He adds: "Indeed, the clinical evidences so far point altogether more to the probability of colour vision being due to a subtle differentiation of light impressions than to any impressions made upon other centres than those in which the consciousness of form are (*sic*) invoked."

In my case the demonstration of a small scotoma for form within the area in which only colour perception was absent and the absolutely blind strip at periphery seem rather to favour Wilbrand's theory.

However, many more cases must be examined and recorded before the matter is settled, and it is as a small contribution to the record that I have adduced this case.

BIFOCAL LENSES.

By W. Kent Hughes, M.B. Lond.,
Melbourne.

Many attempts have been made to obviate the trouble of two sets of glasses—one for reading, and one for distance—and we may divide them into those which are ground upon one piece of glass and those in which two separate lenses are included in the same frame. It is with the former set that I wish to deal; the latter have obvious disadvantages, and would not be used if a more satisfactory bifocal lens were introduced.

In the first edition of Noyes, we read of a bifocal convex lens, in which the anterior face of the upper half has been differently ground to the lower. In the last edition of Noyes no mention is made of such lenses, so I suppose we may take it for granted that they were unsatisfactory, and we can see for ourselves why they were so.

I have here two attempts to carry out this plan :—

Dr. T. K. Hamilton, about eighteen months ago, when I first published a note in the *Australian Medical Journal* on the advantages of Pugh's bifocal lenses, referred me to the catalogue of the "Societe des Lunétiers." They made mention of a bifocal convex lens, and when written to replied that the lower lens was merely cemented on. When asked to grind a bifocal lens on one piece of glass, they sent this specimen. This is a periscopic lens of 6·5 D; the upper portion has been ground down to a lens of 3·25. If you look through the upper portion at an object, say, 6 m away, you will observe that the lens will lift it about 2½ feet from the level presented to the other eye. This prismatic effect, of course, renders such a glass useless, and you can see from this diagram how it is produced. [*Vide* Fig. 1.]

In this spectacle frame I have a bifocal lens of the following combination: Upper, + 1·5; lower, + 4·5. This was made for a patient by Messrs. C. and P., of London, at his urgent request, although they warned him that the glass would be useless on account of the prismatic effect. They arrived at the above result by taking a 4·5 bi-convex lens and grinding down the upper half of the anterior surface to — 0·5. This — 0·5 with a + 2·25 posterior surface gives a result of + 1·75 D; that is to say, there is a difference of + 2·75 D between the effect of the upper and lower portions of the lens. This glass has a slightly less prismatic effect than the one just exhibited, but here the objects are lifted about 2 feet at the distance of 6 m.

To overcome this prismatic effect, Mr. Pugh takes, in the first instance, a + 6 D periscopic lens. Into the posterior surface of the upper portion he grinds a — 2·75 D concave, and into the lower portion of the same surface a — 1·5 D. I find in this diagram (3) a smaller lens has been taken. + 4·5 D; into the upper portion of the posterior surface he has introduced — 3 D, and into the lower — 1 D, producing a bifocal lens of + 1·5 above and + 3·5 below. On the large scale you will observe how small the prismatic effect is, whereas in the size of the spectacle it is hardly apparent, and practically does not exist.

As far as I can gather, the effect is all that can be desired, and I would further point out that not only can combinations of convex sphericals be thus satisfactorily carried out, but combinations of — sphericals and of — sphericals with — cylinders or + cylinders. Every combination is thus possible, except + sphericals with + cylinders. The prismatic effect in such is so great that, even though an improvement on previous attempts, they are not fit for practical use.

I have here a few of the combinations that Mr. Pugh has actually carried out for me, and you will see how varied they are. On this page are a few selections :—

R − 8·00^{s} − 0·50 cyl. ax. 180°. − 5·50^{s} − 0·50 cyl. ax. 180°.	L − 8·00^{s} − 2·50 cyl. ax. 180°. − 5·50^{s} − 2·50 cyl. ax. 180°.
R − 3·50 sph. − 1· cyl. ax. 105°. − 1·75 sph. − 1· cyl. ax. 105°.	L − 3·50 sph. − 0·75 cyl. ax. 75°. − 1·75 sph. − 0·75 cyl. ax. 75°.
R − 7·00 sph. − 3· cyl. ax. 180°. − 3·50 sph. − 3· cyl. ax. 180°.	L − 10· sph. − 2· cyl. ax. 140°. − 5· sph. − 2· cyl. ax. 140°.
R − 10· sph. − 3· cyl. ax. 12°. − 4· sph. − 3· cyl. ax. 12°.	L − 10·50 sph. − 2·50 ax. 168°. − 5·00 sph. − 2·50 ax. 168°.
R − 6·^{s} + 0·75 cyl. ax. 80°. − 4·^{s} + 0·75 cyl. ax. 80°.	L − 6·^{s} + 0·75 cyl. 100°. − 4·^{s} + 0·75 cyl. ax. 100°.
R − 3·00 sph. − 1·50 sph.	L − 5·50 sph. − 2·75 sph.
R + 1· sph. + 5· sph.	L + 1· sph. + 5· sph.

Fig. 1

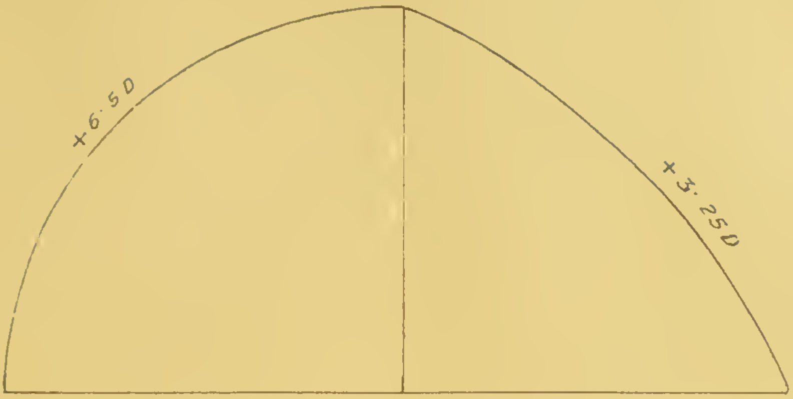
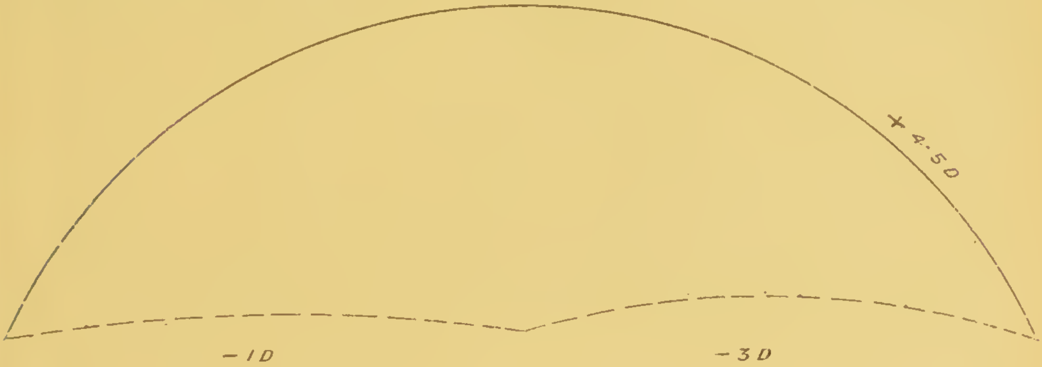
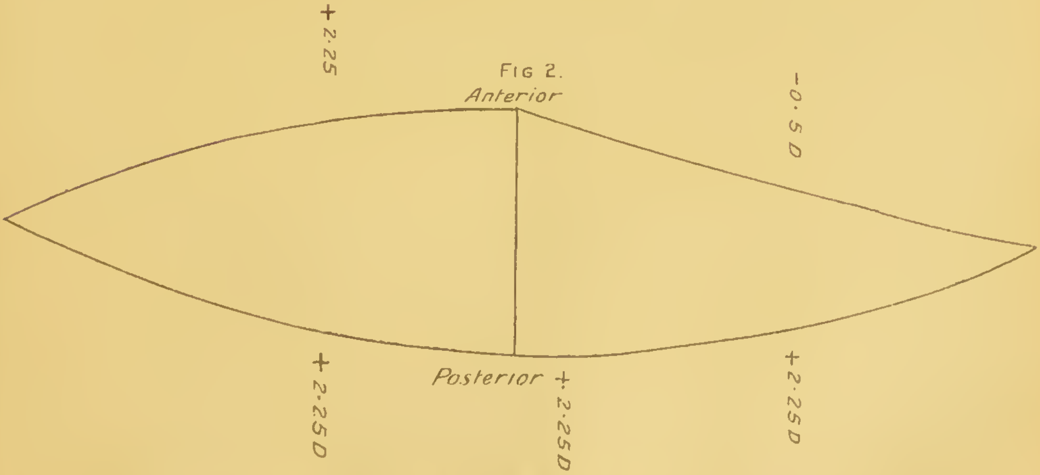


Fig. 3.



On Scale of ordinary Lens

Fig 2.



To illustrate Dr Kent Hughes' paper on Bifocal Lenses.

Made for a patient of Dr. Jackson, Melbourne:—

R	− 6·00 sph. − 2· cyl. ax. 100°.	L	− 4·00 sph. − 2· cyl. ax. 75°.
	− 3·50 sph. − 2· cyl. ax. 100°.		− 2·50 sph. − 2· cyl. ax. 75°.

Made for a patient of Dr. Kenny, Melbourne:—

R	+ 0·75 sph.	L	+ 0·75 sph.
	+ 4·50 sph.		+ 4·50 sph.

Made for a patient of Dr. Gault, Melbourne:—

R	− 5·00 sph.	L	− 7· sph.
	− 3·50 sph.		− 5· sph.

Made for a patient of Dr. Ferguson, Dunedin, New Zealand:—

R	− 2·50 sph. − 2· cyl. ax. 180°.	L	− 1·50 sph. − 3·50 cyl. ax. 30°.
	− 1·00 sph. − 2· cyl. ax. 180°.		− 3·50 cyl. ax. 30°.

Made for a patient of Dr. C. M. Anderson, Christchurch, New Zealand:—

R	− 2·50 sph. − 1· cyl. ax. 90°.	L	− 2·50 sph. − 1· cyl. ax. 90°.
	− 1·00 sph. − 1· cyl. ax. 90°.		− 1·00 sph. − 1· cyl. ax. 90°.

What Mr. Pugh claims to have invented is the compound astigmatic bi-focal lens, not the ordinary sph. bi-focal lens.

SOME REMARKS ON MUSCULAR INSUFFICIENCY.

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When I wrote the following pages I was ignorant or else culpably forgetful of the fact that a former President of the Section, Dr. Symons, had chosen this subject for his opening address at a previous Congress; otherwise I should not have picked on the same subject, which I cannot hope to handle in such an able manner. On reading his admirable paper, which I have only recently done, I find, however, that my remarks touch his at only a few points. This, together with the fact that the topic is one still engaging the attention of ophthalmologists, and on the importance of which opinions widely differ, is my apology for bringing it forward, for I understand that we desire to have before us matters likely to promote discussion, which I believe and hope my remarks are likely to do, and I hope, for my part, to benefit by such discussion and a free criticism. It is not a subject on which I can write with the slightest pretence to authority or originality, and if I appear dogmatic it is merely for the sake of brevity. For this latter reason, also, it is not my intention to go fully into the matter, for that would require a volume; or to adduce more than a very few cases as types of many. I propose to merely touch on a few aspects of the question, and even that not with any attempt at "connectedness"—if I may coin the word.

I am led to the subject because it appears to me one in which we require more light and certainty, for here—in Sydney—as in other places I find that opinions and practice vary greatly. One never feels called on to use prisms or to operate for insufficiency, another does so rarely, another frequently, and so on. Even surgeons who use prisms are not in agreement as to the conditions calling for them, or the condition that they are aiming to produce, or the way in which they should be used—whether for gymnastic purposes or for constant wear. Some surgeons consider that asthenopia due to muscular defects does not exist, and that the term "muscular asthenopia" should be expurgated. Others believe that it may be a source of distress, but merely as a manifestation of lowered general tone of the system, neurasthenia, &c., and not at all as a local trouble. At the other extreme we find some who find in muscular asthenopia a frequent cause, not only of ocular fatigue and headache, but of remote reflex disorders of ovaries, stomach, and other viscera, palpitation of the heart, insomnia, and even chorea and epilepsy. One American surgeon has found to his own satisfaction that muscular insufficiency is a frequent cause of diabetes which is cured by tenotomy, and has aired his views in print.

Both Von Graefe and Donders believed in the practical importance of muscular insufficiency—investigated it in their characteristically thorough manner, and have given us the benefit of their conclusions—chiefly the convergence insufficiency of myopia, though they noted its not uncommon occurrence in other forms of refraction. Against them some surgeons now urge that at that time the immense importance of astigmatism as a factor in causing eye strain was not appreciated, and that if it had been, Von Graefe and Donders would have found in this the true explanation of many, if not all, of their "muscular" cases. It appears to me, however, asking too much to expect us to believe that two such men—one the greatest ophthalmic clinician and most acute observer the world has ever seen, the other our classical authority on refraction and accommodation—could have fallen into such an error. It was not as though they knew nothing of astigmatism, though it would appear that they did not give it the prominence it deserves as a cause of asthenopia. On the other hand, when we find men like Javal and Roosa—the latter a pupil of Von Graefe's—and many others, who for years used and strongly advocated prisms, giving them up altogether, and stating emphatically that all symptoms of so-called muscular asthenopia are really due to uncorrected or faultily corrected ametropia—chiefly astigmatism—one must pause to give due weight to their arguments and conclusions before being so bold as to disagree.

Possibly the suggestion is not a new one, but is it not likely that muscular asthenopia is more frequent now than formerly, and that it will become more and more common? We live nowadays under conditions of strain and excessive wear and tear of mind and body, of excitement, hurry and worry, and acute nervous tension that even our fathers did not know. Modern conditions of life have developed many new disorders; may we not be developing or exaggerating muscular asthenopia also? If we are to form an opinion from the prominence given to the subject in their writings and discussions, do we not find that it is amongst the Americans especially that muscular asthenopia so frequently calls for treatment? In no country in the world do men live so fast or work under such high pressure as in America—in the Eastern States, at any rate; nowhere is the spirit of emulation greater, the stress of competition keener; nowhere do they appear to so neglect the laws of health in the race of life.

Is it that the occurrence or frequency of muscular asthenopia is largely a matter of environment—mode of life—and so becoming a matter of racial aspect? Our Australian type is not yet developed, but it appears to me we are tending to one something between that of the Anglo-Saxon and the American—so far as the latter, which is composed of such heterogeneous elements, can be considered as a distinct type.

This is getting discursive, but it has led up to what I wish to emphasise—viz., that it is amongst neurotic, excitable persons of quick and vivid perceptions, easily depressed, and as quickly and transiently exalted, that we find most trouble from muscular as we do also from refractive and accommodative errors. Of course, I do not mean to say that these defects are more common in such people, but that these suffer more from them, and experience real distress from slight defects which on a more evenly-balanced or phlegmatic person would produce no impression. Be it remembered also that it is in the lesser degrees of muscular insufficiency as it is in the slighter degrees of ametropia that we frequently find the most distressing symptoms. As in high degrees of ametropia, so when muscular defects are great there is no effort to obtain the impossible—in this case binocular vision. Consequently there is no asthenopia, and the only complaint is of strabismus or diplopia. If the diplopia be great the false image falls on a peripheral part of the retina, and the patient either soon learns to suppress it, or his chief trouble is in locating objects. But if the defect be slight the diplopia is more annoying, and if it be possible to get fusion the impulse to it is strong. The strain necessary for this produces symptoms of asthenopia, and the more neurotic or neurasthenic the patient is the more distressing are the symptoms. It is fortunate that the symptoms are associated with the lesser degrees of insufficiency, for only weak prisms can be borne. When other means of treating these cases fail—viz., through correction of any existing ametropia, improvement of general tone, removal as far as possible of adverse conditions and of any discernible sources of reflex irritation—it seems to me that we have in prisms a rational and justifiable means of treatment. Results, however, though often most gratifying to both patient and surgeon, are also frequently disappointing, which is perhaps one reason why many surgeons are sceptical as to their value, and have given them up.

Speaking now of insufficiency of convergence, we find this present in a great many patients without producing any symptoms, and here, of course, no special treatment is called for. Maddox, Berry, Stevens, Noyes, and others consider a relative insufficiency of 4° or 5° for a distance of 30 c.m. quite normal, and Berry has shown how for every additional dioptre of accommodation convergence lags more and more behind. On this point Stevens says, "In the best condition of equilibrium there will generally appear exophoria in accommodation of 4° or 5°, and this should be taken into account in adjustment of glasses for reading, for if the exophoria in accommodation as shown by a prism is abolished the patient is given a practical esophoria." He goes on to say that this important point in the adjustment of reading glasses often demands careful attention, and gives a very simple method for finding the proper adjustment. The phosometer is adjusted for exophoria 4° or 5°, and the patient looks at a dot on a card at reading distance with his correcting glasses in a trial frame. The interpupillary distance is then altered by the adjustable screw in the trial frame until the two dots seen through the prisms of the phosometer are exactly vertical. The interpupillary distance thus shown is that at which the optical centres of the two glasses should stand to give complete comfort to the patient.

I have frequently found this hint very useful. It has doubtless happened to all of us, after carefully prescribing proper and normally centred reading glasses, to be told subsequently that the patient had found more comfort from a pair he had got from a man in the street for 2s. 6d. Excluding cases in which perhaps one has not been careful enough of the good rule of never giving too strong glasses for presbyopia, you may find that the only difference between the half-crown pair and yours is in the intercentral distance. These cases are rather mortifying, which is the reason for this digression to allude to them.

Cases of ametropia are being constantly met with where careful correction does not relieve asthenopia. The normal relation of accommodation to convergence is usually found, but in a certain proportion of cases we find exceptions. The hypermetrope learns the art of accommodating in excess of convergence; the myope learns to converge without accommodating (but that this adaptation is often not acquired is evidenced by the convergent squint of hypermetropia, and the divergent squint of myopia). If we deprive the hypermetrope of the necessity for excessive accommodation or the myope for excessive convergence by correcting his ametropia, he has to acquire a new co-ordination of accommodation and convergence. This is doubtless one reason for discomfort when glasses are first worn, because the discomfort disappears when the new relation is established, which it is in most cases, though there are many exceptions; and because some disregard the exceptions is, perhaps, the chief explanation of the difference of opinion amongst ophthalmic surgeons as to the propriety of using exceptional treatment. Neurotics especially are deficient in adaptive power. That real strain of the eye muscles or dissociation of the accustomed relations of accommodation and convergence does cause discomfort, anyone can prove to himself by the simple experiment of reading with prisms.

Before going further, let me define my own opinion and practice, acknowledging at the same time the desire for something better. At one time I looked for and treated muscular weaknesses, specially whenever after elimination of other possible factors they seemed to me to be causing trouble. Gradually led by adverse opinions, I fell into the habit of disregarding them, and for years scarcely ever prescribed a prism or decentred lens. For some time I have been forced back to a belief in the reality of asthenopia of muscular origin; and when I find asthenopia causing real discomfort and sometimes even incapacity for work, which most careful correction of ametropia and general treatment fail to relieve, vanish by the use of prisms, I do not hesitate to prescribe them, preferring to adapt the glasses to the patient rather than vainly persist in trying to adapt the patient to the glasses. I generally order for constant wear prisms of strength equal to the amount of exophoria for distance—if any—*plus* about half the manifest insufficiency for the working point. Thus if a patient (with symptoms by other means unrelieved) has exophoria for distance of 4°, and for working point 8°, I give 4° prism base inwards for each eye. If there were parallelism for distance, I should give 2° prisms for each eye. It is objected that it is wrong in principle to give prisms for constant use, as they have the effect of further increasing the defect. That in many cases this is not so I have satisfied myself over and over again by finding that after wearing such glasses for a time the exophoria is diminished, the apparent explanation being that by assisting and encouraging the muscles to perform the needful work they are exercised and stimulated instead of being fatigued and worn out by excessive effort. Noyes says that when recovery takes place in such cases the cause of the defect lay in spasm of the opposing muscles. I make the exception of rarely giving divergent prisms for *distant* vision in myopia. Of dyerising or gymnastic exercises, I have little experience. I have tried them, but find in practice that patients do not care for the expense of buying the necessary prisms, and if one lends them the patients forget to bring them back, or if they undertake to do as suggested they either neglect the exercises or else, finding the prisms relieve them, they wear them constantly.

Instead of using prisms or prismospheres, decentering or displacing may in some cases be substituted, though the former method is more exact. There is a point in connection with the use of prismospheres which appears to be often missed. Maddox and Perceval have shown that when a spherical lens is used in combination with a prism the usual line traverses an excentric portion of the lens, so that the deviating effect of this potential decentration must be taken into account. The same applies when the two glasses are fused into one prismosphere or decentred lens. When concave prismospheres are used their effect on the deviation of the fixation lines is always less than that of the prism, but with convex prismospheres their action is greater or less than that of plane prisms according as the distance of the object is greater or less than the focal length of the lens. In the formula and table that Maddox gives for corresponding decentration he does not allow for these differences. Perceval, however, pleads for exactness, and maintains that when using a sphere and prism for investigating or treating the condition of the muscles we must not neglect the deviating power of the lens in the combination. He gives (Knapp's

Archives, vol. xx., part 2) a table in which he has calculated the deviation for every combination of prism and lens, concave and convex. To show how considerable the variation may be, a prism of $5^{\circ}\ 30'^{d}$ combined with a convex lens of 9 D has the same deviating effect as a plano prism of $7^{\circ}\ 17'^{d}$, but with a concave lens of 9 D a prism of $9^{\circ}\ 4'^{d}$ would be required to induce the same deviation.

An ideal examination of our cases would include the testing of the absolute maximum and minimum of convergence—including the position of rest—and also the relative amount after correction of ametropia and careful centering of the refraction-correcting lenses for both distance and near; also the testing of heterophoria of all kinds both for distance and near. (Correcting-glasses do not appreciably affect the testing for distance—as Berry has stated). As the muscular power varies at different times, it is necessary to test repeatedly. Fortunately for our patience, it is not necessary for practical purposes to investigate all these conditions except in very occasional cases. Generally it is sufficient to determine the position of rest for distance, and the maximum of relative convergence, or the degree of insufficiency for the working distance. In distance testing, I generally use Stevens' phosometer in preference to Maddox's rod. The increase in prismatic effect is gradual and even, and the measuring of the amount of exo- eso- and hyper- phoria only occupies two or three minutes, all points of superiority over Maddox's rod test, which is only used in certain cases as a check test. The gradual steady increase in the prism strength and the rapid testing are important. If we substitute different prisms by hand the eyes oscillate in the intervals between the interposing of the different lenses, and we do not get the power of the muscles so fully determined, and if the examination is prolonged the muscles may become wearied, or spasm may be induced, which spoils the results. This test also sometimes suggests the existence of vertical insufficiency, because, as Stevens and Noyes state, if we find esophoria for distance with insufficiency of convergence for the working point, it generally indicates anomalous tension in the vertically acting muscles. For testing at the working distance, Stevens' phosometer may also be used, but for this I prefer Von Graefe's equilibrium test, which, though not free from objections, is generally sufficient for practical purposes. The cover test, as a rough method, and the bonnet pin, or, as I prefer, a fine silver wire for testing, absolute near point of convergence, or as an indication as to whether the defect is shown more in one eye than the other, are other methods occasionally used.

Insufficiency of convergence is the defect most frequently in evidence, though Noyes found in 100 cases of muscular defect 75 per cent. were cases of insufficiency of the externi, or latent convergence. This condition is very common, but is generally believed, unless excessive, to cause no symptoms. Noyes dissents from this, for he advises that whenever, at 20 feet, the abductive power is low—say 5° or less (the normal is only 7° or 8°), and this with or without ametropic correction—that prisms with bases outwards should be worn constantly. He adds that this will be true, notwithstanding insufficiency of the interni for near work. Personally, I have never used converging prisms in such cases. In another place Noyes states that such a condition—esophoria for distance with insufficiency of convergence for near—is generally a sign of anomalous tension in the vertically acting muscles, as I mentioned a minute or two ago.

According to Hansen, Grut, and Schiotz, the normal position of rest is one of divergence. Berry, on the other hand (Trans. Ophthal. Socy. xi., p. 156), after numerous testings, found that in emmetropia and hypermetropia the axes were generally parallel, and that latent convergence was commoner than divergence. In myopia parallelism was less frequent. Divergence and convergence were observed with the same frequency, but on the whole more slight degrees of divergence. For all practical purposes he considers that parallelism may be regarded as the normal position of rest. Maddox and Foster are of the same opinion, and this is what we should, *a priori*, suppose.

Perceval *(loc. cit.)* divides insufficiency of convergence into two classes:—

I. Those characterised by feebleness of one or more muscles, and consequent impairment of movement.

II. Those in which there is a normal or even excessive range of movement, but in an unavailable situation; those in which there is exophoria for distance.

In the first class of cases he deprecates the use of prisms to *relieve* the defect, and recommends dyerising or gymnastic exercises—preferably the latter. In the second group of cases, some surgeons correct all the muscular insufficiency up to the limit of strength that can be borne—*i.e.*, up to 4°, or at most 5°, to each eye. This method does not appear to me to be rational. Others, again, aim at getting exemption from fatigue, and follow Landolt's dictum that only one-third of total convergence can be continuously used, and two-thirds must be in reserve. There appears to be a fallacy here, for it is the relative and not the total range of convergence that we must consider, and the above-named ratio may and does vary with different degrees of accommodation. Moreover, patients present many individual peculiarities in their power of maintaining continuous effort. Other surgeons only use prisms strong enough to relieve the exophoria for distance. This does not appear to me sufficient in cases where there is considerable convergence insufficiency, and only very slight exophoria for distance. There may be no exophoria for distance, but this would come into the first group of cases.

The following is an example of very slight insufficiency causing asthenopia:—

Miss H., aged seventeen, school girl; strong, healthy girl; always enjoyed good health; some years ago was treated by an ophthalmic surgeon for inflammation of the eyelids; suffered and still suffers from headache aggravated by reading. and often wakes up in the morning with headache which is mostly occipital (occipital headache is said to be suggestive of muscular asthenopia); dark glasses without power were ordered after testing of refraction. Some time afterwards, the headache continuing, she went elsewhere, and got 0·5 D spherical for reading. These gave no relief. I found V = 5/6 in either eye, and patient testing under full atropinisation gave no further improvement with any glasses. Retinoscopy showed emmetropia. Exophoria for 5 metres = 2°. Insufficiency for 12 inches = 6°. Prisms of 2° base to nose ordered for each eye. In three weeks reported complete relief, so much so that she wore the glasses constantly. I told her not to do this. She came occasionally for three months subsequently, as I was treating her also for follicular conjunctivitis. At the end of this period the latter condition was cured, and at each visit she reported total absence of headache.

The next is a case of heterophoria with astigmatism:—

A. H.J., aged thirty-nine, merchant. Nervous temperament, and somewhat dyspeptic, otherwise healthy; has suffered severely from headache for years, about twice a week. "Sick headaches" quite incapacitate him for work. They generally come in the morning, and any effort at reading or writing is impossible.

V. R. with + 0·75 D cyl. axis vertical = 5/5.
L. with + 0·75 D cyl. axis 20° from vertical = 5/5.

Homatropine R. $\frac{+\,0{\cdot}75\text{ D}}{+\,0{\cdot}75\text{ D}}$ cyl. axis vertical = 5/5.
L. $\frac{+\,0\,5\text{ D}}{+\,0\,5\text{ D}}$ cyl. axis 20° from vertical = 5/5.

Exophoria for distance = 2°. Insufficiency for 12 inches = 8° prism.

Ordered cylinders only for constant wear.

Two months afterwards reported very slight relief. Headaches not quite so severe, but just as frequent, and not able to work when he had them.

Ordered for distance prisms of 1° base to nose to each eye added to the cylinders, and for working added 2° prisms to each eye and + 0·5 D spherical.

Six months afterwards reports almost complete freedom from headaches ever since getting the prisms, and is able to work constantly without trouble. Had a bad headache this morning, which made him think they were returning, and brought him to me. Found that yesterday he had mislaid and gone without his glasses for a time.

With regard to the vertically acting muscles, we frequently meet with slight defects of equilibrium without any resulting discomfort, but in a few instances the asthenopia appears to depend on this condition.

The following is an interesting case :—

J.P.A., aged twenty-eight, clerk. Always myopic; has worn glasses since twelve years old; has changed for stronger ones several times. Last pair, got four or five years ago, are — 7 D. After working long, eyes ache severely, and he has to desist. Pain is in temple and behind the eyes.

V.R.E. with — 7 D = 5/9. Retinoscopy confirms.
L.E „ — 7 D = 5/9. „ „

Three concentric myopic crescents.

Left hyperphoria, $3\frac{1}{2}$° ; exophoria, 2°.

Two days afterwards, left hyperphoria, 5° ; exophoria, 2°. With his correction has not binocular vision (Snellen's coloured glass test). With — 7 D in trial frame to each eye, and prisms of 2° base up before right eye and base downwards before left eye, has binocular vision, but spaces between the red and green letters are not equal until the interpupillary distance of lenses is 70°, which is the amount of decentering indicated by his exophoria. Without the vertical prisms he only occasionally gets a momentary glimpse of both red and green letters, and they are on different levels. I ordered glasses decentred as above combined with the above prisms. With these he has binocular vision and equal spacing and height between the letters, and his discomfort has disappeared entirely. He can work all day without trouble.

Insufficiency of the obliques or the obliquely acting muscles is occasionally met with. Le Conte and Meissner have shown that outward torsion of the eyes occurs in convergence, and Maddox has described a ready method of demonstrating this. Savage maintains that a certain amount of corrective torsion is necessary when oblique astigmatism exists in both eyes, since the retinal images of vertical objects are not themselves vertical, and that correcting cylinders which make these images vertical alter the torsional requirements. Harold Wilson (*Knapp's Archives*, vol. xxiii., No. 3) questions both his premises and conclusions; but Savage has replied to this critic, maintaining the correctness of his own views.

Maddox allows that oblique insufficiency may cause asthenopia, though he states that such cases are not familiar to him (*Ophthalmic Review*, June, 1894). Most American writers record cases of trouble attributable to this form of defect, and I have seen a few cases myself of trouble for which I can offer no other explanation. The following is one :—

S.T., *ætat.* forty-two, a medical man, with a considerable knowledge of the eye and ophthalmic optics, and who had made a special study of his own case, came to me some years ago complaining of ocular weariness and confusion after a moderate spell at studying. The trouble had existed all his life, and he was wearing constantly glasses prescribed by an eminent British specialist for his oblique hypermetropic astigmatism. His refraction had been tested by several London men, and by Landolt in Paris; and neither they nor I could offer any improvement in the strength of his glasses. As there was a considerable amount of facial asymmetry, I had the lenses carefully centred, which had not been attended to in his spectacles. Still the trouble persisted, and some time afterwards he returned to me, when I discovered that he had a considerable amount of latent torsional insufficiency which I had not noticed before, because I had not examined for it. I am inclined to believe that this must be the cause of his asthenopia, and my friend concurs in this opinion. Unfortunately, nothing can be done to relieve it. This condition may be detected by Maddox's rod or by Savage's modification of Maddox's double prism.

Finally, we have to consider in some cases of muscular insufficiency the question of operative interference. Of this my experience is small, but in the few cases in which I have operated the results have been satisfactory. Cases in which operation is indicated are few (that is excluding cases of actual squint), because not only must the insufficiency be too great for relief by prisms, but yet the impulse to binocular fusion must be strong, and, as Noyes points out with decided myopia, with anisometropia or monocular amblyopia the latter condition is imperfectly developed or wanting. "Cases which have a marked abridgment of muscular power at both ends of the visual line are [also] unsuitable for operation" (Noyes). Landolt says the same in different words, and excludes also neurasthenics and neuropathics as ineligible for operation. Swanzy and others recognise as suitable only those cases in which either there is divergent strabismus or those in which the externi can overcome the power of a prism of 10″ or upwards, base to nose, when the patient looks at a candle flame 3 metres distant. He defines the rules for estimating and controlling the effect of the operation (Handbook of the Disease of the Eye, 5th edition, pp. 540 and 541), which differ slightly only from the rules laid down nearly forty years ago by Von Graefe, whose remarks applied almost exclusively to cases of insufficiency of the interni. In high myopia operation is very permissible when repeated examinations, at considerable intervals, show that the condition is constant. In America the tenotomy mania is startling. Stevens, for instance, operates for hyperphoria of 1°, and in 1890 had performed tenotomy for insufficiency of the externi 2,000 times! As Ernest Clark pithily remarks, "Surely all the insufficiencies of the externi in the world must have found their way to him. It will be very interesting to learn in, say, ten years' time, the further history of those patients who have been so liberally dosed with tenotomy." Of so-called graduated tenotomies I have no experience.

The following is a case in which excellent results followed tenotomy:—

R.C., aged eighteen, strong, well-built young fellow, complains of eyes being sore and aching after reading. Slight granular conjunctivitis:—

V. R.E. 5/5 (?) M.H. = 1D. L. 5/5 ? unimproved.

Atropine R.E. $\dfrac{+ 1\text{ D}}{+ 0{\cdot}5\text{ D}}$ cyl. axis 60° 5/5.

L.E. + 1D sph. 5/5.

Ordered constant—

R.E. $\dfrac{+ 0{\cdot}5\text{ D}}{+ 0{\cdot}5\text{ D}}$ cyl. axis 60°.

L.E. + 0·5 D sph., with zinc sulphate, and boric lotion, and bi-weekly touching of lids with bluestone.

A few weeks afterwards first complained of slight doubling of print, and that "objects sometimes appeared to be in a wrong place." There is marked insufficiency of convergence, but it is not measurable, as one eye diverges and immediately suppresses. After exercising with coloured glass for a week, diplopia was elicited, and found to be equal to 16° prism at 5 yards distance. Right eye chiefly at fault. After repeated examinations, and finding the condition tolerably constant, performed tenotomy on right external rectus under cocain. Immediately after the operation there was no diplopia (no convergence) on holding a lighted taper 15° to left of middle line. A month afterwards there was still 5° or 6° of latent divergence for distance, and 16° for 12 inches as tested by Von Graefe's test, though with a strong effort he could momentarily fuse the distant images. Exercise with prisms failing to increase the converging power, a month afterwards I tenotomised the left external rectus. A month afterwards had perfect equilibrium for both distance and 12 inches. The granulations continued troublesome for some months. Three months after the last operation granulations cured, binocular vision, no diplopia or asthenopia. Five months after that his sister told me his eyes sometimes ached a little. On seeing him I found he had given up using his glasses. No diplopia, binocular vision, and orthophoria. Ordered to resume his glasses. Twelve months afterwards reports no trouble of any kind. Has been for eighteen months in business (which his asthenopia had compelled him to relinquish before the operations) and working hard.

A sister of this patient had a lesser degree of convergence insufficiency with symptoms of asthenopia completely relieved by prisms.

Such, gentlemen, are a few remarks on some points in connection with muscular insufficiency—as I forewarned you, somewhat scrappy and disconnected. The subject is to me an interesting one because of its complexity and intricacy, and I look to you for some more light and certainty in the matter of treatment. To read many writers on the subject is to make confusion worse confounded, and a thorough understanding is not simplified by the different terms such as centrad, prism dioptre, metre angles, deviating angle, angle of aperture, &c., which different authors employ, however scientifically satisfactory the newer terms may be.

DISCUSSION ON MUSCULAR INSUFFICIENCY.

Dr. Symons spoke as to his own experience. He usually tests the convergence and divergence by horizontal prisms. There is much difference of opinion as to the normal amount of convergence which should be expected, but he considers that a patient showing a power of convergence equal to 15° or more free from strain; that between 12° and 15° he may have symptoms of insufficiency of the internal recti, and that under 12° these symptoms are to be expected and calculated upon; that the normal in divergence is 6° or 7°, under 5° giving strain: all, of course, depending on the individual resisting power. Does not think that practising with an object moved to and from the eyes in their plane gives benefit. Always treats the case with prism gymnastics. Had only seen two cases, both females, who were not improved by prism exercise, each being unable to overcome more than 3° base out. They refused operation, and were treated by constant use of prisms 2½° before each eye in conjunction with correcting lenses for their refractional errors. Had only operated on one case, and that with only partial benefit. Had long ago laid aside Maddox's rod, because he could not see how a condition of vision in which the association of the two eyes was disturbed could give information as to the behaviour of the eyes when normally associated in vision.

Dr. Kent Hughes: Personally, I have never ordered prisms, and, though I see a fairly large number of refraction cases every week, I can only call to mind two instances in which the result was not satisfactory. One of them had been ordered a No. 2 prism by Mr. Julor. The other never came back to me—so, perhaps, he may have required a prism. Anyhow, he was a presbyope, and his lenses were decentred as usual. The strength of the ocular muscles must and do vary from day to day, so I cannot see what we gain from a slight aid in the shape of a prism. If we fully correct all the hypermetropia in patients requiring glasses there will be less use of prisms.

Dr. Kenny had found prisms so unsatisfactory that he has ceased to order them. He decentres lenses and uses a Maddox rod.

Dr. Orr corrects refraction and decentres lenses.

Dr. Lockhart Gibson said he endeavoured to correct latent as well as the manifest hypermetropia, and is very careful not to over-correct myopia. He could not help thinking that many hypermetropes made comfortable with prisms and correction would have, with patience, borne a somewhat higher correction, and been then able to do without prisms. He did not order prisms. He had listened with great interest to Dr. Pockley's paper, and complimented him upon it. He had observed that the removal of adenoid growths enabled some children to sustain without discomfort an accommodative effort which had been previously impossible. Pregnant women also often laid aside after confinement hypermetropic glasses which had relieved them of headaches and asthenopia during their pregnancy. He seldom used a mydriatic for his refraction cases, and quoted cases where the full use of atropia had apparently excited accommodation, thus introducing the very fallacy we used it for eliminating. He was not very particular to obtain good distant vision as an immediate result of correcting hypermetropes, but liked them to see $\frac{6}{6}$ after a month's constant wear.

Dr. Kirkland said he seldom used prisms.

Dr. Ferguson considers insufficiency in young people to be usually due to some condition of health, and temporary, but that for presbyopia lenses should be decentred inwards to make up for the alteration in the relationship of accommodation and convergence.

Dr. Pockley, in reply, said that the fact that prisms were prescribed so infrequently justified his paper. He does not order prisms until other means of treatment have been exhausted. Thinks decentering lenses for reading is often valuable.

DISCUSSION ON SIGHT-TESTING IN THE RAILWAY AND MARINE SERVICE.

Dr. H. L. Ferguson, New Zealand (President of Section III.), drew attention to the resolutions passed at the previous Congress bearing on the proper testing of the eyesight of men in the railway and marine service. The resolutions [which were read] urged upon the Governments, railway and marine authorities, and shipping companies of the different colonies, that steps should be taken to provide for examinations by properly qualified ophthalmic surgeons. He explained that the resolutions had been forwarded to the authorities concerned, and was pleased to say that they had in a measure borne fruit. Government systems of examination had been introduced, and, with regard to the marine service, the Union Steamship Company of New Zealand had adopted regulations which were a very great improvement on having no regulations at all; but he was sorry that they had not been submitted to him before they had been put into force. However, they were better even now than the regulations originally drafted, which had been amended after strong remonstrance; but they missed the important point of not stating what amount of ametropia would cause disqualification. As the regulations stood, in this respect they involved very great hardship in the case of many men who were passed at home, and on coming here found that they were required to pass a higher standard. The New Zealand standard was very much higher than that of the Board of Trade in England; so that after passing there a man might come here and fail, thus losing his position in the service. Since the New Zealand regulations were passed several cases had come under his notice in which men coming from England had been unable to pass the examination on account of ametropia. What was wanted was uniformity. He blamed the Board of Trade for admitting to the service boys who had ametropia to such an extent that when they were twenty or thirty years of age they would be thoroughly unfit for duty. It was most reprehensible to find a body like the Board of Trade adopting such an exceedingly poor standard. Under its system of examination candidates were allowed to use either eye, so that they might be absolutely blind in one eye and still succeed in passing the standard. He instanced a case in which a man who was absolutely blind in one eye had been passed by the Board and allowed to run a vessel till he was stopped by the Union Steamship Company's examiner. That man could and would no doubt pass the Board of Trade's examination again. In other respects the New Zealand regulations were practically the same as those of the Board of Trade; in fact, the former were in most particulars a copy of the latter. A difficulty in connection with this was that the tests had become so well-known among seamen that in certain cases they had been learned by heart or written down on shirt-cuffs. The examination required by the New Zealand Government could be passed by many with the use of one eye. It would be possible to pass it with a large amount of defective refraction. Personally, he could make himself ametropic and still pass it. The low standard of the Board of Trade's examination was a serious matter; and while its effect was to admit boys whose sight was so defective that they could not long continue to discharge their duties with any degree of efficiency and safety, it behoved Congress to urge the necessary reforms.

Dr. J. Thomson (President of the Congress) referred to the difficulties which had been met with in introducing an effective sight-testing system in Queensland. The system he adopted was at first considered too severe by the railway people, and even by some of the medical examiners, but eventually it was accepted as the standard. Still he found, like Dr. Ferguson, that the test could be learnt off by heart. For colour testing he had tried the new scheme by means of a disc, two of which he had obtained from Philadelphia. He had met with much difficulty in testing colour vision owing to the fact that many men would not believe they were colour blind. Of 195 men examined by him last year seven were colour blind, but of 100 examined this year only one was so affected. He was convinced that the ailment was incurable, and those who were affected could often be detected at once by the peculiar way in which they handled or looked at the test wools. Even men who were very badly affected could distinguish the colours of wools which they had obtained themselves and practised with. Then, again, men who made great mistakes with the test wools could be found to readily and correctly distinguish the red and green flags. Instances had even occurred in which men who were actually colour blind had brought him certificates to the contrary, which they had managed to secure from medical men whom they had satisfied under examination. The Queensland railway authorities were perfectly willing to adhere to the regulations recommended, and he (Dr. Thomson) was desirous of doing all he could with a view to making the tests still more effective.

Dr. Sprott (Tasmania) said he had presented the resolutions of the previous Congress to the Government and the railway authorities of his colony, and an examiner had been appointed, but they did not go further than colour testing. One man who was actually colour blind had been found acting at an important railway station in the colony. The Commissioner removed him from that responsible position, but considerately retained him in the service in another capacity. If any further resolutions were passed by Congress bearing on the matter under discussion, he would gladly urge them upon the Tasmanian Government and railway authorities.

Dr. Pockley (Sydney) recognised the necessity of having a uniform fixed standard if only for the protection of examiners. One's sympathies were apt to be very much enlisted by a man who had perhaps spent about twenty years in the service, then to find he could not pass the standard. Many such men would break down and become hysterical—all of which was hard for the examiner to resist, unless he was backed up by a recognised fixed standard, without which leniency might result in further defects being gradually allowed to creep in. The marine boards insisted on perfect colour vision for officers, who are also expected to read $\frac{6}{6}$ with either eye, but no attention was paid to ametropia or to the necessity for re-examination. He knew a man with eight diopters of myopia whose duty was to go along the railways as a fettler, and in other respects to see if everything was safe. (*Laughter.*) With regard to testing for colour blindness, he had noted cases in which men failed simply through ignorance of shades of colour. A suggestion had been made in the old country to substitute blue lights for the green ones, and, in his opinion, if that were done, they would not have to refuse so many applicants for admission to the service. Should the conference pass any further resolutions relative to the question under notice, he would be pleased to place them before the Government, Railway Commissioners, and Marine Board of New South Wales.

Dr. Symons (Adelaide) urged the desirableness of endeavouring to bring about a uniform system of testing throughout the colonies, so that an applicant would not be refused in one colony and passed in another. He had recommended that men already in the railway service in Adelaide should be allowed to remain if they could read $\frac{6}{12}$ with each eye, had colour correctness, and were without any eye disease; and that those entering the service should be required to pass $\frac{6}{6}$ with the same conditions as regarded colour sight and absence of eye disease. The colour testing was done with wools, but in addition the eyes were examined by an ophthalmoscope. There was no provision for re-examination periodically. The running staff (locomotive and traffic) were required to read $\frac{6}{6}$, and the labourers $\frac{6}{6}$ with one eye and $\frac{6}{9}$ with the other. He also recommended that any employee who was not on the running staff should be allowed to use artificial aid, such as spectacles or glasses, if they desired.

Dr. Kenny (Victoria) said most of his experience had been with men who, after being rejected, had come to him to seek certificates. In this way several cases of injustice had come under his notice. A boy had come to him who could read $\frac{6}{6}$ easily with one eye and fairly with the other. Then he went to the examiner again, where he was tested with a varnished board which the sun affected in such a way that the lad could see nothing, and so, becoming nervously stupid, he invented a whole set of letters for himself, and of course hopelessly failed. Circumstances like that showed that if they had a good standard they must also have good examiners. Another matter to which reference should be made was the great indistinctness of some of the Snellen's type test prints, which were worse than a blurred and badly printed common newspaper. With regard to what Dr. Thomson had said about men being able to distinguish the red and green flags, though they had failed in the colour test, that was accounted for by the greater intensity of colour, as was also the case with the red and green lights. The red light could be seen much further away than the green. Concerning the suggestion to substitute a blue light for the green, he did not think that would do under any circumstances, as the blue light would be found to be far less suitable than the green. So far as they (the members of the Section) could without appearing to have a desire to create positions for themselves, they should strongly urge that qualified oculists should be appointed to carry out the tests. Under any other system unsuitable men would continue to be let into the service and the best men kept out. [A copy of the Victorian Railway Colour Test Regulations was read by Dr. Kenny and laid upon the table.]

Dr. Kent Hughes (Melbourne) counselled the Congress against the danger of attempting to do too much at a time in the way of reforming matters in connection with the test system. If Victorian conditions with regard to ametropia were enforced, the effect would be to reduce the number of railway employees to an inconvenient degree.

Dr. Ferguson: A large number of the present railway men would have to go under such conditions.

Dr. Kent Hughes considered that the amount of ametropia allowed by the previous Congress was too small. Besides, he did not deem it such a dangerous thing to allow artificial aid such as spectacles to be employed by certain classes of railway men, because a man may only have $\frac{6}{9}$ without glasses and $\frac{6}{6}$ with them.

Dr. Kenny: But the danger is in the effect of rain, dust, smoke, &c., on the glasses.

Dr. Kent Hughes was afraid that the Congress might ask too much, and thus cause the railway authorities to discountenance requests made to them, with the result that points of more importance might thus be lost to the profession. With reference to colour testing, he was of opinion that the use of lights would be preferable to wools, as sets of the latter could be purchased for practising with until candidates were able to deceive the examiner.

Dr. Gibson: Nothing but the matching of colours was of any real value as a test. Matching of wools seemed best, but the *matching* of lights might also be of value. Blue could be seen from a very much shorter distance than either red or green.

Dr. Thomson explained that the lights had been tried for testing, but had not been found so satisfactory as the wools, which, it must be remembered, had to be matched.

Dr. Kent Hughes admitted that the matching of the wools was an important point in testing, but said his suggestion for testing with lights was made with a view to catching men who had been practising with the wools. In Victoria a number of candidates rejected by the examiners had been subsequently admitted to the service after undergoing a practical test by the department.

Dr. Ferguson: And are these men in the department now?

Dr. Kent Hughes: Yes.

Dr. Kenny: It is evident that some of those who had adopted the test regulations had not strictly adhered to them.

Dr. Kent Hughes considered it most important that there should be a periodical inspection, say every five years, which should apply to the whole service.

Dr. Kirkland mentioned the case of a ship's mate of eighteen years' experience whom he found to be well up to the requirements of the regulations, but who nevertheless had been rejected by the Board of Trade's examiner. Subsequently he was examined again for the Board and again rejected, whereupon he brought an action against it for a sum of money and recovered compensation to the extent of £200. After that he was once more examined by the Board and passed. Such cases illustrate the necessity for having skilled examiners.

Dr. Ferguson said that, though the discussion had been very interesting and instructive, they had not gone far towards determining whether any further steps should be taken to improve the systems practised in sight-testing. He thought the suggestion of Dr. Symons for a uniform or federal standard was one that they should strive to give effect to, both as regards the railway and marine service. It was obvious that the Victorian Government did not carry out the regulations in the spirit intended. It was of little use for a medical examiner to test a man and reject him if the same applicant could get admission to the service by passing the practical test of the department. From what he had heard and observed of Dr. Thomson's methods he was satisfied that few took more trouble than he did with a view to carrying out thorough tests—so much so that if Dr. Thomson rejected a man after putting him through the colour test he (Dr. Ferguson) would confidently endorse his decision. In that respect Dr. Thomson must be regarded as a specialist—any man who had so much experience in testing men for the railway service could fairly claim to be a specialist.

Dr. Gibson: He certainly is a specialist in that respect.

Dr. Ferguson pointed out that a difficulty they had to face was the fact that Governments were not disposed to employ specialists to carry out sight-testing, but preferred to have a general examination—optical, aural, and physical. In his opinion, the best course to be taken would be to refer the whole matter to a committee. He moved that the committee appointed to frame resolutions relative to the discussion which had taken place on the "Eyesight of School Children" be also empowered to consider the matter of sight-testing, with a view to urging upon the authorities interested the desirableness of adopting and strictly enforcing the regulations suggested by Congress.

Dr. Kenny seconded the motion, which was carried unanimously.

ON RETINOSCOPY.

By M. J. Symons, M.D. Edin.,
Adelaide.

Mr. President and Gentlemen,—

I am desirous of complimenting you upon having been selected to preside over an important Section of this the fifth Intercolonial Medical Congress, and of congratulating those other members of the Congress who are mostly interested in the branches of the surgery embraced therein upon the existence of such a Section. A Section which enables us to bring forward matters for the expression of opinions, discussion, or controversy which would otherwise load the Section of Surgery as a whole to our possible exclusion. The matter upon which I would call your attention is that method of estimating the refraction of the eye which is most commonly known as retinoscopy.

The recollection of having listened to a remark made during the sittings of a former Congress in our continent to the effect that "Retinoscopy is no good," and to the ofttime repeated question by students, "Does retinoscopy help much?" has suggested to me that a fair unbiased statement of personal experience in its use might be of interest and value to some of my fellow-workers.

Edward Jackson (Philadelphia) says, "It is not supposed that any opthalmologist is quite ignorant of the test, but many do not know its full practical value or how best to use it." These are my excuses for bringing before you some notes on the use of the shadow test and of announcing myself as an advocate for retinoscopy.

Retinoscopy may be defined as a method of estimating the refractive condition of an eye by means of observing the movement of the illumination obtained from the retina by rotating a mirror in its principal axes. It is well to recollect that it is the illumination obtained from the retina and the shadow created by the movement of the mirror which supplies the information obtained by this objective test.

It is not my purpose to describe the mode of examination, or to compare its value with that of any other method, but simply to endeavour to demonstrate its intrinsic worth by offering for your consideration some of the results of my experience of its use, and that in a very brief manner.

For this purpose I have taken a few recent cases from my note-books, patients of an age when they were able to read type, and when atropine could be safely used.

These include: One hundred cases of astigmatism measured under atropine, and 100 cases of astigmatism measured without atropine, arranged in accordance with the dioptric numbering found in an ordinary trial case, and then tabulating the results; 200 cases, as they occurred in the note-books, in which there was astigmatism, but free from any eye disease—making in all 400 eyes and 800 principal meridians I would like to state that, being mindful of human fallibility, and doubtful of my own power to resist the temptation of skipping an embarrassing case, I have had the measurements made out by another, of sufficient intelligence, yet devoid of any acquaintance with the routine of examination, so as to avoid any selection, or any leaning towards or against any particular case; and the gauge of each meridian may be, therefore, taken as that actually appearing in the case-books without any degree of influence emanating from myself—an unbiased record. A concave mirror was used throughout. The retinoscopic measurements were made, the formulæ expected after deducting the usual allowance for retinoscopy noted, the formulæ obtained by subjective examination entered, and any agreement or disagreement between these two latter shown; and, further, whether the expected result was greater or less than that obtained subjectively, and whether in the direction of the spherical or the cylindrical lens; also, whether with or against the rule. In considering the following tables, it will be well to recollect that the difference between the various measurements cannot be exactly estimated—the measurement must be placed either as approaching more closely to the number of the trial lens above that obtained, or below; thus what is put down as + 4·50 may be that exactly or + 4·25 or 4·75, or any other fraction between + 4 and + 5, or in the case of + 0·50 it may be any other fraction between + 0·26 and + 0·49.

Examples: + 0·75 deducted for retinoscopy.

Retinoscopy.	Expected.	Obtained.	Difference.
H V + 2·50 ... ... + 5·00	+ 1·75 + 2·50	+ 1·50 + 2·50	+ 0·25 less + 0·00
H V + 2·25 ... . + 5·00	+ 1·50 + 2·75	+ 1·75 + 2·75	+ 0·25 more + 0·00
H V + 2·50 ... . + 4·50	+ 1·75 + 2·00	+ 1·75 + 1·75	+ 0·00 + 0·25 less
H V + 2·25 ... ... + 5·00	+ 1·50 + 2·75	+ 1·50 + 3·00	+ 0·00 + 0·25 more
H V + 2·50 .. ... + 5·00	+ 1·75 + 2·50	+ 1·75 + 2·50	+ 0·00 + 0·00

The resulting combination of compound lenses, after allowing for the use of atropine, is not given, nor is the vision attained shown, as these have no bearing upon the working out of the retinoscopy.

TABLE I.

Shows agreement or difference between the formulæ obtained by retinoscopic measurement and the formulæ obtained subjectively.

A.—With atropine. 100 cases, 200 eyes, 400 meridians.

First, with allowance of + 0·50 for retinoscopy

Agreeing in *both* meridians	Right	16	Left	20
Agreeing in *one* meridian	,,	56	,,	52
Differing in *one* meridian	,,	56	,,	52
Differing in *both* meridians	,,	72	,,	76
Meridians	Right	200	Left	200=400

Second, with allowance of + 0·75 for retinoscopy.

Agreeing in *both* meridians	Right	42	Left	36
Agreeing in *one* meridian	,,	55	,,	49
Differing in *one* meridian	,,	55	,,	49
Differing in *both* meridians	,,	48	,,	66
Meridians	Right	200	Left	200=400

Third, with allowance of + 1·00 for retinoscopy.

Agreeing in *both* meridians	Right	28	Left	30
Agreeing in *one* meridian	,,	47	,,	50
Differing in *one* meridian	,,	47	,,	50
Differing in *both* meridians	,,	78	,,	70
Meridians	Right	200	Left	200=400

B.—Without atropine. 103 cases, 200 eyes, 400 meridians.

First, with allowance of + 0·50 for retinoscopy.

Agreeing in *both* meridians	Right	34	Left	34
Agreeing in *one* meridian	,,	57	,,	55
Differing in *one* meridian	,,	57	,,	55
Differing in *both* meridians	,,	52	,,	56
		200		200=400

Second, with allowance of + 0·75 for retinoscopy.

Agreeing in *both* meridians	Right	68	Left	58
Agreeing in *one* meridian	,,	52	,,	49
Differing in *one* meridian	,,	52	,,	49
Differing in *both* meridians	,,	28	,,	44
		200		00=400

Third, with allowance of + 1·00 for retinoscopy.

Agreeing in *both* meridians	Right	30	Left	24
Agreeing in *one* meridian	,,	64	,,	58
Differing in *one* meridian	,,	64	,,	58
Differing in *both* meridians	,,	42	,,	60
		200		200=400

Table II. shows that a closer approximation of measurement can be obtained either with or without atropine, when the allowance of + 0·75 is deducted for retinoscopy, as compared with a lesser allowance of + 0·50 or a greater allowance of + 1·00.

TABLE II.

Shows the amount of disagreement in dioptres.

With atropine—*More* than estimated.

Right Eye—Sphere.

Error.	Allowance, + 0·50.			Allowance, + 0·75.			Allowance, + 1·00.		
	Meridians.		Dioptres.	Meridians.		Dioptres.	Meridians.		Dioptres.
+ 0·25	5	=	1·25	10	=	2·50	17	=	4·25
+ 0·50	3	=	1·50	2	=	1·00	8	=	4·00
+ 0·75	0	=	0·00	2	=	1·50	1	=	0·75
+ 1·00	0	=	0·00	1	=	1·00	2	=	2·00
	8	=	2·75	15	=	6·00	28	=	11·00
– 0·25	18	=	4·50	6	=	1·50	5	=	1·25
– 0·50	6	=	3·00	1	=	0·50	0	=	0·00
– 0·75	1	=	0·75	0	=	0·00	0	=	0·00
– 1·00	1	=	1·00	0	=	0·00	0	=	0·00
	26	=	9·25	7	=	2·00	5	=	1·25

Shows the amount of disagreement in dioptres.

With atropine—*Less* than estimated.

Right Eye—Sphere.

Error.	Allowance, + 0·50.			Allowance, + 0·75.			Allowance, + 1·00.		
	Meridians.		Dioptres.	Meridians.		Dioptres.	Meridians.		Dioptres.
+ 0·25	15	=	3·75	15	=	3·75	9	=	2·25
+ 0·50	15	=	7·50	7	=	3·50	2	=	1·00
+ 0·75	7	=	5·25	1	=	0·75	0	=	0·00
+ 1·00	2	=	2·00	0	=	0·00	1	=	1·00
	39	=	18·50	23	=	8·00	12	=	4·25
– 0·25	6	=	1·50	10	=	2·50	22	=	5·50
– 0·50	3	=	1·50	2	=	1·00	9	=	4·50
– 0·75	0	=	0·00	3	=	2·25	2	=	1·50
– 1·00	0	=	0·00	0	=	0·00	2	=	2·00
	9	=	3·00	15	=	5·75	35	=	13·50

With atropine—*More* than estimated.

Right Eye—Cylinder.

Error.	Meridians.		Dioptres.	Error.	Meridians.		Dioptres.
+ 0·25 ...	12	=	3·00	– 0·25	8	=	2·00
+ 0·50	5	=	2·50	– 0·50	3	=	1·50
+ 0·75 ...	0	=	0·00	– 0·75	0	=	0·00
+ 1·00	0	=	0·00	– 1·00	0	=	0·00
	17	=	5·50		11	=	3·50

With atropine—*Less* than estimated.

Right Eye—Cylinder.

Error.	Meridians.		Dioptres.	Errors.	Meridians.		Dioptres.
+ 0·25 ..	9	=	2·25	– 0·25	5	=	1·25
+ 0·50 . .	4	=	2·00	– 0·50	0	=	0·00
+ 0·75	1	=	0·75	– 0·75	0	=	0·00
+ 1·00 ...	0	=	0·00	– 1·00	0	=	0·00
	14	=	5·00		5	=	1·25

Of the 100 right eyes (200 meridians) examined with atropine, we have :—

With 0·50 allowance, 82 meridians disagreeing, with a total error of 33·50 dioptres.
With 0·75 allowance, 60 meridians disagreeing, with a total error of 21·75 dioptres.
With 1·00 allowance, 80 meridians disagreeing, with a total error of 30·00 dioptres.

On the other hand—

With 0·50 allowance, we have 118 agreeing ;
With 0·75 allowance, we have 140 agreeing ;
With 1·00 allowance, we have 120 agreeing ;

Or—

	Meridians Disagreeing.		Meridians Agreeing.		
0·50 allowance	82	...	118	=	200
0·75 allowance	60	...	140	=	200
1·00 allowance	80	..	120	=	200

If we eliminate the meridians in which the error is only 0·25 D (a very insignificant error), we have—

	Meridians Disagreeing.		Meridians Agreeing.		
0·50 allowance ...	38 or 22·50 D	...	162	=	200
0·75 allowance ...	19 or 11·50 D	...	181	=	200
1·00 allowance ...	27 or 16·75 D	...	173	=	200

With atropine—*More* than estimated.

Left Eye—Sphere.

Error.	Allowance, + 0·50.			Allowance, + 0·75.			Allowance, + 1·00		
	Meridians.		Dioptres.	Meridians.		Dioptres.	Meridians.		Dioptres.
+ 0·25	2	=	0·50	13	=	3·25	13	=	3·25
− 0·50	2	=	1·00	2	=	1·00	11	=	5·50
+ 0·75	1	=	0·75	1	=	0·75	1	=	0·75
− 1·00	0	=	0·00	0	=	0 00	1	=	1·00
	5	=	2·25	16	=	5·00	26	=	10·50
− 0-25	15	=	3·75	7	=	1·75	8	=	2·00
− 0·50	8	=	4·00	0	=	0·00	3	=	1·50
− 0·75	2	=	1·50	2	=	1·50	0	=	0·00
− 1·00	2	=	2·00	0	=	0·00	1	=	1·00
	27	=	11·25	9	=	3·25	12	=	4·50

With atropine—*Less* than estimated.

Left Eye—Sphere.

Error.	Allowance, + 0·50.			Allowance, + 0·75.			Allowance, + 1·00		
	Meridians.		Dioptres.	Meridians.		Dioptres.	Meridians.		Dioptres
+ 0·25	19	=	4·75	16	=	4·00	9	=	2·25
+ 0·50	15	=	7·50	8	=	4·00	0	=	0·00
+ 0·75	6	=	4·50	0	=	0·00	1	=	0·75
+ 1·00	0	=	0·00	1	=	1·00	1	=	1·00
+ 1·25	1	=	1·25	1	=	1·25	0	=	0·00
+ 1·50	1	=	1·50	0	=	0·00	0	=	0·00
	42	=	19·50	26	=	10·25	11	=	4·00
− 0·25	6	=	1·50	10	=	2·50	14		3·50
− 0·50	1	=	0·50	4	=	2·00	7	−	3·50
− 0·75	0	=	0·00	0	=	0·00	2	=	1·50
− 1·00	0	=	0·00	0	=	0·00	0	=	0·00
	7	=	2·00	14	=	4·50	23	=	8.50

More than estimated.

Left Eye—Cylinder.

Error.	Meridians.		Dioptres.	Error.	Meridians.		Dioptres.
+ 0·25	13		3·25	− 0·25	5	=	1·25
+ 0·50	9	=	4·50	− 0·50	2	=	1·00
+ 0·75	1	=	0·75	− 0·75	0	=	0·00
+ 1·00	0	=	0·00	− 1·00	0	=	0·00
	23	=	8·50		7	=	2·25

Less than estimated.

Left Eye—Cylinder.

Error.	Meridians.		Dioptres.	Error.	Meridians.		Dioptres.
+ 0·25	7	=	1·75	− 0·25	1	=	0·25
+ 0·50	5	=	2·50	− 0·50	2	=	1·00
+ 0·75	1	=	0·75	− 0·75	0	=	0·00
+ 1·00	0	=	0·00	− 1·00	0	=	0·00
	13	=	5·00		3	=	1·25

Of the 100 left eyes (200 meridians) examined with atropine, we have—

With 0·50 allowance, 81 meridians disagreeing, with a total error of 35·00 dioptres.
With 0·75 allowance, 65 meridians disagreeing, with a total error of 23·00 dioptres.
With 1·00 allowance, 72 meridians disagreeing, with a total error of 27·00 dioptres.

On the other hand—

With 0·50 allowance, we have 119 agreeing meridians ;
With 0·75 allowance, we have 135 agreeing meridians ;
With 1·00 allowance, we have 128 agreeing meridians ;

Or—

	Meridians Disagreeing.	Meridians Agreeing.		
0·50 allowance	81	119	=	200
0·75 allowance	65	135	=	200
1·00 allowance	72	128	=	200

If we eliminate the meridians in which the error is only 0·25 (a very insignificant error), we have—

	Meridians Disagreeing.	Meridians Agreeing.		
0·50 allowance	39 or 24·50 D	161	=	200
0·75 allowance	19 or 11·50 D	181	=	200
1·00 allowance	28 or 16·50 D	172	=	200

Without atropine—*More* than estimated.

Right Eye—Sphere.

Error.	Allowance, + 0·50.			Allowance, + 0·75.			Allowance, + 1·00.		
	Meridians.		Dioptres.	Meridians.		Dioptres.	Meridians.		Dioptres.
+ 0·25	4	=	1·00	8	=	2·00	16	=	4·00
+ 0·50	2	=	1·00	0	=	0·00	3	=	1·50
+ 0·75	0	=	0·00	0	=	0·00	0	=	0·00
+ 1·00	0	=	0·00	0	=	0·00	0	=	0·00
	6	=	2·00	8	=	2·00	19	=	5·50
− 0·25	23	=	5·75	15	=	3·75	2	=	0·50
− 0·50	10	=	5·00	0	=	0·00	1	=	0·50
− 0·75	0	=	0·00	0	=	0·00	0	=	0·00
− 1·00	0	=	0·00	0	=	0·00	0	=	0·00
	33	=	10·75	15	=	3·75	3	=	1·00

Without atropine—*Less* than estimated.

Right Eye—Sphere.

Error.	Allowance, + 0·50.			Allowance, + 0·75.			Allowance, + 1·00.		
	Meridians.		Dioptres.	Meridians.		Dioptres.	Meridians.		Dioptres.
+ 0·25	23	=	5·75	10	=	2·50	6	=	1·50
+ 0·50	10	=	5·00	2	=	1·00	1	=	0·50
+ 0·75	1	=	0·75	0	=	0·00	0	=	0·00
+ 1·00	0	=	0·00	0	=	0·00	1	=	1·00
+ 1·25	0	=	0·00	0	=	0·00	0	=	0·00
+ 1·50	0	=	0·00	0	=	0·00	0	=	0·00
+ 1·75	1	=	1·75	0	=	0·00	0	=	0·00
	35	=	13·25	12	=	3·50	8	=	3·00
– 0·25 ...	1	=	0·25	16	=	4·00	30	=	7·50
– 0·50	1	=	0·50	1	=	0·50	15	=	7·50
– 0·75	0	=	0·00	0	=	0·00	0	=	0·00
– 1·00	0	=	0·00	0	=	0·00	0	=	0·00
	2	=	0·75	17	=	4·50	45	=	15·00

Without atropine—*More* than estimated.

Right Eye—Cylinder.

Error.	Meridians.		Dioptres.	Error.	Meridians.		Dioptres.
+ 0·25	12	=	3·00	– 0·25	2	=	0·50
+ 0·50	2	=	1·00	– 0·50	4	=	2·00
+ 0·75	0	=	0·00	– 0·75	0	=	0·00
+ 1·00	0	=	0·00	– 1·00	0	=	0·00
	14	=	4·00		6	=	2·50

Without atropine—*Less* than estimated.

Right Eye—Cylinder.

Error.	Meridians.		Dioptres.	Error.	Meridians.		Dioptres.
+ 0·25	7	=	1·75	– 0·25	2	=	0·50
+ 0·50	1	=	0·50	– 0·50	0	=	0·00
+ 0·75	0	=	0·00	– 0·75	0	=	0·00
+ 1·00	0	=	0·00	– 1·00	0	=	0·00
	8	=	2·25		2	=	0·50

Of the 100 right eyes (200 meridians) examined without atropine, we have—

With 0·50 allowance, 76 meridians disagreeing, with a total error of 26·75 dioptres.
With 0·75 allowance, 52 meridians disagreeing, with a total error of 13·75 dioptres.
With 1·00 allowance, 75 meridians disagreeing, with a total error of 24·50 dioptres.

On the other hand—

With 0·50 allowance, we have 124 agreeing meridians;
With 0·75 allowance, we have 148 agreeing meridians;
With 1·00 allowance, we have 125 agreeing meridians;

Or—

	Meridians Disagreeing.	Meridians Agreeing.
0·50 allowance	76 ...	124 = 200
0·75 allowance	52 ...	148 = 200
1·00 allowance	75 ...	125 = 200

If we eliminate the meridians in which the error is only 0·25 (a very insignificant error), we have—

	Meridians Disagreeing.	Meridians Agreeing.
0·50 allowance ...	25 or 14·00 D ..	175 = 200
0·75 allowance ...	3 or 1·50 D ...	197 = 200
1·00 allowance ...	21 or 11·00 D ...	179 = 200

Without atropine—*More* than estimated.

Left Eye—Sphere.

Error.	Allowance, + 0·50.			Allowance, + 0·75.			Allowance, + 1·00.		
	Meridians.		Dioptres.	Meridians.		Dioptres.	Meridians.		Dioptres.
+ 0·25	6	=	1·50	9	=	2·25	15	=	3·75
+ 0·50	0	=	0·00	3	=	1·50	4	=	2·00
+ 0·75	0	=	0·00	0	=	0·00	2	=	1·50
+ 1·00	0	=	0·00	0	=	0·00	0	=	0·00
	6	=	1·50	12	=	3·75	21	=	7·25
− 0·25	20	=	5·00	10	=	2·50	3	=	0·75
− 0·50	8	=	4·00	1	=	0·50	3	=	1·50
− 0·75	1	=	0·75	0	=	0·00	1	=	0·75
− 1·00	0	=	0·00	0	=	0·00	0	=	0·00
	29	=	9·75	11	=	3·00	7	=	3·00

Without atropine—*Less* than estimated.

Left Eye—Sphere.

Error.	Allowance, + 0·50.			Allowance, + 0·75.			Allowance, + 1·00.		
	Meridians.		Dioptres.	Meridians.		Dioptres.	Meridians.		Dioptres.
+ 0·25	22	=	5·50	10	=	2·50	4	=	1·00
+ 0·50	7	=	3·50	0	=	0·00	2	=	1·00
+ 0·75	0	=	0·00	1	=	0·75	0	=	0·00
+ 1·00	1	=	1·00	0	=	0·00	0	=	0·00
	30	=	10·00	11	=	3·25	6	=	2·00
− 0·25	3	=	0·75	14	=	3·50	28	=	7·00
− 0·50	3	=	1·50	1	=	0·50	14	=	7·00
− 0·75	2	=	1·50	0	=	0·00	1	=	0·75
− 1·00	0	=	0·00	2	=	2·00	1	=	1·00
	8	=	3·75	17	=	6·00	44	=	15·75

Without atropine—*More* than estimated.

Left Eye—Cylinder.

Error.	Meridians.		Dioptres.	Error.	Meridians.		Dioptres.
+ 0·25	13	=	3·25	− 0·25	8	=	2·00
+ 0·50	1	=	0·50	− 0·50	1	=	0·50
+ 0·75	1	=	0·75	− 0·75	0	=	0·00
+ 1·00	0	=	0·00	− 1·00	0	=	0·00
	15	=	4·50		9	=	2·50

Without atropine—*Less* than estimated.

Left Eye—Cylinder.

Error.	Meridians.		Dioptres.	Error.	Meridians.		Dioptres.
+ 0·25	6	=	1·50	− 0·25	6	=	1·50
+ 0·50	2	=	1·00	− 0·50	2	=	1·00
+ 0·75	1	=	0·75	− 0·75	0	=	0·00
+ 1·00	0	=	0·00	− 1·00	0	=	0·00
	9	=	3·25		8	=	2·50

Of the 100 left eyes (200 meridians) examined without atropine, we have—

With 0·50 allowance, 73 meridians disagreeing, with a total error of 25·00 dioptres.
With 0·75 allowance, 51 meridians disagreeing, with a total error of 15·00 dioptres.
With 1·00 allowance, 78 meridians disagreeing, with a total error of 28·00 dioptres.

On the other hand—

With 0·50 allowance, we have 127 agreeing meridians;
With 0·75 allowance, we have 149 agreeing meridians;
With 1·00 allowance, we have 122 agreeing meridians;

Or—

	Meridians Disagreeing.	Meridians Agreeing.
0·50 allowance	73	127 = 200
0·75 allowance	51	149 = 200
1·00 allowance	78	122 = 200

If we eliminate the meridians in which the error is only 0·25 (a very insignificant error), we have—

	Meridians Disagreeing.	Meridians Agreeing.
0·50 allowance	22 or 12·25 D	178 = 200
0·75 allowance	8 or 4·25 D	192 = 200
1·00 allowance	38 or 15·50 D	162 = 200

Table II. shows that a closer approximation is attainable by an allowance of + 0·75 than 0·50 or 1·00, and also for 1·00 in comparison with 0·50.

TABLE III.

A—Shows relation of cylindrical lenses towards the vertical or horizontal meridian.

With atropine.

Vertical, or with the rule—

Right eye, 68; left eye, 67 total, 135

Horizontal, or against the rule—

Right eye, 32; left eye, 33; total, 65

200 meridians.

Right Eye.	Hypermetropic Eyes.	Myopic Eyes.
Vertical	52	16 = 68
Horizontal	17	15 = 32
		100 Eyes.

Left Eye.	Hypermetropic Eyes.	Myopic Eyes.
Vertical	64	3 = 67
Horizontal	17	16 = 33
		100 Eyes.

B—Shows relation of cylindrical lenses towards the vertical or horizontal meridian.

Without atropine.

Vertical, or with the rule—

Right eye, 49; left eye, 50; total, 99

Horizontal, or against the rule—

Right eye, 51; left eye, 50; total, 101

200

Right Eye.	Hypermetropic Eyes.	Myopic Eyes.
Vertical	37	12 = 49
Horizontal	31	20 = 51
		100

Left Eye.	Hypermetropic Eyes.	Myopic Eyes.
Vertical	32	18 = 50
Horizontal	34	16 = 50
		100

Total (with and without atropine)—

With the rule	234 meridians
Against the rule	166 meridians.

The greater advantage of the retinoscopic test lies in the estimation of the cylinders (the astigmatism), as is well shown by Table II., where we see that, of the 800 meridians examined, only 164 disagree, having a total error of 58·25 D as compared with 636 in which there is agreement, or, if we eliminate those having an error of only 0·25, we find only 48 meridians disagreeing with a total error of 25·25 D as compared with 752 in which there is agreement.

My contention is that retinoscopy, in the hands of those who carefully employ it, gives very reliable and proximate data for acquiring an advantageous prehension of the refraction of an eye. I claim that the foregoing tables strongly support, if not thoroughly substantiate, this contention, and feel convinced that whoever employs this method will find in it a very valuable adjunct and time-saver in the daily routine work.

It is difficult to imagine in what way one could better cope with the optical correction of kerato-conus than by the aid of retinoscopy; in such cases there is no more rapid or satisfactory objective test. This is well illustrated by the following case:—

Conical Cornea. Male, Aged, 34.

Retinoscopy.		Expected.	Obtained.	Difference.
R	− 5·00	+ 3·25	+ 3·50	+ 0·25 more
	+ 4·00	− 9·00	− 10·00	− 1·00 more
L	− 4·	+ 5·25	+ 5·00	+ 0·25 less.
	+ 6·	− 10·00	− 10·00	− 0·00

The final result is:

R + 3·50 Sphere = − 10·00 cyl. at 115° = 6/6 partly
L + 5·00 Sphere = − 10·00 cyl. at 125° = 6/9.

In measuring eyes by retinoscopy I have long been in the habit of using an arrangement of frame to place upon the patient which has let into it the lens equivalent to the allowance for retinoscopy. [Instrument shown.]

It has the advantage of giving the same numbers in the case-book as found in the dark room; doing away with calculations and thus simplifying the process. It has also the advantage of obviating the troublesome condensation of moisture from the warm moist eye upon the cold trial glasses—the steaming of the trial lenses being rendered impossible by the intervention between them and the eye of the once warmed lens of the "Simplifier."

DISCUSSION ON RETINOSCOPY.

Dr. **Ferguson** considered full measurement of refraction by retinoscopy was measurement of the eye. Each patient has to be dealt with rather from the point of view of personal equation than from the hard-and-fast measurements. He calculates to leave one-third of total accommodation in reserve.

Dr. **Kirkland** practises retinoscopy and uses atropine for children and homatropine for adults.

Dr. **J. P. Ryan** practises retinoscopy under atropine, but instils ezerine immediately after estimating the refraction of elderly people on account of the risk of atropine inducing glaucoma.

Dr. **Orr** uses retinoscopy, especially for children, and thinks it is the best method.

Dr. **Pockley** employs retinoscopy under homatropine and cocain; he rarely uses atropine. He leaves some accommodation in reserve, corrects the whole of the manifest and half the latent hypermetropia to begin with, because they will not wear their spectacles if all the latent hypermetropia is corrected.

Dr. Kent Hughes: I agree with Dr. Symons in failing to get a result by retinoscopy that agrees with the subjective test. The latter I look upon as the perfect measurement of the eye—that is to say, the glass that the patient takes, not what we think he ought to take. I always use a mydriatic and never trust to homatropine in patients under thirty if there is any difficulty about the case. When it is only possible, as in young children, to employ an objective test I always perform retinoscopy three times at least. It is very difficult in young children to get a perfect measurement, as the least difference in the angle will cause disappointment. I persevere till I get three results agreeing; it means often a lot of examinations, but it is quite necessary. I always order hypermetropic patients the highest glass they will accept under complete mydriasis. I never allow for the tone of the ciliary muscle. It has either got no tone, or atropine does not banish it. Ten years ago nearly I was taught by Mr. Archer to give full correction by this method. I always did so for his patients at the Central Ophthalmic, and his splendid results in the treatment of hypermetropia in ordinary cases, and in strabismus, soon convinced me of the utility of the method. Dr. Barrett, at the last Congress, also testified to its value in his paper on strabismus. Starting his inquiry with great doubt as to its utility, he concluded by observing that I had underestimated its importance. I have never yet known a case of hypermetropia that refused full correction. In some cases it is necessary when atropine has not been used, or its effects have worn off before the glasses were obtained, to instil homatropine and cocain when the glasses are first put on.

Dr. Lockhart Gibson did not employ retinoscopy, but relied upon the direct method of examination, aided in adults and in children of above seven years by test-types. He did not use a mydriatic, except very rarely; he found that he was able in a great majority of cases to give very full correction by these means, and not infrequently fuller correction than had been ascertained by others with retinoscopy. He was convinced of the necessity for full correction in hypermetropic cases suffering from asthenopia, and thought this was seldom obtained if the patient had good distant vision with the spectacles ordered until after some weeks of constant wear. He impressed this fact upon his patients, and only occasionally found them too impatient or too devoid of trust and intelligence to persevere. He considered also that by combining the direct method with test-types the danger of over-correcting myopia was practically non-existent. Apart from the ordinary objections to a mydriatic, which from the patient's point of view were considerable, he had seen reason to believe even apparently complete mydriasis not devoid of fallacy. He quoted a case of hypermetropic astigmatism, who on account of an attack of rheumatic iritis had to be brought and kept under complete duboisia and atropia mydriasis for some weeks, and who, after the attack had passed off, but before the mydriatic had been relaxed to any extent (while, in fact, a drop of a 4 grains to the ounce solution of atropine was being instilled three times daily), had his vision, which had been previously $\frac{6}{5}$ with a + 1 D cylinder, reduced with this cylinder to less than $\frac{6}{12}$, and only brought up to $\frac{6}{5}$ when a — 1 D cylinder was transposed for it, with axis, of course, at right angles. That the apparent myopia was only temporary and functional was proved by the fact that after the mydriatic had been abandoned vision became again $\frac{6}{5}$ with his + 1 cylinder. He did not suppose that any method was without its fallacies; but he had seen or heard of nothing regarding retinoscopy which inclined him to adopt it to the exclusion of direct estimation of refraction. It was quite possible that his difficulties were increased in a few cases by his disregarding the method; but he was firmly convinced that to get full value out of direct estimation it must be constantly practised and relied upon, and that a greater interval than ·5 D must not exist between the lenses in the ophthalmoscope, and that they must be single. He considered that his results in refraction cases were as a rule good.

A CASE OF RETINITIS CIRCINATA.

By James P. Ryan, L.R.C.S.I., M.K.Q.C.P.I., Chevalier of the Legion of Honour;
Hon. Surgeon Victorian Eye and Ear Hospital.

Mrs. N.K., æt. seventy-eight, of stout build and healthy looking, was admitted into the Victorian Eye and Ear Hospital on the 22nd June, 1899—V.R., $\frac{2}{60}$; V.L., $\frac{4}{60}$.

History.—Up to five years ago her sight was perfectly good for distant objects, and with the aid of glasses she was able to read the smallest print. She then for the first time began to notice some failure of vision, which has gradually but steadily become worse.

Present Condition.—There is nothing unusual in the appearance of either eye, and the corneæ of both are clear. There is incipient cataract in both eyes, and the vitreous of each contains some fine opacities, interfering to some extent with a clear view of the fundus.

In the right fundus, arranged in a roughly circular way around the macula, are several somewhat recent hæmorrhages. The optic disc and remainder of the field appear to be normal. A rather better view is obtainable of the left fundus, and one is immediately struck by the brilliant appearance of a glittering white band or wreath surrounding the macula. It is almost a complete circle with only a slight deficiency at the lower and outer part, its diameter being from three to four times that of the disc, and its width varying from half to the diameter of the disc, which it just touches at the inner margin. It has a small outgrowth or projection from the upper and inner and another from the lower and outer segments, and the temporal vessels cross over it above and below. The macula is at most slightly mottled, and the retina between it and the wreath is normal in appearance. There are no hæmorrhages and no deposits of pigment.

There is no history, nor are there any signs of former syphilis; the pulse is one of somewhat high tension; the sp. gr. of the urine is 1·010, and it contains neither sugar nor albumen.

Retinitis circinata was first described by Fuchs in 1893. That it is a rare affection may be gathered from the fact that he met with it only twelve times in 70,000 cases, and De Wecker fifteen times in 150,000. Ten of Fuchs's cases were women, and only two were men. Both eyes were implicated in five, and one eye only in seven of the cases, and the average age of the patients was sixty.

The clinical appearances, which are remarkable, consist of certain alterations in the macula, from a greyish opacity in mild cases to dense deposits of pigment and hæmorrhages in severe and more advanced ones. Then surrounding the macular region is a circular or elliptical zone of white spots or plaques, set more or less closely together, and in some cases forming a regular white band or wreath. For the most part the zone is broken or deficient in more than one place, and its width varies in the different segments. Not unfrequently the retina included between the affected macular region and the white zone is, or appears to be, normal. The ring is usually within the limits of the superior and inferior temporal vessels, and its diameter is about three times that of the disc. The latter may be slightly blurred and the vessels tortuous, though more generally both appear to be entirely unaltered. In the early stage of the disease the vitreous is clear, but later on opacities develop in it, and frequently there is a central scotoma. Light and colour sense are not much altered, but the vision slowly but surely deteriorates, though the patient seldom becomes totally blind. Fuchs holds that the disease is a new and distinct one, and assumes that the white spots or patches are exudates of fibrin into the deeper layers of the retina. In his cases the disease was not associated with syphilis, diabetes, or albuminuria, though the vessels were generally rigid. De Wecker, on the other hand, declares it is only a variety of hæmorrhagic retinitis, with a supervening fatty degeneration. According to him, the affection begins with degeneration of the circum-macular vessels, followed by hæmorrhages, and succeeded later on by fatty degeneration. Other and later observers appear to corroborate this view.

Last year Dr. Amman, of Zurich, was enabled to examine pathologically an eye affected with retinitis circinata. He found numerous blood clots in the intergranular layer of the retina; transitions between extravasations of blood and hyaline plates; and, finally, white spots and patches, which were due to clusters of fat cells. Hence the stages of the evolution of the disease would appear to be first degeneration of the circum-macular vessels, extravasations of blood, deposits of pigment and fibrin, and lastly fatty degeneration.

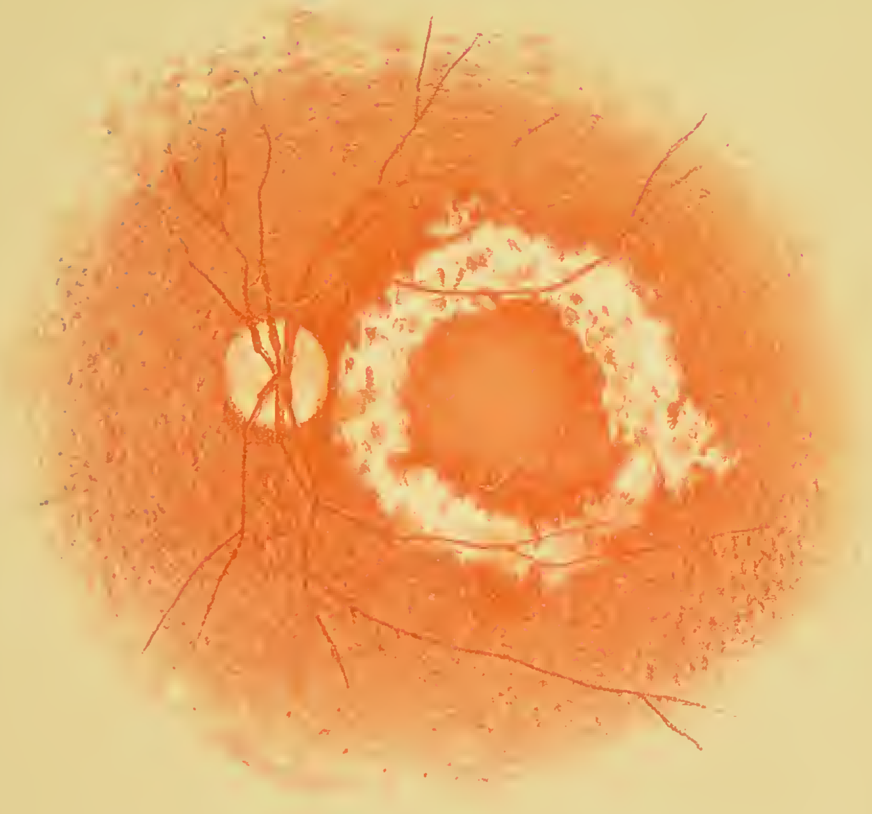
The disease, whether we regard it as a special or distinct one or merely as a variety of hæmorrhagic retinitis, is so remarkable in its appearance as to make it difficult to account for the fact that only within the last half-dozen years has it been noticed and described. So far as I know, plates of it are not found in the acknowledged ophthalmological atlases, and the only ones I have had an opportunity of examining are a few scattered over some volumes of the Transactions of the Ophthalmological Society of the United Kingdom. The first is a case published by Mr. Holmes Spicer in 1894. The subject was a woman of seventy-three. The right eye contained hæmorrhages in the neighbourhood of the upper temporal vein. In the left was an elliptical zone of white spots (the ring being somewhat incomplete) surrounding the macula, which was grey and opaque and slightly pigmented. A case (with a plate) was recorded by Mr. Lawford in 1895 of a woman of seventy-seven, who had hæmorrhage and degenerative pigmentary changes at the macula of the right eye. That of the left was also pigmented, and, in addition, was surrounded by a band of whitish exudation, varying in width and broken in places. The white zone was bordered slightly here and there with pigment. In the same volume Mr. Laws describes the case (without a plate) of a man of fifty-six, in which the changes "were symmetrical in both eyes. The maculæ were pigmented, and each was encircled by a deposit of closely set, small opaque spots, partly discrete, partly confluent." A crescent of white spots, somewhat pigmented, occurred in the nasal side of the disc. He had fair vision in both eyes.

Mr. Hartridge's case (in the same volume) was that of a woman of sixty-one whose sight had been failing for seven years—V. R. $\frac{1}{60}$, V. L. $\frac{6}{18}$. There were some pigmentary changes in the macula of the left eye, but no hæmorrhages and no white patches. Around the macular region of the right eye were a number of white radiating lines, arranged in a circle, which, however, was incomplete in two or three places.

There was pigmentation about the edges of some of the patches, but no hæmorrhages. The whole region embraced by the circle was the seat of a choroido-retinitis. The disc and retinal vessels were normal, and the latter passed clearly over the white deposit. Hartridge believed the white appearance to be due to degenerated nerve fibres which had lost their transparency, or possibly to exudation between the fibres. In the 18th volume of the Transactions Mr. Doyne gives a short description, with a drawing, of a case of the disease that occurred in his practice; but the appearances are not quite typical, and the age of the patient and the amount of vision present are somewhat against the case being one of retinitis circinata.

In the same volume Mr. E. C. Fisher publishes notes of a case (a woman of sixty-six), and gives a pretty drawing of the fundus of the affected eye. The zone or wreath of white deposit forms a nearly complete circle, but narrowing almost to a curved line of spots in its lower segment. Within its circumference were some pigmentary deposits, and just below it were signs of a recent hæmorrhage. The right eye was healthy.

RETINITIS CIRCINATA



View of Fundus of Left Eye.

In none of the above-mentioned examples of the disease is the glistening white sheen of the affected zone and the perfectly circular form of its arrangement so beautifully seen as in the case the notes of which I have just read, and a very accurate drawing of which is now before you.

Restoration of vision does not occur, and I do not know that any form of treatment has been followed by successful results.

I am indebted to Dr. Harold Lister, our house surgeon, for part of the above notes, and to Dr. Constance Stone for the beautiful drawing of the fundus which accompanies this paper.

PRONE POSITION IN OPERATING FOR POST-NASAL GROWTHS.

By W. Kent Hughes, M.B. Lond.

Operating for post-nasals is now such a universal practice that perhaps I will be forgiven for dwelling upon what I consider a matter of some improvement in the method of operating.

The position for the patient varies from the sitting position to the supine with head hanging down. All of them have, to my mind, disadvantages. We can dismiss the sitting position as being inapplicable to cases operated upon with anæsthesia, and I would say, in passing, that cases operated upon without anæsthesia are not to be considered as seriously operated upon at all.

The supine position, with head on a level with the body, is perhaps the most convenient position for the operator when forceps are used, but there is certainly no inconsiderable risk to the patient from the inspiration of clots; and I believe that most of those who adopt this method, only employ partial anæsthesia, recognising this danger—it is certainly the most "blood-scattering" method that I have ever seen. A large quantity of blood is certain to be swallowed. In the supine position, with head hanging down below the level of the body, from Dr. Lockhart Gibson's position to that where the head is almost directly downwards at a right angle with the body, there are several disadvantages. A minor point is the increased congestion of vessels, which is, of course, the more pronounced the lower the head is placed, producing increased hæmorrhage. Where there are large quantities of post-nasals, this becomes rather an important question, as you must have all seen cases in which children were decidedly pulled down from loss of blood.

The blood collects in great quantities in the nasopharynx, and we have large clots produced mixed with more or less remnants of post-nasal growths. These clots are liable to be inspired into the larynx; they are certainly swallowed in very great numbers, and also collect in the upper nasal chambers, probably to putrefy—those of you who are not specialists would be surprised at the capacity of the nasal chambers.

Some deny that there is much danger of inspiring clots, but no one can deny that it is a possibility always present, and it is an occurrence that I myself believe takes place more often than we give it credit for. Some operators fear it so greatly that they will only operate with partial anæsthesia —a proceeding most uncomfortable to everyone concerned; and I do not hesitate to say that often cases operated upon in this manner are not scraped as thoroughly as they ought to be. I remember operating upon a patient who had a permanent tracheotomy wound. There were not many post-nasal growths, and there was not much hæmorrhage. I adopted the supine position with head hanging down, and I was surprised to see quite a quantity of blood issue by the tracheotomy wound. Of course, there was in this case a more or less impaired larynx to deal with, which might account for the escape of the blood, but still it was to my mind a very instructive case; there was no lividity present, and no derangement of respiration.

Again, in the supine position with head hanging down, blood is almost certain to enter the Eustachian tubes.

To obviate all these disadvantages, I have adopted the prone position. The patient is chloroformed in the usual manner, and when ready for operation the gag is inserted, and the patient is turned over. The chloroformist holds the gag with one hand, and keeps the head at a convenient height with the other—that is, just below the level of the body; he can thus easily watch his patient's condition. The upper part of the chest should be well over the end of the couch.

In this position it will be found that post-nasal growths and blood will pour forth out of the mouth and nostrils; and even here, where there is a direct run, so to speak, for the blood, clots will form which seem loath to leave the nasal passages. The patient is held in this position for a little while after the operation is over, and then pulled up on to the couch, lying slightly on one side of the face, and the remainder of the blood is allowed to drain off.

The advantages of this position are that no blood can gather in the upper nasal chambers, little or no blood is swallowed, and the danger of inspiring blood is reduced to a minimum, and no blood can enter the Eustachian tubes. A minor point is the greater cleanliness, and if a pretty wide bucket be conveniently placed all the blood will enter it; there is thus a saving of aprons and in many instances of the clothes and persons of those in the immediate neighbourhood. The hair of the patient cannot get saturated with blood, and the anxious parents are not alarmed by the copious vomiting of blood that almost inevitably occurs in the supine method.

There is an apparent disadvantage to the operator in the awkward position of the patient; but one can quickly adapt oneself to the altered condition, and it is nothing like so awkward as the first post-nasal that one ever operated upon. The operation is more easily performed with the patient upon a couch somewhat higher than the ordinary. Some chloroformists object to the position, and, of course, one must always give in to their wishes, but there is, to my mind, no increase of danger to the patient from the position. Chloroform is administered in the usual position; the patient has taken it well and is completely under the anæsthetic; the dangerous period is over for short operations, the patient is turned over, the tongue and palate fall forward, and where respiration is of a pronounced post-nasal character the patient in the prone position will breathe more freely. This I have noted in scores of cases; the restless respiration is replaced by full and quiet breathing.

DISCUSSION ON PRONE POSITION IN OPERATING FOR POST-NASAL GROWTHS.

Dr. Brady (Sydney) said, regarding anæsthesia, although in young children he always operated under chloroform, in patients over the age of twelve years he frequently removed adenoids under cocain anæsthesia. In the prone position recommended by Dr. Kent Hughes, the impossibility of getting a good view of the operation field appeared to be an objection, particularly so in cases where it is necessary to remove tonsils as well as adenoids. He would like to know the opinions of members on the question of the advisability of removing tonsils and adenoids under the same anæsthesia. Personally, he always removed at least one tonsil without an anæsthetic some time before the operation for adenoids. The remaining tonsil and the adenoids can then be removed more safely under chloroform. He considered that when a double tonsillotomy and the removal of adenoids is performed under the same anæsthesia the danger of the anæsthetic is enhanced, and the risk of hæmorrhage much greater. Regarding Dr. Kent Hughes's objection to the hanging-head position—that it caused congestion of the head, and greatly increased the hæmorrhage—he had not experienced any trouble from this cause. The patient is anæsthetised in the usual position lying on the table. When quite ready, the head is drawn over the end of the table, and the adenoids rapidly removed, generally in one piece, with the Beckmann-Gottstein curette; using this instrument, which should be kept well sharpened, the operation can be so rapidly performed that there is no time for the congestion of the head spoken of to take place. As soon as the adenoids are removed, the patient is rapidly turned over on the table, face downwards, with the head hanging over the end of the table. The blood is allowed to flow from the nose and mouth. For this he considered Dr. Kent Hughes's position useful; only he used it after, not during, the operation.

Dr. Lockhart Gibson's (Brisbane) objection to operating upon a patient in the position suggested by Dr. Kent Hughes was that he did not think that the breathing under an anæsthetic ought to be impeded by practically giving the muscles of inspiration the whole weight of the trunk to lift. Furthermore, he thought that there would be distinct risk of blood getting into the larynx in the prone position during complete anæsthesia. In the position suggested and adopted by him (Dr. Gibson), which is a modification of the hanging-head position, the head is hardly lower than the trunk, but it is fully extended, so much so that the position cannot as a rule be perfectly obtained except under complete anæsthesia, which he always employs, and thinks much safer than partial. The vertex rests on a movable shelf below the edge of the table, the vertical line of the face is at right angles to the trunk, while the chin is on a level with the anterior chest wall. The position might be best called "the supported rectangular head position." The supporting of the head prevents any excessive loss of blood, and he is satisfied that in no position is less blood lost. In no case has he experienced excessive or undesirable loss of blood since abandoning the hanging-head position. It is impossible for blood to get into the larynx in this position, because while completely anæsthetised the larynx is at a higher level than the mouth and nose, the mouth being kept open by a Whitehead's gag. When imperfectly anæsthetised and struggling, the laryngeal reflex prevents blood entering the trachea. Blood can be swallowed when anæsthesia is not complete, because the constrictors get hold of it and worm it down. Next best to this position is the hanging-head position, which he had previously adopted. In it more blood was lost because the weight of the head caused the neck muscles to obstruct the venous return. With his position there was no need for any unnecessary hurry over the operation. The operation was a very short one, and with a comfortable position there was not the least need for hurry. It could be completed quietly, and the necessary toilette also. He considered the position a perfect one for cleft-palate operations, and for the prolonged administration of chloroform. Clots or *débris* did not remain in the nasal passages; they were forced out during the operation and prior to the gentle subsequent syringing of the nose. [An endeavour was made to demonstrate the position at the meeting upon himself and another member, but the impossibility of voluntary complete relaxation of the neck muscles made it impossible to place the head in position, and gave the impression that the position is a strained one.] He cuts tonsils beforehand, and without an anæsthetic. Only when specially indicated does he cut tonsils and remove adenoids at the same operation. Still, cutting the tonsils hardly adds to the length of the operation, and is unattended with more troublesome bleeding than when done without an anæsthetic and in the sitting posture. He deprecated the expression "removal" of tonsils as misleading to patients, and inaccurate. In adults and children above thirteen or fourteen years he removes adenoids under cocaine with Löwenberg's forceps (different modifications); adopts the sitting posture, and guides the forceps by a laryngeal mirror. Secondary hæmorrhage, in his experience, never occurs if the growths are thoroughly removed. He would like to cordially agree with Dr. Kent Hughes regarding complete anæsthesia, the necessity for thorough removal of the growths, and the absence of necessity for doing the operation in record time.

Dr. Alex. Francis (Brisbane) thoroughly approved of the position advocated by Dr. Kent Hughes as far as controlling the hæmorrhage was concerned; his objection to it would be the awkward and cramped position in which the operator must be placed. He was always in the habit of placing the patient in Dr. Kent Hughes' position immediately after the operation, which he found prevented any further trouble from the blood. During the operation he liked the head to rest downwards on a gently inclined plane. This lessened the risk of blood entering the larynx, and yet put no strain upon the neck, which he believed was the cause of trouble with the anæsthetic sometimes met with in the "hanging-head" position. In older children he was in favour of removing adenoids with a local anæsthetic. The application of supra-renal capsule extract a few minutes previously rendered the operation almost bloodless, which was of great assistance in preventing the blood from obscuring the vision. He considered it a great mistake, except under special circumstances, to remove tonsils at the same time as adenoids. The tonsils should be operated on first without any general anæsthetic.

Dr. Kirkland (Sydney) employed the hanging-head position, but spoke of the Trendelenburg position as one which might answer.

Dr. A. L. Kenny (Melbourne) found prone position objectionable on account of (1) awkwardness of position for operator, particularly if wanting to use both hands; (2) open to same objection from chloroform administration point of view, as in lumbar operations; (3) inability to watch face and see pharynx. He preferred head hanging down and supported by inclined plane in a not fully extended position, in order that the larynx may not be antero-posteriorly compressed. Trendelenburg position is unsuitable for post-nasal operations, the pressure of the movable abdominal viscera on the diaphragm being objectionable. Blood freely enters lowermost Eustachian tube in lateral position. He advocated complete chloroform anæsthesia and thorough removal of all adenoid growth.

Dr. Ferguson (Dunedin) operated entirely with forceps, removed bodily large pieces, and then used the blade as a curette, with a finger in the post-nasal space. Originally operated with patient lying flat. In 1885 began to throw head back over the pillow. Chloroform deeply, and should not require fresh chloroform. If quick, less hæmorrhage. Thought Kent Hughes' position would be difficult to operate in. Removed tonsils beforehand and without an anæsthetic.

Dr. W. K. Hughes, in reply: I was prepared to meet opposition on account of the awkwardness of the prone position for the operator and the supposed increased danger from chloroform. To those who use a curette there is really no difficulty whatever; it only requires a little practice. As regards the use of forceps, there may be some difficulty, but as I so very seldom use them I cannot speak on this point. I would point out that both hands are free; the left forefinger is passed over the left side of the patient's head into the mouth while the curette is held in the right. I am convinced there is no increased danger from chloroform—the patient has been chloroformed in the usual position, the danger from chloroform for a short operation is over, and the patient's chest muscles are not impeded when in the prone position. Practically patients with bad post-nasals, as I mentioned in my paper, breathe much more quietly and more deeply than in the usually adopted position, the tongue and soft palate both falling well forward. The quality of the respiration and the condition of the patient's face are both easily observed by chloroformist and operator with very little trouble to the former. The Trendelenburg position has all the faults of the ordinary position; and the lateral position has, I believe, been abandoned by its advocates—it renders entrance of blood into one Eustachian tube a certainty. Someone has stated that the Eustachian tubes are practically closed and would not admit blood. In nearly every case a large amount of deglutition is in force, and the tubes must be well opened each time the act of deglutition is performed. The anæsthetic I employ has invariably been chloroform. I like to take plenty of time over the operation and satisfy myself that the growths have been thoroughly removed. A large amount of discredit is being thrown upon the operation by the feverish anxiety of some to perform it in record time; and it behoves us as specialists to do our work so well that the public mind will not be filled with so much distrust as it is at present. As regards instruments, each has his own fancy. I use a curette and my finger-nail, and, like Dr. Brady, prefer Jansen's modification of Gottstein's to any other. Sponges I never used—not even before adopting the prone position. As regards the membranous bands mentioned by Dr. Kenny, I have never found them below the Eustachian tube, and should question the possibility of true post-nasal tissue extending that distance. I think the suggestion of the use of supra-renal capsule by Dr. Francis most valuable. Many of us must have seen cases in which loss of blood has seriously interfered with the patient's health for the time being. As regards the removal of tonsils, I never remove them in adults or children over twelve years of age at the same time as post-nasals; and in younger children I only remove one at the most. I don't think it is right to expose such a large raw surface to the danger of infection—a surface which it is impossible to render aseptic or keep aseptic. Again, even in children it is not wise to remove both tonsils at the same sitting; if serious hemorrhage should ensue, the situation is then very grave. When I do remove one tonsil, I remove the post-nasals first.

MACEWEN'S BURRS.

By A. J. Brady, L.R.C.S.I.,
Sydney.

Dr. Brady showed Macewen's round-headed mastoid burrs. His experience coincided with that of Macewen—viz., that they opened the mastoid rapidly and with great smartness, and all concussion of the head, such as occurs in the gouge and mallet method, was avoided. As showing the delicacy with which the burrs removed the bone, he instanced a case recently operated on: When opening a mastoid abscess a smooth glistening membrane was exposed at the bottom of the wound; it appeared that the sigmoid sinus might have been laid bare, but on using a probe it was found to be the fine sac wall of an abscess off which the burr had stripped the bone without tearing it. In his earlier operations he had used a dental engine to drive the burrs, but he found this lacking in power, and the driving cable was liable to buckle up and make the burr jump. In the engines with wheel and pulley driving shaft this is avoided. The surgical engine, of which plates were exhibited, made by the S. S. White Dental Company is a perfect one for the purpose of driving these large burrs. The burrs driven by the surgical engine are admirable for opening the frontal sinus and the antrum of Highmore. After-dressings, on account of the smoothness of the openings, are much less painful than those where the openings are made by other methods.

Dr. Brady considered Dr. Kenny's instrument for opening the frontal sinus appeared to have this disadvantage, that as the size of the sinus could not be determined beforehand it was possible in using the instrument that the limits of the sinus might be overstepped and the cranial cavity might be opened. As a means of avoiding deformity, the operation was an artistic one. He had succeeded in curing an old-standing case of frontal sinus suppuration by opening the sinus with a dental burr under the brow, where it forms an angle with the nose—absolutely no after-deformity resulted. In cases where deformity is not of so much importance a more radical operation is performed.

Dr. Kenny had used burrs driven by a dental engine. He found them very satisfactory to work with, but did not find the dental engine strong enough. He would get a surgical engine like Dr. Brady. He liked opening the antrum of Highmore with a burr.

Dr. Lockhart Gibson had for several years used Macewen's burrs for opening the mastoid antrum, especially in children, and thought that no better instrument could be devised. He had a strong dental engine, but was satisfied that a stronger engine, such as Dr. Brady now employed, would be more satisfactory to work with, and quicker.

QUININE AMAUROSIS.

By T. S. Kirkland, M.D. Glas.,
Sydney.

Quinine amaurosis is comparatively rare in the records of ophthalmology, and I beg with this contribution to add another case to the at present scanty literature on the subject.

Since the isolation of the alkaloid by Pelletier and Caventon in 1820, eighteen cases have been collected by A. E. Browne, and eleven cases by Gruening, and sixty-nine by De Schweinitz—in fact, those collected by the latter embrace the former; so that in a period of seventy-six years very few cases have come under the notice of the oculist. It is more than probable that the disease is more common than published statistics would have us believe.

Quinine is a largely used antipyretic, and has been recommended in large doses, especially in malaria. It is sometimes used for long periods of time, so that we should expect quinine amaurosis to be more frequent than it apparently is.

The Anglo-European residents of India look upon quinine as their sheet anchor in disease, and as a means of preventing the development of febrile manifestations. They resort to it upon the slightest occasion, and hence we find a good number of the recorded cases to have suffered from malaria.

Mr. Nettleship published two cases following the administration, in one case, of a fairly large dose for Congo fever, in which the patient stated that blindness caused by the fever was very common on the east coast of Zanzibar —*i.e.*, the quinine caused the blindness which was attributed to the fever.

In general textbooks on therapeutics very little mention is made of quinine amaurosis, while the disturbance of hearing is such a common occurrence that every practitioner is familiar with it. In some of the cases the authors themselves were in doubt as to their etiology; and when specialists have so much difficulty in deciding, we can hardly expect ordinary practitioners to be dexterous enough in the use of the ophthalmoscope to recognise these cases when they occur.

All cases in the pre-ophthalmoscopic days were called by the general name of "amaurosis." This may account for their scanty record in those times. Even at the present day there are those who would contest the cause, as it occurs in most cases in association with other disease, thus making it difficult to exclude its associate and thus satisfy the stringent demands of science. In a case in point, recorded by Knapp, he was disposed to attribute the cause to meningitis until the history of the case pointed to the true diagnosis.

In a case seen by Michael a similar mistake occurred. A competent ophthalmologist at his invitation examined the patient ophthalmoscopically, and pronounced it a case of primary atrophy of the optic nerve. Now, while all the ordinary appearances of atrophy were certainly present, he felt justified in denying the case to be one of optic atrophy. He says: "I hold that atrophy of the degree indicated by the appearances of the disc could not possibly occur in the short space of one month, and science had yet to recognise the existence of a white disc with minute vessels which was not indicative of existing atrophy. I regarded them as blanched discs, the cause of which I was disposed to attribute to spasmodic contraction of the musculature of the arterial walls." This case followed the administration of quinine during an attack of pneumonia, in which the physician attending failed to see any connection between his treatment and the resulting blindness.

In a single and isolated case by Giacomini the drug was taken by mistake. In my case the drug was taken for toothache; so that we have here two cases which exclude other factors in their causation.

William Jackson, æt. twenty-nine, barman, in February, 1894, took sixpennyworth of quinine (120 gr. of sulphate) at one dose in a quarter of a tumblerful of water, followed by vomiting, which lasted for eighteen hours. The vomit was mixed with blood; he complained of pains in the head, but no deafness. The following morning he was quite blind, and could not distinguish light from darkness with either eye: "the sight seemed to go all at once." Four days later sight began to improve gradually, and in three weeks' time he was able to read; having improved thus far, he returned to his former employment. Three mouths later, his vision became worse, at which time the following notes of his vision were taken:—

June 8, 1894.—R.E. $\frac{6}{9}$, L.E. $\frac{6}{9}$, fields very contracted, discs pale, each fundus anæmic and with white lines along the arteries and veins; the vessels are contracted to fine threads. There is evidently perivasculitis present, and the fundus looks as if the condition had followed optic neuritis.

June 28—V. reduced to P.L. only, pupils dilated and inactive, knee jerks exaggerated with ankle clonus.

October 9—V. R E., J.20; L.E., J.18 words; treatment by amyl nitrite improved his vision, which, later on, was R.E., J.20; L.E., J.12; nitro-glycerine tabloids given, one every other day.

October 23.—R.E., J.20; L.E., $\frac{6}{12}$ 2 letters J.1.

April 16, 1895.—R.E. $\frac{6}{18}$ J.2, L.E. $\frac{6}{9}$ J.1.; the pupils 9 m.m. oscillate when the light is thrown on to the eye, but they do not contract. They react to convergence.

The fixation point remained uninvolved, and his central vision included the inner circle of the perimeter chart. The fields were taken at the first examination, and only very slight improvement followed. It was impossible to estimate his colour vision with such telescopic vision. We have here a case of blindness following upon the administration of quinine, and declaring itself within twelve hours. The examination reveals perivasculitis with consequent narrowing of the vessels, accompanied by pallor of the disc, concentric limitation of the field, and pupils non-responsive to light, while behaving in the ordinary way to convergence and accommodation. The latter two symptoms point to non-involvement of the nuclear fibres of the third nerve, while the former points distinctly to a local lesion. This lesion might be explicable upon two hypotheses. Firstly, double embolism of the central artery of the retina; and, secondly, upon a primary atrophy, associated with lateral sclerosis, to which view the presence of exaggerated knee jerks with ankle clonus might lend some colour and support. Embolism of the central artery is rarely double, presents no signs of perivasculitis, and the sight is irrecoverable if the vessel is completely occluded. Against the latter view, it is hardly necessary to enter into the differential diagnosis, as the atrophy in this case is always a gradual process, and not a sudden affection. Having established the case to be one of quinine amaurosis, let us now examine the symptoms, and note wherein they differ from and agree with other cases reported by other observers. I have not been able to find any case in which, after improvement, deterioration began, followed by subsequent improvement without any apparent cause. Whatever produced his decline in vision was evidently not due to any permanent cause. Nettleship specially mentions that these cases are extremely susceptible to the action of the drug, and that even small doses will produce manifest impairment of vision. It agrees with most of the cases in the main symptoms—viz., in pallor of the discs, contracted arteries, concentric limitation of the fields, dilated pupils insensible to light while reacting to convergence. It is unusual, however, to find such marked disturbance of sight without involvement of the auditory apparatus. Other symptoms have been observed in other cases, *e.g.*, anæsthesia of the cornea (Voorhies), cherry-red spot at the macula by Browne and Gruening, divergent strabismus in Knapp's, in Browne's nystagmus, lateral and vertical, and in the experiments conducted by de Schweinitz proptosis was noted. Gruber, in his *Ohrenheilkunde*, mentions a case of glaucoma produced by quinine and operated upon by von Arlt. This patient took about 100 gr. of the drug, when she was seized with the symptoms of glaucoma. The dilatation of the pupil probably accounted for its onset by narrowing the angle of filtration.

Tiffany's case is reported with increased tension.

Nettleship's cases showed a marked contraction of the field for colours—at any rate for red and green, the colour being as a rule definitely recognised only when close to the centre. "It is interesting to note that the place of worst colour perception was between the fixation point and the blind spot," and that apart from its colour the coloured spot always looked darker in this situation than at the corresponding point to the inner nasal side of the fixation point.

Todko's had a double central scotoma; Galezowski's patient had a transient central scotoma.

In nearly all the cases in which large doses had been given, complete blindness of a temporary character was the first symptom, central vision being restored after a lapse of time varying from a few hours to a few weeks. It is worthy of note that the lesion must be situated in a different part of the optic nerve from that of tobacco amblyopia, in which a central colour scotoma is the main symptom, and the lesion is found in the maculo-papillary bundle.

Dickinson in his cases mentions a congestion of the retinal and choroidal vessels, with tumefaction of the optic nerve. The visual fields are usually contracted, and, as stated by Gowers, are usually transversely oval; but this is not invariably the case, as will be seen by the accompanying charts.

The clinical picture of these cases is so complete and perfect, and differs in such a marked degree from all other ocular affections, that their recognition should be a matter of comparative ease. It is most likely to be confounded with optic atrophy; but a careful study of the condition of the arteries, together with the history of the case, should simplify the diagnosis.

The prognosis, so far as can be gathered from published cases, depends upon the degree of intoxication. In the minor forms, recovery may be expected without limitation of the field, while the graver recover central vision with improvement in the periphery; but in no case amounting to complete convalescence. Knapp and de Schweinitz think it possible that further atrophy may supervene, leading to complete and permanent blindness. The drug in most instances gained entrance to the circulation in the ordinary way—viz., through the stomach. In one of Knapp's cases it was given by enema. The form in which it is given seems to have little significance. The sulphate is undoubtedly stronger than the cinchonine preparations, while in de Schweinitz' experiments upon dogs with Quin. bimur. carbamidat, the effect was found to be greater than with ordinary sulphate of quinine.

It is not always necessary to administer a large dose in order to have the preceding symptoms developed, as in some cases the dose was apparently a small one; in Nettleship's first case only two grammes were taken. Mr. Nettleship explains this by saying that the patient was intolerant of the drug.

Gowers says that the amount of quinine which caused the symptoms varied from a minimum of 80 gr. in thirty hours to a maximum of 1,300 gr. in three days. (This is merely quoting Knapp.) It is obvious, however, that the minimum dose is really much less than he has stated, as in Holtz' case 40 gr. were responsible for reducing the vision from $\frac{16}{32}$ to $\frac{16}{200}$.

The blindness may set in within a few hours, as in my own patient's case, or be delayed for several days. Blindness, as a rule, persists for three or four days; in fact, its duration seems to be largely influenced by the amount of the drug ingested.

It appears that young and old are alike subject to the deleterious influence of the drug. The age in Browne's cases varied from seven years to forty-five, while in those of de Schweinitz it varied from three to sixty-five, and in each case the greater number were between the ages of thirty and forty. If any age is more susceptible than another, the period of complete adolescence seems to be the one.

The treatment so far adopted has not been shown to have any direct effect. Graefe treated his two cases with Heurteloup's leech, which, however, is not founded upon a very scientific basis if the present view of its pathology is accepted. Each application was followed by a distinct improvement, which can only be explained on the supposition that the venesection deprived him of a certain amount of highly cinchonised blood, thus removing the cause of irritation. We could conceive this to have been beneficial immediately after the symptoms manifested themselves, and before the excretory channels (the kidneys and salivary glands) had got rid of the drug. Stimulation of the intestinal canal might be used as an adjunct to this line of treatment, with free diaphoresis. Depletion, however, is to be used in a guarded way, lest we should lower too much an already lowered vascular tension. It is to be remembered that while quinine in small doses is tonic in its effect large doses depress the heart through their effect upon the heart muscle itself and its intrinsic ganglia. It has not been noted in the cases published the exact condition of the general circulation. One would expect that the circulation would return to its normal standard, and then justify the administration of local and general vasomotor dilators, *e.g.*, nitrite of amyl and nitro-glycerine. This method of treatment was adopted in Jackson's case, and seems rational enough to warrant further application. Gruening, however, observed no change in the retinal vessels, after his patient otherwise showed its effect. The reason for this will be adverted to later on, when we come to consider the pathology of the disease. Strychnine has been recommended in this as in other diseases of the optic nerve on the ground of its stimulating effect, but, as I hold this disease to be primarily due to vascular changes, it is difficult to comprehend in what way this drug can be useful. The cessation in the administration of the drug should be insisted upon immediately vision becomes notably affected. Galvanism has been employed, but with what success we cannot at present tell. Browne mentions that it is possible that the cervical sympathetic has something to do with the changes induced; this might furnish a reason for its use.

It is a common practice to use hydro-bromic acid in conjunction with quinine, in order to lessen the auditory disturbances, so that we would expect it to have some influence, in an antidotal way, in quinine amaurosis. Potassium iodide has produced in a case by Calderai a beneficial effect. Pathology is perhaps the most interesting point in connection with all diseases, so in this a comprehension of its true nature is the only way leading to its rational treatment. Previous to the experiments of Brunner and De Schweinitz, the pathology of the disease was entirely hypothetical, and founded chiefly upon the condition of the fundus seen by the ophthalmoscope, although the expressed pathology of these two writers in no way helps to reconcile its accepted action upon the ear with that upon the eye. In the case of the ear it is generally believed and taught that hyperæmia of the middle and internal ear takes place. Can we believe that its effect upon these two sensory nerves can be diametrically opposite—in the one case producing hyperæmia and in the other case ischæmia? It is possible and likely that an intermediate stage of short duration occurs, producing dilatation of the retinal vessels, and succeeded by subsequent contraction. This view is supported by two experiments which I undertook in order to determine this point, and which shall be mentioned later on. Let us first study the anatomical relations of the intra-ocular vessels, and see if it is possible for dilatation to occur sufficiently great to be recognised except by the closest inspection. The eye is supplied by three systems of blood-vessels—namely, the conjunctival, the retinal, and the uveal; that of the conjunctiva is free to expand under any influence, not being subject to or restrained by external pressure.

The retinal vessels, on the other hand, have to pass through the sclerotic ring in the centre of the optic nerve, so that any marked dilatation would be hindered by the unyielding scleral ring; furthermore, the intra-ocular divisions of this artery are subject to intra-ocular pressure, and this, I think, accounts for the non-recognisable expansion of these vessels, in the case recorded by Gruening under the influence of nitrite of amyl. Upon this theory Gowers built his hypothesis explaining how pulsation of the veins occurs in aortic regurgitation.

I do not here for a moment maintain that dilatation of the retinal arteries cannot occur, but I wish to emphasise the statement I have already made, that these arteries, owing to their environment, cannot expand like other arteries in the body. Given a case in which expansion takes place in the optic nerve, the inevitable result must be displacement of the lymphatic fluid in the vaginal space, which space is relatively small, allowing expansion in proportion to the displacement. This, of course, would hinder the egress of blood through the retinal vein, and thus produce enlargement of the intra-ocular veins which Dickinson has actually observed to take place, together with tumefaction of the choroidal vessels and the optic nerve.

Roosa experimented upon Dr. Hammond, giving him 10 gr. of quinine, which was followed in half-an-hour by congestion of the inner and outer canthus, together with congestion of the whole surface of the palpebral conjunctiva, one hour later by ringing of the left ear, burning of the auricles, and flushed face; as these vessels are not subject to pressure, they are free to dilate. Later, he observed vessels running along each malleus; this, however, is opposed to the experiments of Guder carried out under the supervision of Weberlieb, in which the temperature of the external ear was not raised, and no vessels were seen on the tympanic membrane.

Professor Jacobi says that, as the results of experiments in Germany and of clinical experience, anæmia of the ear and eye is the first phenomenon observed. Dr. Kurchner, of Wurzburg, gave large doses of quinine to rabbits, and on killing and examining them, he found hyperæmia and hæmorrhage in the mucous membrane of the tympanum and in the labyrinth; he attributes these changes to vaso-motor disturbance, leading to congestion and exudation.

Roosa believes that hyperæmia and dilatation of the arteries at the base of the brain actually occur together with a true vasculitis of the retina. Large doses of quinine produce intense frontal headache, suffusion of the eyes, flushed face, dull expression, stupidity and delirium, and finally convulsions; these symptoms point to cerebral vaso-motor change, and not entirely to their supposed specific action on the eye and ear. Probably the serious involvement of the ocular and auditory nerves is due more to their anatomical surroundings than to any affinity those nerves possess for the drug itself; in fact, the change is one primarily due to change in the vessels reacting upon the nerve. This view is supported by the experiments of Barabaschew upon human beings. He concludes in the following manner:—"In quinine poisoning the vascular system plays an important rôle, essentially through the medium of the vaso-motor centres, which by reason of its condition of irritability causes strong contraction of the peripheral vessels; whether the changes in the peripheral vessels are entirely dependent on this condition of irritability cannot at present be decided." One of the best-established effects of quinine is its effect upon the spleen, producing contraction of that organ, which we may assume is vascular in character. Another well-known effect is that of lessening protoplasmic and amœboid movements, checking the migration of the white corpuscles of the blood. Following out this line of argument, and combining it with two experiments by myself upon rabbits, the following is the sequence of events:—Firstly, large doses produce transient and very moderate dilatation of the vessels of the retina, succeeded by contraction, retarding of the blood current, leading to a lowered vitality of the tunica intima which favours thrombosis with its results. The following are the two experiments:—A rabbit weighing 1½ lb. was given 5 gr. of quinine at 7·30 p.m., and the state of the retinal vessels accurately noted; at 8·30 p.m. very slight dilatation of the veins was observed; the artery appeared to be of the same size for some distance from the optic disc, and at one point was covered by a slight haze indicating a small degree of exudation. It was difficult to determine to what degree vision was affected; on holding some lettuce before it, no notice was taken of it until it came in contact with the nose. On the following evening, a similar dose was administered, but before giving it the haze of the retinal artery was noted to have disappeared, while the veins remained of the same size as on the previous night. Three hours later the veins were decidedly smaller, while, so far as could be judged, the arteries remained of the same size.

De Schweinitz has enunciated the theory of the selective action of quinine upon eye and ear; and in stating it he compares it to the action of digitalis upon the heart. This supposed selective affinity can be easily explained by their anatomical environment. In the case of the auditory nerve, it passes through unyielding bony channels, and so any inflammatory or other affection of this nerve places it under totally different conditions from that of any other cranial nerve. We have already spoken of the anatomical connections of the optic nerve. The change induced by quinine is really a vaso-motor change, acting in the same manner in all organs of the body, but more accessible to observation through the transparent media of the eye than in other parts. The eye has ever been regarded as a suitable organ for observing changes in disease. Years ago the phenomena of iritis were compared to inflammatory processes in serous membranes.

De Schweinitz, as the result of his experiments upon dogs, found the following changes in the optic nerves. He says: "I may say, with regard to the microscopical appearances, that there are thickening and changes in the wall of the optic nerve-vessels (endo-vasculitis); organisation of a clot the result of thrombosis—an organisation which has been carried on even to the extent of its being channelled by new vessels; the widening of the infundibulum of the vessels, as the result of the constrictions of the surrounding nerve fibres, causing appearances not unlike a glaucomatous excavation; and, finally, practically complete atrophy of the visual path, including the optic nerves, optic chiasm, and optic tracts as far as they could be traced."

De Bono has observed three cases in human beings, and has made experiments on dogs and frogs. He believes that quinine paralyses the movements produced in the retinal elements under the action of light by an intoxication of the protoplasm; and further believes that the action of the drug is upon the rods and cones. This, of course, ignores its action upon the vessels, and is based upon the idea that the visual purple is hindered in some way from forming. Microscopically, no effect has been noticed upon the rods and cones.

Finally, Buller's view of the pathology differs entirely from either of the preceding. He believes that blindness is caused by a rapid effusion of serum into the lymph spaces around the optic nerves, sufficient to produce œdema and impede the circulation. This view is hardly consistent with clinical observations, as in no case, except Dickinson's, was any decided swelling of the papilla observed. Before concluding this paper I may mention a curious phenomenon—viz., the occurrence of quinine crystals in the anterior chamber of the eye, in a case recorded by Lang, in which quinine was used as an eye lotion. It is difficult to understand how this could occur, unless it was absorbed in solution, and re-crystallised on entering the anterior chamber.

It is always interesting and instructive to study the charts in connection with diseases of the fundus. Of the six charts here shown, the case recorded by me is the one in which the greatest contraction has taken place. They become progressively larger, beginning with Browne's and ending with Mellinger's; those by Gruening occupy an intermediate position. The relationship between the dose and the visual contraction will be best studied by noting in each case the amount of the drug given, which I here append.

In each field the macular region has recovered and has remained permanently good. Can we account for this on the anatomical distribution of the fibres of the optic nerve? Bunge has stated that the bundle of fibres going to the macular region is no less than one-fifth of the whole nerve, many of which are direct, and a few crossed. In photographs of sections of the optic nerve of the dogs examined by De Schweinitz, atrophy and degeneration seem to be spread uniformly throughout the whole transverse section of the nerve, so that apparently the maculo-papillary bundle is involved as well as the other fibres of the nerve. This is not in accordance with the clinical experience, in which it is clearly shown that, even in cases where the macular portion of the nerve is affected in the early stages, recovery subsequently takes place; so that this part of the nerve should exhibit no degenerative change. Around the macular region there is a plentiful capillary circulation which, no doubt, maintains this part of the retina in greater vitality, and yet in retro-bulbar neuritis this is the favourite localisation of the disease. In quinine amaurosis, if we accept the results obtained by De Schweinitz upon dogs as final, we have yet to invent a theory to account for the escape of the maculo-papillary area. His pictures clearly show that the change present in the sections is evenly distributed over the whole nerve and not confined to any one section. No case of permanent blindness has yet been recorded in this disease, so that it may be affirmed that this portion of the nerve is never affected permanently, and the reason of its escape so far is more a matter of conjecture than of actual knowledge, and also that pathological research has only partially accounted for the amaurosis In some of the cases scotomata are recorded, especially by Galezowski and Todko; in the former without restriction of the field. It is probable, however, that some other cause was present to account for this change, as these are exceptions as compared with the large number in which no scotomata were observed. I would suggest a reason for the speedy and permanent recovery of vision in the fixation point—viz., that as the disease is essentially due to vascular change. and this part of the retina being endowed by a better capillary circulation, free inosculation finally establishes and maintains a better blood supply than in other parts of the retina.

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AURAL VERTIGO.

By J. Lockhart Gibson, M.D. Edin., M.R.C.S. Eng.,
Brisbane.

A discussion which occurred at the March meeting of the Laryngological Society of London indicates that the theory of aural vertigo is still not universally accepted. Mr. Atwood Thorne, and those taking part in the discussion, will pardon my quoting somewhat fully from the report in the Journal of Laryngology,* as by doing so not a little interest will be added to the account I am about to give of two remarkable cases, and to the explanation of aural vertigo which they and other less noteworthy cases have caused me to favour.

Mr. Atwood Thorne's case was called by him one of laryngeal vertigo. The patient, he said, gives the following history :—" Whenever he has a fit of coughing he gets giddy and lurches toward his right front. He has been subject to paroxysms of coughing on and off for two years, but the condition has lately been getting worse. He has never fallen, but has to catch hold of something to prevent his doing so. He is slightly deaf, and for the past two months has had noises 'like heavy traffic' in his head. He has polypoidal hypertrophy of both middle turbinates, some lymphoid hypertrophy at the base of the tongue, and some slight swelling in the inter-arytenoid space. There is some pulmonary emphysema."

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* Journal of Laryngology, Rhinology, and Otology, May, 1899, p. 255.

To this account is added the following statement:—"No other causes of vertigo being ascertained, the case is brought forward as one of laryngeal vertigo."

In the discussion which followed:—

Dr. Law thought it was a case of aural vertigo, and pointed out that the patient complained of deafness, and that catheterisation would reveal the Eustachian tubes to be over-patent; the man, he said, had probably for some time given his ear repeated concussions either by coughing or blowing the nose.

(With Dr. Law's diagnosis of aural vertigo I would like upon the facts recorded by Mr. Thorne to cordially agree, but his explanation of the manner in which the aural vertigo was brought about requires some modification.)

Dr. Hill, who had treated the case with Mr. Thorne, confessed to three separate diagnoses. His first was aural vertigo; his second, nasal vertigo; and his third, laryngeal vertigo, because the attack always came on in connection with some laryngeal irritation and cough.

Dr. Dundas Grant said that, although the theory of aural vertigo had been propounded by some authors, he was indisposed to accept it, if only because of the extreme rarity with which vertigo followed inflation of the middle ear, a result he himself had never seen. In a case, he said, of very definite laryngeal vertigo, or, rather, syncope, as it was better called, there was a strong gouty tendency, after treatment for which he believed the vertigo had disappeared.

Dr. St. Clair Thomson suggested that Mr. Thorne's case might be one of cardiac syncope. The patient's pulse was small and weak and slightly irregular. The man himself had said that when he bent forward to lace his boots he felt inclined to fall on his nose.

In February, 1890, Z.Y., male, aged forty-six years, gave me the following history:—Five years ago, had an attack of giddiness from no apparent cause; it lasted three or four hours, and he had a similar one a year later. Fifteen months ago, a severe attack started after breakfast, and lasted three or four hours. With this attack he vomited. Has had attacks pretty often since; not always sick with them. Has had no sickness for the last seven months, during which time attacks have been less frequent than during previous seven months. Has known attacks last for two or three days, but not severely. Fell down with one attack, but recovered quickly, and only felt "seedy" for a short time afterwards. Right ear has had singing in it for many years, and it hears imperfectly. "Hears perfectly with the other ear." Has been treated for the past six months with a mixture of bromides of potash, ammonia and soda, iodide of potash, and bicarbonate of soda, he says, under the impression he was suffering from epilepsy.

Examination.—Tuning-fork heard equally from vertex; right ear, air conduction better than by mastoid; left ear, louder by mastoid than by air conduction, though not longer. Watch (4-yard watch): Right ear, 3 inches; left ear, 40 inches. Both membranes indrawn, specially the left, and both opaque, the right being more so, and its light cone smaller. Politzerisation sends air in very badly, with immediate slight improvement in a giddy feeling which existed, and no appreciable improvement in hearing. The slight lessening of the giddy feeling was, however, very temporary, as it soon returned and increased, and he began to stagger in his walk. The staggering increased so much that I had to make him lie down. He began to retch, and broke out in a cold perspiration; the very giddy condition lasted for some hours. On further examination I found him to be suffering from psoriasis palmaris and plantaris. The diagnosis was aural vertigo, due to a combination of labyrinthine disease of a tertiary syphilitic nature and to Eustachian blocking. The former appeared to me to be the predisposing cause; but it was probably not, as a rule, enough in itself to bring on an attack unless when aided by increased pressure on the fenestræ of the middle ear. The attack brought on in my room appeared to be explained as follows:—

Prior to politzerisation he experienced slight giddiness owing to Eustachian blocking, consequent exhaustion of air in the middle ear or ears, and consequent imperfectly-resisted atmospheric pressure in the external meatus forcing the drum membrane and the footplate of the stapes inwards, and thus increasing the intra-labyrinthine pressure. The air went very imperfectly into his middle ears, but there was an immediate slight improvement in the giddiness, no doubt due to the temporary patency of the Eustachian tubes. The tubes, however, being very narrow, immediately closed up again, leaving the middle ears for the time being over-distended with air. As the drum membranes had become accustomed to a retracted position, they failed to yield to this, and the fenestræ were, therefore, still more forcibly pressed upon by the positive pressure in the middle ears themselves than had previously been the case by atmospheric pressure. This explanation seems to me to be more exact and complete, and more in accord with the facts, than to attribute the vertigo to increased middle ear pressure alone acting on a healthy labyrinth. I attributed the attacks chiefly, if not entirely, to the state of the right ear. Antisyphilitic treatment was advised, with a result that he lost his vertiginous attacks entirely, and they have not returned. I was told that his hearing also improved.

My second case was sent me on 11th May, 1899, by Dr. David Hardie, whom he had come from North Queensland to consult on account of attacks of giddiness.

May 11.—D.M., fifty-two, male, has suffered from attacks of giddiness for the past seven weeks, one every week, on about the same day of the week. The attack comes on suddenly, and is followed by vomiting, which relieves the giddiness. Is "slightly" deaf in one ear. Was asleep when first attack came on, and awoke to find himself giddy.

Examination.—Both tympanic membranes quite opaque and pretty indrawn. Tuning-fork heard on left side from vertex. Heard twenty-five seconds longer by air than by mastoid on left side, ten seconds longer by mastoid than by air on right side Watch (4-yard watch) heard R.E. close contact; L.E., 3 inches. Nasopharynx and pharynx healthy. Anterior nares: a marked deflection crest in the left nostril, and a very large inferior turbinated body in the right nostril. Politzerisation did not cause immediate improvement in hearing.

Diagnosis.—Chronic catarrh of middle ears of long standing; extension latterly to each internal ear. Attacks of Eustachian blocking during last seven weeks—the last straw in inducing the attacks of vertigo. Advised regular politzerisation, and gave a guarded to hopeful prognosis if nose treated as well, and made a better breathing channel.

May 15.—Has had no attack since visit on 11th May, and thinks head has been clearer, though he is hearing no differently. Politzerised again. He became distinctly a little giddy afterwards, complaining of a "full feeling" in his ears. He sat down in the waiting-room for a little, as he felt too uncertain of himself to walk away. He grew rapidly worse, and I had to make him lie down. He retched violently and repeatedly, and broke out in a cold perspiration, his face becoming extremely pale. For a period of two hours the least turn of the head around its vertical axis occasioned fresh retching. I was surprised to learn from him that his weekly attacks had been (some of them) fully worse than that I witnessed. After the two hours of retching, he slept for two hours. My explanation of this attack will be gathered from that given in the former case I concluded that I had brought it on by over-filling with air a chronically contracted middle ear or ears; that immediately thereafter the Eustachian tube or tubes closed; and that the increased pressure on the fenestræ of the middle ear induced the vertiginous attack, the labyrinth being the subject of chronic changes predisposing thereto. Possibly one or both horizontal semi-circular canals were the parts of the internal ears most answerable.

Although pleased to have demonstrated to myself and the patient the correctness of my diagnosis, I had no wish to reproduce such really alarming symptoms. I decided, therefore, to give him more room in his nose before again practising inflation of his middle ears. As the very hypertrophied inferior turbinated was causing more obstruction in his right nostril than the somewhat irregular crest in his left, I decided to cauterise it. This was done freely on 18th May. The nostril became very much more free, so much so that I ventured to let him go home on 30th May without removing the crest from his left nostril. He had been fifteen days without an attack, was breathing nicely, and his hearing was somewhat better—viz., R.E. watch before contact; L.E. 4 inches.

In answer to inquiry I received a wire from him last week (about 15th September), four months after his visit to Brisbane, to say, "Breathing well; hearing much the same; no return giddiness."

Vertigo, to be truly nasal, would require to be a reflex nasal neurosis. To call it nasal when the internal ear is the true peripheral starting point of the reflex path seems to me to be inexact. Doubtless, in one of my cases, possibly in both, the nose first and the Eustachian tube and middle ear after, induced the labyrinthine reflex; but the nervous path commenced, surely, in the internal ear. To accept a diagnosis of laryngeal vertigo in any given case, I should require evidence of healthy internal and middle ears.

For the induction of aural vertigo, however, we do not of necessity require a diseased labyrinth, as it is quite possible to conceive of the intra-labyrinthine tension being sufficiently increased to start the reflex from a healthy labyrinth. I have, indeed, observed two attacks of vertigo, which I believed to be so induced.

Mr. Atwood Thorne's case would bear the following explanation:—Chronic middle ear catarrh with indrawn membranes. Eustachian blocking, violent pumping of air into middle ears during paroxysms of coughing—subsequent closure of Eustachian tubes with consequent increased air tension in the middle ears, and increased pressure on the fenestræ, inducing vertigo by means of increased intra-labyrinthine pressure on an already diseased internal ear.

Dr. Ferguson agreed generally with Dr. Gibson's interpretation of the cases, and expressed surprise at Dr. Dundas Grant's doubt regarding aural vertigo. He had had cases falling off his chair after politzerisation owing to giddiness.

Neither Dr. Ferguson nor any other present had observed such severe symptoms after politzerisation as in Dr. Gibson's cases.

Dr. Gibson explained, in answer to a question by Dr. Kirkland, that when hearing by air conduction was very much reduced for ordinary sounds, and the low-toned tuning-fork was, in spite of this fact, better heard by air conduction than by bone conduction, it seemed to him fair to conclude that the labyrinth was implicated.

STRABISMUS.

By A. W. Orr, M.D. Dub.,
Brisbane.

In asking you to listen to notes on a case of alternating divergent strabismus treated according to the method advocated by Javal, I have ventured to do so, because the educative treatment of strabismus, as it is called by Priestly Smith, seems to me worthy of our consideration, and I hope that even so slight a reference to it as the case affords will lead to a discussion of the subject.

M.E., aet. fourteen, schoolboy, was brought to me on the 19th September, 1896. He complained of headaches and pain in the eyes on reading. Divergent strabismus was well marked. It was stated to vary in amount from time to time, being sometimes well marked, at other times hardly noticeable. No definite information could be obtained as to the period which had elapsed since it first appeared. He states, however, that for the last three or four weeks he has been accustomed when tired to use the eyes alternately, especially when reading. This enables him to read longer and with less discomfort. The non-fixing eye deviates outwards, and is not closed. He has never noticed that objects become double.

Alternating divergent strabismus. Fixation of R.E. preferred.

Monocular vision for prox. et. longe.

V. of either eye $\frac{6}{6}$, 1st July, pp. 11 cm. Apparently no error of refraction.

Some days later refraction tested by retinoscopy under cocaine and homatropine—same result obtained.

Perimetric measurement of squint angle, R.E. 15° L.E. 20°.

He has greatly outgrown his strength, and is not in good health. As it was thought possible that when his general condition improved the squint might diminish in extent, and as his parents were averse to any operation being done, he was removed from school. He was allowed to read for such time only as could be accomplished without fatigue. In this way the time was gradually increased from five minutes to an hour. Dr. Odillo Maher, of Sydney, who saw him in January and February, 1897, found no error of refraction, but as his vision under atropine was $\frac{6}{6}$, with + 1·25 sph., ordered him spectacles for reading, + 0·75 sph. These he found enabled him to read with much more comfort. Operation was suggested, but refused. Their use was continued till January, 1898.

In the hope that, by the adoption of Javal's method of treating concomitant strabismus, binocular vision might be re-established without operation, and, even were such necessary, the enforced use of the left eye would contribute towards such result, I decided to make the attempt, and began on 31st January, 1898.

The patient's general health had improved. Vision, however, was still monocular, and the amount of the squint remained unchanged. The accommodation of the R.E. was paralysed, a solution of atropine being used thrice daily for a fortnight, and the R.E. covered with a shade. No error of refraction discovered. Use of atropine discontinued. When mydriasis had passed off, the shade was transferred to the L.E., which was treated in a similar way. Retinoscopy showed slight astigmatism + 0·5 cyl. axis 165°, V. $\frac{6}{8}$ letter. Diplopia, on removal of the shade, which could at first only be obtained when one or other eye was covered with a coloured glass, was immediately produced on the 4th March.

Exercises with a simple stereoscope and cards H and I series were employed daily.

March 11.—Measurement obtained with card H was 3, card 9 J 2½ fused.

Failing to recognise the reason why, in spite of the amount of divergence, the measurement obtained was so small, I wrote to Dr. Javal, asking him how such could be explained; if he considered operation necessary or advisable, as enabling the time of treatment to be shortened; if necessary, should advancement be preferred to tenotomy.

March 18.—Measurement with card H, between 5 and 6 fusion card I 5½. I then discovered that there was crossed diplopia for distance, which would explain the small measurement obtained with card H. The case was therefore one of false projection, necessitating operation.

I decided, however, to wait till I received a reply to my letter. Meanwhile a shade was worn constantly over one or other eye—generally the left—and exercises with the stereoscope, &c., continued at intervals.

Dr. Javal kindly sent me an answer, which I received on the 11th July. He explained that with the amount of divergence present the measurement obtained with card H would have been greater than 6 were it not a case of false projection.

The treatment he advised was as follows:—

1. Shade to be worn permanently over the better eye (R) for one or two months.
2. Tenotomy of ext. rect. of L.E., such imperfectly correcting the deviation and producing direct images (homonymous) in spite of the remaining divergence.
3. Transformation of the homonymous into crossed diplopia (as in the case of H.B., p. 366 of his Manual of Strabismus).
4. Operation on the other eye, R.E.

The R.E. was covered with a shade, which was worn permanently, being only removed during exercises, till 11th September.

September 9.—Angle of squint measured—Longe, R.E. 17°, L.E. 25°; prox. R.E. 10°, L.E. 12°. V. R.E., $\frac{6}{4}$ 6 letters; L.E. + 0·5 cyl. axis 165° $\frac{6}{5}$ 5 letters.

September 11.—Tenotomy ext. rect. L.E. under cocaine and gas—slight divergence remaining, crossed diplopia for longe et prox., images about 6 inches and ½-inch apart respectively; six hours later, card I 6 almost fused. R.E. covered with shade. Exercises, stereoscopic, &c., continued daily from half to two hours.

September 13.—Homonymous diplopia for distance.

September 21.—Angle of squint, L.E. 5°.

September 23.—Tenotomy ext. rect. R.E., under cocaine. Divergence fully corrected. Diplopia again crossed for distance. Exercises continued daily for from one to four hours. Bar reading attempted.

September 26.—Succeeded in reading with bar for ten minutes.

October 10.—Succeeded again with bar, with which he read for several hours. No longer found any difficulty in using it.

Exercises continued, chiefly with cards of I K and L series, for several hours daily till 4th January, 1899. Bar always used when reading.

Binocular vision for near and distant objects in spite of slight divergence of the L.E., which remains.

The exercises should, I think, have been continued for a longer period, but I was unable to devote the time necessary, and his parents were anxious that he should return to school. He was told to practise for a short time daily with cards K 3, L 4-3-2, and use the bar when preparing his lessons in the evening.

He returned to school in February. Three months later he found reading tiresome. The spectacles ordered by Dr. Maher—T. 0·75 sph., which I advised him to use when reading—gave him relief.

A few days ago he called to tell me that for the past month he has found them no longer satisfactory. The divergence of the L.E. has not increased, and binocular vision still continues.

If the addition of a cylinder correcting astigmatism of the L.E., which at present he finds more comfortable, fails to afford him relief, possibly it would be wise to correct the slight divergence of the left eye which remains.

CONICAL CORNEA.

By H. Lindo Ferguson, M.D. Dub., F.R.C.S.I.,
Dunedin.

The President briefly introduced the subject of conical cornea, basing his remarks chiefly on fourteen cases of the condition which had come under his observation during the last few years. Of these ten were women varying in age from sixteen to thirty-five, and four were males ranging from fourteen to twenty-nine years. Several cases were only seen once, or seemed to be stationary when lost sight of, but two were certainly getting worse when they passed from observation. In four cases the condition became so bad as to call for operative interference. After reference to earlier experience of other operative measures, these four cases were stated to have been operated on by the use of the cautery. A small flat cautery was used to the apex of the cone, and a valvular paracentesis was then made through the floor of the ulcer thus formed with a fine chisel-shaped cautery. The anterior chamber took about three weeks to refill, except in one case where the edge of the iris plugged the opening and checked the drainage of the aqueous. This synechia was cut later with a sharp needle. The results were as follow:—

Miss A., æt. seventeen.—V. Right = Motion of hand, cornea very conical the apex of the cone showing considerable grey opacity. Left V. = 6/24, cornea also conical. Operation on right eye, May, 1896. Three years later V. Right = 6/60; left 6/60 bare, conicity increasing in the left eye.

Miss B. S., æt twenty.—Right V. = 6/24, cornea conical. Left V. = Fingers at 0·4 Metres, cornea intensely conical, extreme distortion of ophthalmoscopic images. Operation early in June, 1897.

Two years later V. Right = 6/60, cone increasing; Left V. = Fingers 6 M.; no distortion of the ophthalmoscopic images.

Mr. P., æt. twenty-nine.—V. Right = Fingers at 3 M.; Left V. = Fingers at 0·5 M. Both corneae were intensely conical. Operation on left eye in August, 1897. Six months later V.R. *stat. id.*; L.V. = Fingers at 5 M.

Miss W., æt. nineteen.—V. with each eye; Fingers at 2 M. Treated for six months, from October, 1898, with pilocarpine and bandages. On April 10, 1899, V.R. = F. at 2 M.; L.V. = F. at 1·25 M. The apex of the left cone was becoming cloudy, and was operated on. Three weeks later an anterior synechia was cut. On May 26, V.R. = F. at 2 M.; Left = F. at 5·5 M.

His routine treatment was to use pilocarpine drops to lessen the tension of the eye, and to use a compress bandage at night to support the cornea during sleep. When the conicity became extreme, he thought the cautery operation preferable to any other. The apex of the cone being generally below the corneal centre, the leucoma resulting from operation did not cover the pupil, and like other cautery leucomata cleared up to a remarkable degree. In none of the four cases had an optical iridectomy been necessary. His results led him to believe that in the cautery operation we had a measure which afforded reasonable hope of restoring useful vision in an otherwise very hopeless condition.

Dr. Pockley said he had never operated on a case of conical cornea, and he had seen very few. One case remained stationary for twenty years by changing his work to farm work. He could read a bit.

Dr. Kent Hughes cauterised each eye in one case, but did not open the anterior chambers.

The discussion showed that the experience of members had been very limited, and the treatment had not been attended with satisfactory results.

CICATRICIAL CONTRACTION OF THE ORBIT AND RESTORATION OF THE FORNIX CONJUNCTIVÆ FOR THE RETENTION OF A PROTHESIS.

By T. K. Hamilton, M.D. Dub., F.R.C.S.I.,
Adelaide.

There is probably no condition of the eye which is more difficult to deal with, and which taxes more frequently the patience and resourcefulness of the surgeon, than cicatricial contraction of the orbital conjunctiva, and, strange to say, though the condition is of fairly frequent occurrence, few authors refer to it except to point out its intractable nature, and these for the most part omit to give definite directions for its treatment. There have been, however, from time to time, especially of late years, various operative procedures recommended for the condition, some of which can be said to be successful, and some which can scarcely be considered such.

Amongst the cases which come under treatment in our cliniques where enucleation has been performed, there is always a percentage in which the insertion of a prothesis is either difficult or impossible. The cause of this difficulty is usually due to a partial or complete obliteration of the fornix conjunctivæ, the lower one usually, sometimes the upper, and sometimes both. The origin of this condition may be traced to several factors. One of these is to be found in a carelessly performed enucleation in which insufficient attention has been given to the preservation of as much of the bulbar conjunctiva as might or should be preserved. Another common factor is found in the long-continued non-use of a prothesis after enucleation. Trauma may be another factor in the causation of adhesive processes in the cul-de-sac either before or after enucleation. I have seen the condition result from irritation set agoing from the continued use of a broken prothesis or one sufficiently roughened on its edges to cause ulceration of the conjunctiva. Again, essential shrinking of the sac or the form of xerophthalmia, which in these colonies sometimes accompanies old cases of trachoma, and which Fuchs has pointed out to be due to a fatty degeneration of the epithelium and not of a true bacillary origin; such degenerative changes also come in as factors. These and many other causes combine to bring about the same conditions, which, although they present a variety of clinical pictures, are alike in these respects: That in one and all the wearing of a prothesis is either difficult or impossible, and in order to get over the difficulty operative interference is imperative. Since transplantation of flaps on the lines suggested by Reverdin, Thiersch, Wolfe, and others has come into use in general surgery, various ingenious operative procedures by flaps, with or without pedicles, have been recommended for orbital contractions as well as for pterygium, symblepharon, and the blepharo-cheiloplastic operations for entropion and ectropion. Dr. Moore, of Melbourne, has been one of our most recent contributors to the literature of plastic surgery, and for the admirable papers published by him in our Intercolonial Medical Journal, containing, as they do, so many practical suggestions on these and other flap operations, he has, I believe, earned the gratitude of many of his professional brethren.

In addition to these plastic operations, certain other improvements in ophthalmic surgery, as affecting the technique of these procedures, have further contributed to make operations of this kind a success. I refer more particularly to transplantation of spheres subsequent to evisceration or enucleation, and to the introduction of artificial eyes of improved shape. Let us deal with these two more in detail.

Removal of the eyeball or its contents in early life undoubtedly tends to create the trouble connected with the condition under review. In many of these cases the orbital tissues, the pad of fat included, atrophy and shrink to the utmost extent before a prothesis is or can be worn. It has been stated that an actual arrest of development of the bony walls of the orbit also follows early removal of its contents; but Gordon Byers, after making numerous experiments on such cases, proves that this is not the case. That shrinkage and consequent contraction and displacement of the orbital conjunctiva and the lids do occur is a matter of every-day observation, and hence the importance of anticipating these conditions. To do this I am in the habit of discouraging enucleation in young patients, and where it is unavoidable I order a full-sized, unbreakable prothesis to be worn at once and continuously until the patient is old enough to be allowed the usual kind of artificial eye. This, in the case of children, not only secures proper distension of the sac, for the prothesis can be increased in size as the child grows, but it meets the difficulty in connection with children wearing eyes before they are old enough to know how to take care of them, avoids the danger attendant on breakage, and saves unnecessary expense. The protheses I use are fairly thick leaden shells. One more precaution I take; following the suggestion of Schwenk,* I have the leaden shell perforated to prevent irritation of the sac caused by the retention of secretion and the œdema of the conjunctiva from the vacuum-formation, which the ordinary shells often cause. These perforations can also be utilised for washing out the sac without having to remove the shell each time, which removal is, under certain conditions, as will be shown later on, often undesirable. In addition to the prothesis, I now invariably do a sphere implantation after either evisceration or enucleation. This line of treatment will, of course apply, though in a lesser degree, to adults as well as to children. If evisceration can be safely performed, Mule's operation is done, and where enucleation is necessary Tenon's capsule is made the receptacle for the sphere. Since Mule introduced his method of implantation after evisceration in 1885,† there has been a marked difference of opinion amongst ophthalmic surgeons as to its safety. To get at the actual facts, and to find out how the matter really stands, the Ophthalmological Society of the United Kingdom appointed a committee in March, 1896, to consider the relative value of simple excision of the eyeball and the operations which have been substituted for it. This committee has brought up a most interesting and exhaustive report, which has been published in the Transactions of the Society (1898, p. 233), and which is not only a most useful historical record of the various operative procedures at present in use for these conditions, but is also an expression of the opinions, backed up by clinical facts, of the leading ophthalmic surgeons of the day. The report confirms the general impression that there is a certain amount of danger in adopting Mule's operation, except in carefully selected cases. I have, however, done a large number of Mule's, and I have had up to the present nothing but good results attending them. Should evisceration not be deemed advisable, implantation into the capsule of Tenon can be done. This method, originally suggested by Lang,‡ and afterwards improved on by Frost, is now known as the Lang-Frost operation. For various reasons, partly because the sphere sometimes became subsequently displaced outside the cone of muscles—I have had this occur in my own practice—and principally, I think, because the results were disappointing owing to provision not having been made in the operation for maintaining the correct position of the motor muscles of the globe, this operation had fallen into disuse, and not until lately—since Morton§ (Minneapolis) and Barrett‖ (Melbourne) modified the original, providing, as their operations do, for a proper advancement of the muscles—has the procedure been put on a proper basis, and has its lost reputation been re-established.

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* Oph. Rev., vol. xvi., p. 96.

† Brit. Med. Journal, Dec. 19, 1885.

‡ Trans. Ophth. Soc., vol. vii., p. 286.

§ New York Med. Jour. 1897.

‖ Intercol. Med. Jour., vol. iii.

Morton and Barrett, in their descriptions of the operation, both lay great stress upon the careful adaptation of sutures, so that the tendons assume the exact position they occupied in the living eye, and they contend that, if these precautions be taken, transplantation after enucleation can now be performed with as good results as to mobility of the prothesis, cosmetic effect, &c., as when a Mule is done. My experience, drawn from a number of cases of these newer methods, goes to prove the correctness of these statements. The provision made in the recent suggestions of Priestly Smith* for uniting the tendons of the muscles to the conjunctiva, on H. Schmidt's plan, is, it seems to me, also arranged for in both Morton and Barrett's operations. To make the results even more perfect, another departure has been made on the recommendation of Bickerton, Webster, Fox, and others—that after both evisceration and enucleation a metal instead of a glass sphere should be used. The metal recommended is silver. This gets rid of one objection which has been urged against implantation of glass spheres on the ground of the danger from possible breakage. Again, to overcome, as far as possible, the tendency to expulsion of the sphere, Bryant (Omaha) has been using perforated spheres. The perforations allow of new tissue growing in and around the artificial ball, which interlacings most effectively aid in its fixation, and thus by lessening the tendency to its expulsion do away with yet another objection—that of implantation. Bryant uses spheres made of aluminium, on account, I presume, of their lightness, but I scarcely think this metal can be recommended owing to the gradual disintegration it undergoes when exposed to or imbedded in moist surroundings. I have made experiments with aluminium left in water and in normal alkaline solutions, and the same process of disintegration which gradually causes probes made of this metal to triturate away soon reduced them very visibly in shape and bulk, nor can this be prevented, as aluminium cannot be easily plated. I have, therefore, adopted the perforated silver sphere, and it seems to suit admirably. Lactate of silver is thrown out from the sphere; this acts as a useful antiseptic, and I have not, as yet, seen argyriasis of the conjunctiva in any cases where the spheres are worn, though such is said to have been observed. One word as to the size of the spheres. It is well known that if the sphere be too large it is more likely to be expelled, and it is also alleged to be a cause of irritation if it fill the sclerotic too closely in Mule's operation by the pressure it exercises on the optic nerve. The spheres in adult eyes should range from 13 to 14·5 mm. in diameter for Mule's operation, and 1 mm. larger when introduced into Tenon's capsule. These measurements can be reduced for children in proportion to their age. The silver spheres figured in Meyrowitz' catalogue range from 13 to 16·50 mm.; the latter size is, in my opinion, slightly too large. Lastly, sterilised sponges have been used for implantation, but the objections to their use are that they are liable to work out of the cavity in small pieces, and that the stump thus formed becomes very much reduced in size as time goes on.

To conclude this part of the subject, let me say that I feel satisfied that, if the practice of capsule implantation and the wearing of a shell afterwards were more generally adopted, and precautions thus taken to prevent shrinkage of the orbital tissue after enucleation, we should have fewer cases of extreme cicatricial contraction of the sac to deal with, and better cosmetic results when the permanent artificial eye comes to be worn; and I fail to see why we should deprive our patients of the advantages which such implantations give, the more particularly when provision can now at the same time be made for the proper advancement of the ocular muscles, and since no objection can be raised to this procedure—as can in some cases be urged against Mule's operation—on the grounds of danger to the other eye.

This brings me to the next and most recent improvement in dealing with the question of my paper. I refer to the new protheses introduced by Professor Snellen about eighteen months ago. These protheses are now on their trial, and from all accounts their use is likely to meet the requirements of most, if not all, the cases in which transplantation has by its advocates been recommended as a necessary sequel to enucleation or evisceration. Snellen, in his paper discussing this matter, says: "The preservation of the stump in the socket either by evisceration of the scleral cavity or by the insertion of a glass ball is of great advantage for the motility of the artificial eye; the principal advantage, however, is, and this is of special value, that by this means the dead space in which mucus and tears collect is closed by the heretofore employed shell-shaped artificial eye which lies on the stump. It seems to me that in suitable cases this disadvantage may be avoided after simple extirpation in another way. After extirpation cannot the shell-shaped eye be replaced with advantage by one with which no dead space will remain? With this object in view, I have made some experiments by filling the ordinary shell-shaped eye with plaster, later with a gutta-percha preparation, such as is used in dentistry ('Gilbert's temporary stopping'). The latter satisfies all requirements. It becomes soft in warm water, assumes then any desired form, and its surface becomes soft and smooth. This experiment gave immediately the desired success. Although the filled eye was much heavier, it was nevertheless preferred by the patient to the shell-shaped artificial eye, because the sharp edges were now covered, and no more conjunctival secretion collected."*

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* Ophthal. Rev. vol. xviii., p. 121.

Müller Bros. (Wiesbaden) have made artificial eyes, too, on this principle—Snellen's order of three different shapes, two of which (Nos. II. and III.) are double-walled. No. II. has a concave posterior wall, and is suited for being worn over the stump left after evisceration, or after sphere implantation in the sclerotic or Tenon's capsule; and No. III. with a convex posterior wall to be worn after a simple enucleation. It is the No. III. shape which threatens, by so simplifying the after-treatment of simple extirpation of the globe, to do away with the necessity of transplantation. Wolff (Berlin)† has apparently been working on the same lines as Snellen, and having come to similar conclusions recommends protheses similar in shape to Snellen's No. III. He says that the stump, after transplantation following evisceration or enucleation, moves better than the prothesis placed upon it, because of the friction and especially the form of the latter. To obviate these hindrances to a freer motion of the prothesis, Wolff has attached it to the ball formerly introduced into Tenon's capsule, thus producing, according to him, a better position, and, because of the smaller size of the prothesis required, greater mobility. I have been using Snellen's eyes for some time, and they do seem most satisfactory, especially in cases where there is a tendency to irritability of the sac. But whether they are destined to supersede the older shapes, and to take the place of transplantation, my comparatively limited experience of their use does not yet qualify me to say. If they should be found to fulfil all the indications, and have all the advantages claimed for them, the adaptation of artificial eyes will in the future be more easily perfected and more simplified than it has ever been; simple enucleation will then be the rule, and opponents and advocates of implantation will meet on common ground, a compromise having been made which will be equally satisfactory to both parties.

So much so for the expedients which have been found useful to avoid cicatricial contraction. Now as to the operations themselves.

Grafting of pieces of mucous membrane—first done by Stellwag, of Vienna (1873), and Wolfe, of Glasgow (1872)—"does," according to a statement of Stellwag in a later publication,‡ "not in any way answer the requirements we could reasonably expect." I have found these grafts practically useless, and have long since abandoned the method.

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* Ophthal. Record, vol. viii., p. 353.

† Arch. Ophthal. vol. xxviii., page 411.

‡ Allgem. Wien. Med. Zeitsch., 1889.

Of skin-grafting operations, that recommend by Maxwell (Dublin)* was one of the first I performed. It consists in transplanting a piece of skin from the upper or lower lid into the conjunctival sac, leaving the flap attached at each end, and tunnelling a bridge under the lid margins to allow of its introduction into the orbit. I cannot say I have had much success with this operation, and I have not done it for some years. It seems more suited for symblepharon than for other contraction of the conjunctival sac. Ogston's operation†—also a transplantation with a pedicle—I have done; but it, too, has not been found as successful as one could wish, and it has the disadvantage of causing unnecessary disfigurement of the parts Ogston gets his flap from the skin on the outer side of the eye, and, having split it into two horizontal segments, passes these under the canthus, and fixes one in the upper and one in the lower fornix. Harlan's operation‡ is the next. The conception of this operation is good, but the practical results I have found to fall short of that which the originator of the method leads us to expect, and probably the reason of this is that in most cases something more than re-adjustment and stretching of the cicatrised tissue of the sac is required, and that something means the introduction of new tissue into the contracted orbit from without. He makes an artificial sulcus in the fornix by passing a thick lead wire around the bottom of the cavity through the contracting bands, and allows it to remain *in situ* until the walls of the sinus it has formed are completely cicatrised. He then incises the tissue in front of the wire, and inserts a leaden shell, allowing the edges to rest in the groove the wire has made. The operation may succeed in the less severe cases of contraction; but, as I have already said, the principle underlying the procedure seems to be faulty. But transplantation of skin flaps without a pedicle I have found the most satisfactory of all. Hirsch was the first to adopt the method, following the principle of Wolfe's transplantation, and I have had such almost uniform success with this method that now I don't feel disposed to follow any other. The procedure is very simple. An incision is made about 4 mm. from and along the ciliary border of the lid, right down through the subconjunctival tissue; on the raw surface thus made a flap of skin taken, say, from the internal aspect of the arm is implanted, and its outer margin sutured to the lid border. A leaden shell is then inserted, the largest size the sac will admit, so as to thoroughly distend the cavity, and so that its edges will push the flap well down into the newly-formed fornix. This is one of the most important parts of the operation to attend to, for, if the prothesis be not a full-sized one, the new fornix is sure to become eventually too shallow. Sutures are not necessary on the posterior edge of the flap, as the shell is sufficient to keep this portion of the flap in apposition to the raw surface behind. Two holes are made through the convex surface of the shell, sufficiently large to allow of the introduction of the nozzle of a syringe, so that daily washing out of the sac can be performed without removing the shell. This perforation was not a part of the original procedure, but was suggested to me by a patient who was wearing a shell at the time, and I have found it a convenient arrangement, as it prevents unnecessary disturbance of the flap during union, and facilitates the healing process. The shell should not be removed sooner than the tenth day after the operation, but the sutures, which can be taken out without disturbing the shell, may be removed on the third or fourth day. I have done this operation many times—in some very extreme cases, too; and they have been nearly all successful One or two further precautions in carrying out the details of the operation I have found it necessary to take. The flap should be as thin as possible, composed of the skin proper only and freed from all subcutaneous connective tissue, fat, &c.; if this be not attended to, sloughing is more apt to occur. Some operators prefer to remove the flap by Thiersch's method; again, it should be about a quarter larger than the surface it is required to cover to allow for shrinkage; once more the leaden shell must be worn for some considerable time before it is replaced by the artificial eye; many cases fail owing to this precaution not having been observed. If the artificial eye be introduced too soon, the fornix may become too shallow, as an ordinary and comfortable fitting prothesis does not exercise anything like the pressure the leaden shell is capable of doing. This applies as much, if not more so, to Snellen's as to the older variety. Moreover, the shell being of lead it exercises a healthy astringent effect on the parts, and probably prevents discharge and the formation of granulations. The flap after a short time becomes smooth and soft, and differs only from the conjunctiva by its opaque whitish colour; in fact, it seems to take on something of the nature of the surrounding parts; hence the objection sometimes made that cutaneous should not be grafted on to mucous tissue does not seem to have much weight in actual practice. In conclusion, I have found in at least two of my cases a difficulty in keeping the lower fornix deep enough to retain the artificial eye, owing to relaxation in the one and cicatricial contraction outwards and downwards in the other of the lower lid—conditions often present in injury to the globe involving the lids. This is a complication most difficult to deal with. The action of the orbicularis is in these cases so imperfect that the new fornix cannot be kept open. I have inserted Snellen's sutures in one case, and have subsequently done Argyll Robertson's newer operation for ectropion,* but the condition of this eye is still far from satisfactory.

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* Oph. Rev., vol. xii., p. 189. † *B.M. Jour.*, Dec., 1894. ‡ Oph. Record, vol. vi., p. 195.

These are the operative procedures of which I have had personal experience, but even by the last-mentioned method, which is, in my opinion, far and away the best, the operation may have to be done more than once with modifications and additions to suit the requirements of individual cases; and the remedial treatment in most, if not all cases, calls for the exercise of a degree of patience and inventive ingenuity which any operator who has done a few such operations will soon come to recognise as indispensable to the attainment of ultimate success.

SECONDARY CATARACT

By T. K. Hamilton, M.D. Dub., F.R.C.S.I.

The subject of secondary cataract is one of very considerable importance, and one which has of late years more than ever engaged the attention of ophthalmic surgeons. The importance of the subject and the interest surrounding the condition are all the greater because secondary cataract is connected with that operation upon the eye of all others—extraction of the lens—which presents the greatest possibilities, and therefore one in dealing with which the surgeon can most readily improve or damage his reputation as an operator. Again, there is no eye operation which seems to be regarded by surgeons with such different feelings, some speaking of it as one which may be undertaken without any, or at least very little, apprehension, and others speaking of it as an operation so much to be dreaded that it should not be recommended to the patient unless it is urgently demanded, or unless it can be performed under circumstances most favourable to its success. This diversity of opinion is, however, to a certain extent capable of explanation when we come to make a clinical classification of the cases with which we have to deal. These cases may for practical purposes be grouped under two distinct heads:—(1st) The simple or non-inflammatory variety, in which the opacity is thin, more or less transparent, and elastic; and (2nd) the inflammatory, where the opacity is thick, and more or less non-elastic, the product of exuded material from iritis and blood clot. Obviously the treatment of two such conditions, and the results obtainable therefrom, must be very different. Take one instance by way of illustration: Operation in the one can be undertaken as soon as the eye has recovered from the immediate effects of the extraction; while in the other, often, the longer it is postponed the better, unless such a complication as increased tension demands interference.

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* *B.M. Jour.*, July, 1898.

What is the exact nature of the opaque membrane in secondary cataract? Some authorities say that it takes its origin from the epithelium of the anterior capsule which has been left behind during the extraction, and that the opacity is due to proliferation of these elements, while others fail to find any trace of the anterior capsule in its structure, and think it is entirely a new connective-tissue formation.

The principal interest in a purely surgical condition, such as secondary cataract is, necessarily centres in the operation for its removal and the complications, &c., connected therewith. When a case presents itself to us for treatment, we have to consider, first of all, the question of the advisability of operating upon it with a view to the improvement of vision, and each case has thus to be dealt with on its own merits. Here there is room within certain limits for a difference in practice, and we shall find that some will be disposed to leave the eye alone if the patient has anything approaching satisfactory vision, while others—and this group includes the majority of up-to-date operators—will recommend operation when the vision falls at all short of the best that the eye is capable of attaining. In this latter group we find an experienced operator, Kuhnt, who records his conclusions briefly as follows:*

The indications for operation are—

1st. In all cases where the vision is less than ⅓, and where no spontaneous improvement is to be expected.

2nd. Even in those cases where vision is less than ⅓ and while a part only of the pupil is perfectly clear, the rest being cloudy.

3rd. In that class of cases where the subjects are indolent and unintelligent, and where vision, while it may be ⅓, will, we know from experience, grow worse from the development of capsular cataract.

4th. In those cases where the manner of life—that is to say, the occupation—demands a greater visual acuity than ⅓, and where we have reason to hope that the visual acuity will still further be improved by the operation.

Again, surgeons do discission for secondary cataract almost as part of a routine practice—as, for example, Knapp, who performs every extraction with a view to a subsequent discission which he finds a necessary sequel to his peripheral capsulotomy. He thus summarises his practice: Simple extraction, peripheral capsulotomy, and subsequent discission, by following which routine he claims to have fewer failures and more permanently good results.

The preparation of the eye before operation demands as much, if not more, attention than other operations in which the globe is opened, and probably this applies most of all to those cases of secondary cataract resulting from previous inflammatory trouble. Although it is true that antiseptic precautions cannot be relied upon entirely, it being impossible in eye surgery to make the field of operation perfectly aseptic, and though, in addition, non-pathogenic germs—the normal conjunctiva even always contains bacteria—may become harmful under certain favouring conditions, still antiseptic precautions must never be neglected, and the greatest attention should be given to strict cleanliness on the part of the surgeon, and that in every case, even the simplest and least complicated, so as to render the eye as aseptic as it can be made. Many recommend the local antiseptic treatment to be commenced several days previous to the operation, and all agree that the agents used should be non-irritating. Physiological salt solution is as good a cleansing application as any, experimental investigations having proved that it diminishes the quantity of micro-organisms in the conjunctival sac as well as solutions, for example, of sublimate. I have adopted Fryer's method,* and used a 2 per cent. solution of protargol, applying it to the sac after a cleansing lotion. Protargol is said to destroy organisms, and it does not irritate. Careful inspection of the tear passages and treatment of any unhealthy conditions of the adnexa which may exist are also essential. In this connection it is interesting to refer to a now well-known fact that suppuration hardly ever follows on operations in which the lens is left intact. De Wecker thinks that the aqueous saturated with lens matter is an excellent nutritive medium for the cultivation of micro-organisms; and still—and this also is of equal interest in secondary cataract operations—Andogsky has found by experiment on animals that, when the posterior capsule is torn, cocci do not enter the posterior chamber, as they are met by a mass of leucocytes, which migrate from the optic nerve and form a sort of protecting shield for the nerve. Hence possibly the explanation of the rarity of suppuration in the posterior part of the eyeball after discission, and also of the fact that panophthalmitis after extraction is rarely followed by meningitis. Again, the position chosen for entering the knife or other instrument for discission of the capsule in secondary cataract has also an important bearing on the question of infection, and will be referred to later on. Once more, before leaving this part of the subject of the introduction of infection from without, it should be mentioned that it seems extremely probable that when severe reaction does follow the comparatively innocent operation of discission, it is mostly due to conditions such as thickened and adherent capsule connected with the original extraction, in performing which, germs were introduced into the eye, and lying dormant are awakened up to life by the additional traumatism caused by the discission. I have at present a case of this kind under observation. Extraction had been performed some months ago, and from the account given by the patient of the progress afterwards there must have been a severe iritis. Discission was done by me with every possible care and the strictest antiseptic precautions, but, in spite of all, iritis commenced immediately with hypopion on the third day, and now the good effect of the discission has been entirely undone by exudation in the pupil.

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* Zeitschr. fur Augenheilk. Bd. I. Heft 3.

In certain cases it may be necessary to operate for the removal of secondary cataract when the eyeball is still inflamed; then it is desirable that as much as possible of the inflammatory congestion should be removed previous to the discission. To accomplish this, various methods of treatment are recommended. I have found that the application of hot and cold water to the globe is one of the best of these. Sterilised water at a temperature of 115°-120° Faht. is sprayed on the eyeball for some minutes, and immediately afterwards a spray of cold water for about ten seconds or so. This alternate hot and cold application should be made two or three times consecutively, and repeated frequently during the day. The rapid dilatation and contraction of the vessels thereby induced is, I believe, physiologically one of the best means at our disposal of restoring tone to overfull vessels, and thorough cleansing of the parts is at the same time secured. I commend this treatment to my *confrères* with some confidence as an excellent routine practice, and one which, in cases of hyper-vascularity of the eyeball and its appendages, gives the most satisfactory results. Hot water alone, or hot and cold in succession, applied by an irrigator has a large range of utility as a theurapeutical agent in eye diseases; and so impressed am I with this fact that I have been conducting a large series of experiments during the past twelve months, the result of which I hope soon to make public. Galezowski uses steam instead of hot water, and all cases of this kind are submitted to treatment by steam sprays in his clinique. To relieve congestion just immediately before the operation, the instillation of a solution of supra-renal capsule is indicated, and is useful in bringing about a temporary ischæmia of the parts. The best formula for this application is a 10 per cent. solution of the gland in a solution of equal parts of glycerine and distilled water. A solution thus made is most permanent, and does not undergo the putrefactive changes which gland solutions are so liable to do. It has been recommended to add a little carbolic acid to this solution to make it keep. This, however, is now said to be not only unnecessary but also incorrect, as no other substance can be mixed with the extract without impairing its astringent action on the eye. It goes without saying that all instruments should be carefully sterilised before use. Boiling has till lately been the method most usually relied on for this purpose, but as this process, though most effective, is not suitable for cutting instruments on account of the blunting of the edges which it soon causes, the method of sterilisation suggested by De Schweinitz is now likely to supersede it. He uses a vapour of formaldehyde as a sterilising agent, and it is said to be an excellent one. Dr. Borthwick has recently made some experiments in the Bacteriological Institute here with a formaldehyde steriliser, and reports most favourably on its efficiency. He finds that when instruments are first carefully cleansed of all impurity, and then thoroughly exposed to the vapour for a given time, that their sterilisation is complete.

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* Arch. Ophthal., vol. xxvii., p. 448

As an anæsthetic, holocain seems destined to displace its older rival, cocaine, in this and certain other eye operations. The advantages claimed for it over cocaine in discission are: That it is quicker in its action (Hirschberg has found a 1 per cent. solution of holocain render the eyeball quite insensitive in half-a-minute); that it does not alter the tension; that the corneal epithelium is not changed by it in the slightest degree; that its instillation into the eye aids in the absorption of mydriatics; that it renders the iris more insensible (a degree of inflammation may sometimes be present which prevents cocaine being absorbed, and here holocain has the advantage); most of all, that it is antiseptic. Heinze has found it to be in ordinary strengths a powerful germicide. Randolph also has demonstrated its inhibitive influence in bacterial growth, and asserts that it is a protoplasmic poison arresting amœboid movements, putrefaction, and fermentation; and, again, Knapp thinks its germicide properties are due to its non-interference with the circulation. As blood is the best safeguard of infection, its outflow during the operation carried the germs away from the wound; whilst cocaine by its constriction of the vessels and other tissues sucks tissue juice and other germs inwards and thus favours infection; and, finally, that the solution keeps well and does not require boiling to sterilise it. In order, however, to secure the permanency of holocain solutions, they must be carefully prepared, as Hinshelwood has pointed out; they must be made up with freshly distilled water, and all bottles, droppers, etc., must be thoroughly de-alkalinised for half-an-hour before use.

In operations on the capsule it is very necessary to have the membrane especially the wrinkled and more transparent ones, well in view, and this can best be accomplished by operating in a darkened room with the aid of electric light thrown into the eye. I find the incandescent lamp we have for transilluminating the frontal sinus the most convenient to use, as the shield surrounding its burner focalises the light admirably. Landolt uses a pair of spectacles (*lunettes-loupe*) when he is operating, and he has found them a great help. They are composed of two glasses, + 2·50 D combined with No. 6 prisms, cut out of a large lens, and are placed in a frame in the position they originally occupied in the lens. With them you get considerable enlargement of the image without losing the benefit of binocular vision, both of which are very important when you are operating on a delicate semi-transparent membrane. I have found by using a frontal mirror in front of the spectacle frame and a bull's-eye condenser lamp I get the best view of the capsule, and can see not only the details of the pupil area, but also those of the fundus in the aphakic eye. [*Spectacles exhibited.*]

What is the best time to operate for secondary cataract in ordinary uncomplicated cases? Most operators now prefer to operate early after extraction. Da Gama Pinto recommends the beginning of the fourth week if the eye is quiet, and does not even wait for the capsule to become opaque. This time allows of any post-extraction absorption which is likely in most ordinary cases to take place. Knapp also mentions a month after extraction as an average time. Speaking generally on this point, the sooner the membrane is divided after the eye is free from the irritation following the extraction, the easier the manipulation is and the less the danger of complications.

Discission—a name originally given by Beer—is an old operation, but newer methods of operating and different kinds of instruments have gradually displaced older ones. Most operators now use some kind of cutting instead of the non-cutting instruments formerly recommended. Knapp's knife-needle or some modification thereof is the instrument now in most general use; indeed, one must use some such instrument to properly carry out Knapp's own rule, which is: "Do not tear, cut." Arlt says that no instrument is less suited for discission than the older variety of needles. A modified Graefe's knife is used by some, but the Knapp needle-knife possesses advantages over it in many ways—*e.g.*, its cutting blade being shorter and confined to the top of the instrument, you can better secure retention of the aqueous during the operation; and, again, an instrument shaped like Knapp's makes a better fulcrum against the cornea when the point is cutting the membrane than a Graefe does. The character of the operation, however, necessarily depends entirely on the nature of the secondary membrane. If the capsule be fairly thin, a single knife-needle will probably be all that is required to make a sufficient opening in the capsule; if it be at all thick or tough, a second needle will be necessary. My practice is to introduce an ordinary non-cutting needle first, making at the same time counter pressure with my index finger against the globe opposite to the site of its introduction. I push this through the membrane, and then, having introduced through the other side of the cornea a cutting needle, push it also through the membrane, carefully avoiding evacuation of the aqueous during each process. I then make use of the first needle to steady the portion of the capsule to be incised, and cut through with the other. If properly done with a well-sharpened knife, the procedure just detailed prevents all dragging on the capsule. This is the same principle originally recommended by Bowman in his historic bi-manual operation, but with this difference: That in the latter both needles were non-cutting, and the process was therefore a tearing and not a cutting one. The place in the cornea at which the needles should be inserted is of also great importance, and the point of selection now recommended is at the limbus, or, better still, 1 mm. behind it. The advantages claimed for this site are: You can thus divide the membrane with the longest possible sweep of the knife, and you can at the same time most easily divide posterior synechiæ when they exist, having the support of the adherent iris in so doing; you can also in this way most easily prevent the escape of aqueous, and are therefore much less likely to have protrusion of vitreous through the wound; and, lastly, you reduce to a minimum the risk of infection, because a wound of the most vascular part of the cornea closes most promptly; in a word, the use of the needle-knife by these methods combines all the advantages of the older needling operations with none of the disadvantages. In view of the occasional occurrence of glaucoma after discission, Knapp thinks it is wiser to divide the capsule by a ⊤ or X rather than by a + shaped incision; this latter, though giving a clearer pupil, is difficult to obtain, and does not ensure as much gaping of the wound as the former.

Referring again for a moment to the instruments in general use now for dividing the capsule, it may be mentioned that some few operators, of whom Schweigger is one, have abandoned needles of every description, and use scissors introduced through an opening made with a broad needle.

The operation known as "posterior discission," or *discissio per scleram*, is recommended and practised by some, but as strongly condemned by others. Arlt thinks it is an operation quite indefensible on account of the fallacy of working in the dark and the danger of intra-ocular hæmorrhage; while Würdemann designates it "an operation out of date." I have had no experience of it.

When the capsule is very thick and non-elastic—*i.e.*, the result of previous inflammatory mischief—it is a difficult matter often to keep the opening from closing. In these cases a capsulectomy becomes necessary, followed by removal of a portion of the thickened tissue, and here the scissor form of instrument comes in—De Wecker's or some other variety. In certain cases where division of the capsule only is aimed at without any removal of a portion of the same, the capsule scissors introduced by the President of our Section* have distinct advantages which makes this instrument a most useful addition to the ophthalmic armamentarium. To remove the incised portion, the capsule is cut in certain directions with scissors such as De Wecker's, as each case seems to require, and the separated portion subsequently extracted with a forceps or hook. In many cases, the forceps devised by Panas (*pince-capsulaire*)—which I exhibit—will be found a most convenient instrument, while in others the hook will answer the purpose better. In the more extreme cases of closure of the coloboma after extraction, due to the irido-cyclitis, recovery of useful vision is rare. Probably in many of these cases an attempt should be made as soon as possible after the eye gets quiet to make an artificial pupil in the hopes that restoration of the communication between the anterior and posterior chambers may prevent degeneration of the globe. In addition to capsulectomy, a sphincterectomy may sometimes be necessary, especially if the pupil is much drawn out of place. Division of the sphincter iridis and adhesions connected therewith may then be advantageous. I have more than once adopted this practice.

Of the complications connected with discission operations, one of the most disagreeable is entanglement of the vitreous in the corneal wound. The capsule itself does not, as a rule, become adherent to the cornea, but what is often taken for capsule is really a thread of vitreous. Its presence in the wound gives rise at times to considerable irritation, and may be a source of infection. It should be cut off level with the cornea, so as to let the corneal wound close over as soon as possible.

I have already referred to glaucoma as occurring occasionally after discission. Graefe first noted this complication many years ago, and it seems difficult to assign a cause for its occurrence. Knapp thinks that the character of the capsule and the manner in which it is attacked are to be looked upon as the main factors in causing its development. When the capsule is tough, dragging on the ciliary processes we know is very likely to take place during the cutting operation, and it is precisely in these cases that glaucoma sets in. He has also seen the greatest reaction when he endeavoured to cut dense cords or membranes in the region of the extraction scar—the most vulnerable region, he says, of the aphakic eye—and a region to be carefully avoided in secondary operations. Pagenstecher attributes glaucoma, after discission, to corneal traumatism; but Da Gama Pinto points out that it also follows posterior discission in which the cornea had not been touched; therefore this cannot be the cause. For treatment of the glaucoma, Knapp recommends iridectomy, which, obviously, can only apply to cases following simple extraction. Sclerotomy, according to Pagenstecher, offers the best prospect of permanent reduction of tension; while Bowman was in the habit of adopting a very simple expedient, which often answered well. He performed a paracentesis of the cornea, and at the same time passed the broad needle on through the iris into the vitreous. It is in operating on such conditions as are presented in cases of congestive glaucoma that solution of the extract of the supra-renal capsule is so useful; its vessel-constricting action is then particularly valuable, for, after its instillation, cocaine, which previously was prevented from acting, is able to produce anæsthesia. Sympathetic ophthalmia has been known to follow the discission of a thickened membrane after iridocyclitis. De Schweinitz records a case.

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* Oph. Review, vol. xiii., p. 38.

In conclusion, let me very briefly refer to the interesting question of accommodation in the aphakic eye. In certain cases of secondary cataract, when a pin-hole opening is made in the capsule, the eye seems to have greater range of accommodation than we are wont to expect after extraction, and this occurrence seems at first sight to give colour to the contention of some authorities, as Förster and others, that the aphakic eye can accommodate. I have had a well-marked instance of this lately in a boy for whom I had removed both lenses. He uses only one pair of glasses for all purposes. Such high authorities, however, as Donders and Helmholtz, assert that the lenseless eye has no trace of accommodation at all, and that, in those cases of exceptional ability to read with the distance glasses, what appears to be accommodative effort is due to an unusual faculty in certain eyes of overcoming dispersion circles, and here it is that the limitation of the pupillary area above referred to comes in as a help to the patient. Rogman* (Ghent) and Salzmann† (Vienna) have recently contributed articles on this so-called "pseudo-accommodation," and the moral drawn from its existence by the former writer is the importance of maintaining in any operation upon the lens as small and as active a pupil as possible.

TWO CASES OF SUB-HYALOID HÆMORRHAGE.

By J. Lockhart Gibson, M.D. Edin., M.R.C.S. Eng.

I have seen only two cases of sub-hyaloid hæmorrhage, and their ætiology seems to me so obscure that the introduction of the condition as a subject for discussion may be not without interest, and possibly tend to increase our knowledge. One occurred in a young man aged nineteen years, the other in a young married woman. The cause in each seemed to be venous stasis, and as causes of this last there appeared to be little but sedentary habits, sluggish digestive apparatus, and constipation.

S.F., aged nineteen years, a well-grown and fairly healthy-looking young man, engaged in a shop, which necessitated an indoor life, standing for a greater part of the day, and some night-work. Unaccustomed to take out-door exercise. 4th August, 1897: Considers himself in usual health, except that he has been suffering from indigestion and constipation, and that his breath has been offensive. For the past six days the light has seemed to him red and misty with his left eye, and his sight with it has been defective. The lids of both eyes have been slightly inflamed and somewhat adherent in the mornings. No headache. No asthenopia.

Vision: L.E. $\frac{6}{60}$; R.E. $\frac{6}{6}$.

Examination.—Slight marginal blepharitis and slight conjunctivitis. Cornea of left eye may be a trifle less clear than that of right. No vitreal opacities. Disc may be a trifle hazy, but there is no exudation into it, and no optic neuritis. The upper and lower retinal veins are somewhat distended without being tortuous. Macular region is represented by a glistening white spot, the size of a pin-point, apparently in the position of the fovea centralis, and forming the centre of a dark-red pear-shaped area apparently as large as a mediumly dilated pupil (to direct examination). The white glistening spot is in the centre of the thicker part of the pear, and the thinner end of the pear points upwards and slightly inwards. The arteries in the fundus are not smaller than natural. There is an absolute central scotoma for red, and a central area in which there is a partial scotoma for white, and at the edges of which perception is more distinct than elsewhere in the visual field. Everything seen with that eye has a red appearance, more especially light.

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* Annales D'Oculisque, Oct. 1899.

† Graefe's Archiv. Ophthal., xlix., i., 1899.

Diagnosis.—Sub-hyaloid hæmorrhage at the macula, though the possibility of some of the hæmorrhage being into the retina had to be borne in mind, and made the prognosis more guarded. There was no albumin; no history of a blow; no cardiac bruit, though a somewhat accentuated second sound over the praecordia.

Treatment.—Rest in bed for a week; calomel purges; three subcutaneous injections of pilocarpin to the production each time of free diaphoresis; iodide of potash by the mouth, and later a mixture of nitrohydrochloric acid and nux vomica. The blood extravasation underwent the usual changes, the central bright spot became more irregular and granular-looking. Things appeared to him red until 1st October, practically for two months, and on this date there was still a little colour in the extravasation. The patch itself had become triangular in shape, and much less noticeable. Vision had slowly improved to $\frac{6}{18}$ and Jg. i. at 12 inches. On 3rd November, vision $\frac{6}{12}$ and one letter of $\frac{6}{9}$. March, 1898: Vision, three letters of $\frac{6}{9}$, but perception most acute immediately on each side of point of fixation. If asked to look straight at one letter of $\frac{6}{12}$, he saw the letter on either side of it more plainly than the central one. When last seen in June, 1898, vision was four letters of $\frac{6}{9}$, and the macular region merely looked a trifle blurred. I have heard that he now sees as well as with his other eye, but have myself no evidence of this.

Mrs. S., aged twenty-eight years. Married for four months. Consulted me on 30th December, 1892, complaining of seeing badly with her right eye. The loss of sight had come on suddenly while sitting at dinner three weeks before. Two months previously she had suffered from styes in that eye, but saw quite well with it after the lid trouble disappeared. Menstruated three weeks ago. Complains of pains across the temple.

Vision: R.E. $\frac{6}{60}$; L.E. $\frac{6}{6}$.

R.E.: Central scotoma nearly absolute.

Examination of R. Fundus.—Marked optic neuritis; exudation into and around the disc. Veins dilated and very tortuous. The macular region is occupied by a dark-red spot appearing, to direct examination, to be 2 to 3 mm. in diameter and having a very small white spot in its centre. No hæmorrhage elsewhere in the fundus. Examination of heart gave only a blowing systolic murmur in the tricuspid area. No heart symptoms.

Diagnosis.—Venous thrombosis, and resulting sub-hyaloid hæmorrhage. Treated by a blister to the temple, and by iodide of potash and sulphate of magnesia to keep the bowels open. Improvement occurred much more quickly than in the case of the young man, although the fundus appearances were much more alarming. By the middle of January vision was $\frac{6}{18}$ and Jg. 4, much less exudation into and around the disc, and veins much less tortuous and less dilated. Hæmorrhage at macula still dark-red with a white centre. A month later vision had improved to $\frac{6}{9}$ and Jg. 1. A blurring remained at the macula, though the hæmorrhage had disappeared. Disc still somewhat swollen, and margins not quite defined. I did not see her after this, but heard three years later that the eye was as good as the other.

REFLEX NASAL NEUROSES.

By G. Heuzé Hogg, M.D.,
Launceston, Tasmania.

Medical writers from the time of the great Father of Medicine himself have noticed and alluded to the existence of certain phenomena reflexly arising from the nose. Thus, Hippocrates refers to the reflex connection between the nose and diaphragm; Aretæus and Pliny to the onset of epileptic fits as the result of irritation of the nasal mucous membrane; Rhazes to the existence of what we may call a "violent catarrh"; Fernelius to the dependence of some forms of hemicrania on nasal trouble; while in our own time Voltilini, Hack, Beard, Daly, and Noland Mackenzie have dealt with the question in a more scientific manner, the numerous papers of the latter authority being especially interesting and worthy of notice. Notwithstanding that such conditions have been so long recognised, and of late years so thoroughly investigated on this subject among many of the profession, either scepticism or ignorance reigns supreme. Indeed, I might myself have been fairly classed amongst the sceptics until repeated cases coming under my personal observation have compelled me to become a believer.

Of course, like much else in medicine, these reflex nasal neuroses have been overdone, but the absurdities of faddists should not drive us to the other extreme or make us shut our eyes to actual facts.

I do not, therefore, desire to appear as an apologist for those who would seek an intra-nasal cause for the thousand-and-one ills that flesh is heir to, and who, desiring to imitate their compeers in gynæcology, would gravely affirm "*Le nez c'est l'homme*"; my only wish is to draw attention to the existence of such reflex troubles, and to point out that in the nose may occasionally be found the exciting cause of symptoms which may have baffled the physician.

The following are brief notices of a few of the most illustrative cases which I have met with:—

Case I.—Male, aged fifteen years, suffered from asthma for last five or six years, necessitating his removal to a school in a distant town, where he was comparatively free. The nose showed a hypertrophic rhinitis; treatment by cauterisation with chromic acid relieved the condition so that the boy remained free from attacks, and was able to reside at home.

Case II.—Male, aged twenty-five years, suffered from severe asthmatical attacks followed by trying cough—the attacks coming on several times a week, and robbing him of his sleep, so that he only obtained three or four nights' sleep in a week. He had in consequence lost weight, and his general health had become much impaired. Many remedies had been tried without success. Examination of the nose revealed the presence of an advanced atrophic rhinitis, treatment of which by a cleansing douche, and a thymol and albolene spray, caused a cessation of the attacks, so that the patient put on weight and gained strength rapidly. This is, so far, the only case that I have met with where atrophic rhinitis has been associated with asthma, and the condition must be rare, for I find no allusion to it in the various authorities consulted. No doubt the crust formations brought on the attacks by reflex irritation.

Case III.—Female, aged twenty-five years, suffered from chronic cough, which was worse at nights, and frequently interfered with sleep. As there was a history of phthisis in her family, also one of hæmoptysis in this patient, some anxiety had prevailed about the case, and treatment had been directed towards the lungs. Examination, however, revealed that they were free from disease, but that there was a marked hypertrophy of the inferior and middle turbinate bodies; irritation of the middle turbinate with a probe caused an attack of coughing, and further inquiry showed that the patient was subject to attacks of hay fever each summer. The hypertrophy was reduced by the application of the galvano-cautery with the result that the cough disappeared.

CASE IV.—Female, aged twenty-seven years, who complained of chronic cough, and had been treated by various sedative cough mixtures without effect. The cough varied in degree, sometimes almost disappearing. It was said to become much worse at the menstrual period, when breathing through the nose also became difficult as if the nose were stopped up. On examination the lungs were found to be healthy, as was the throat apart from some chronic pharyngitis; a marked hypertrophic rhinitis existed, and when the nose was inspected two weeks later, at the menstrual period, the inferior turbinates were so swollen as to almost block the nasal passages. The hypertrophic condition was treated later by the galvano-cautery with the result that the cough disappeared.

CASE V.—Male, aged seven years, suffered from attacks of croup. Removal of "adenoids" from the nasopharynx was followed by the disappearance of the trouble.

CASE VI.—Female, aged twenty-seven years, who had been subject to nasopharyngeal catarrh, had suffered for some years from frequent and severe headaches and from "eye strain." Correcting cylindrical lenses had been worn for a couple of years, but afforded only partial relief to the symptoms. Examination of the nose revealed the existence of some old "adenoids" and of a hypertrophic rhinitis. Removal of the "adenoids" and the application of the galvano-cautery to the nose caused great improvement, the headaches and asthenopic symptoms disappearing.

CASE VII.—Female, aged twenty-five years, suffered from severe headaches, especially at the menstrual periods. Cauterisation of the middle and inferior turbinates, which were hypertrophied, greatly relieved the attacks, which ultimately, under general treatment, ceased to give trouble.

CASE VIII.—Male, aged thirty years, complained of tinnitus aurium. He had been subject the last few years to attacks of hay fever, when not only had the usual nose and eye symptoms been present, but there had been noises in the ears, and great irritation of the external meatus. Hearing was almost normal, as were the membranæ tympani. He had had a slight return of the hay fever, and was fearing severer attacks. The nasal mucous membrane, which was hyperæsthetic and somewhat congested, was touched with the cautery. The tinnitus soon disappeared, and for that season the hay fever absented itself.

CASE IX.—Male, aged thirty years, complained of deafness, noises in the ear, and hay asthma. He had been subject to the attacks of hay asthma for many years, when, in addition to the nasal symptoms, there had been great irritation of the external auditory meatus, coughing, deafness, and tinnitus. The deafness had increased with each attack, and is now extreme. The membranæ tympani were greatly thickened and their mobility impaired, while in the nose there was hypertrophic rhinitis with great hyperæsthesia, so that, even under the influence of a 20 per cent. solution of cocaine when the galvano-cautery was applied, severe paroxysms of coughing occurred, although no sensation, painful or tactile, was experienced. Under treatment the hay asthma disappeared entirely for that season, but the changes in the middle ear, of course, rendered the hope of much improvement there but slight.

Reflex nasal neuroses develop most frequently in connection with the respiratory system, and less frequently in connection with the eye and ear—sometimes, it is believed, in connection with the heart, stomach, and sexual apparatus. In addition, they occasionally also play a part in the development of general nervous symptoms.

The neuroses developed in connection with the respiratory system are the most important, including as they do hay fever, asthma, laryngismus stridulus, and croup. and it is incumbent on the physician in such cases to remember the possibility of some nasal trouble. I have been much impressed in the last few years by the number of cases of chronic cough, which, having baffled the therapeutics of myself and others, have yielded to the treatment of some intra-nasal cause.

The following table shows the conditions that are most frequently met with associated with such neuroses :—

Neuroses.	Exciting Nasal Causes.
Hay fever	Hyperæsthetic rhinitis, with or without hypertrophy.
Asthma	Polypi. Adenoids. Hypertrophic rhinitis, spurs. Atrophic rhinitis.
Cough	Hypertrophic rhinitis. Adenoids. Polypi. Atrophic rhinitis.
Laryngismus stridulus Croup	Adenoids.

Eye.—In connection with the eye, associated with pathological nasal conditions, have been noticed : Blepharitis, conjunctivitis, phlyctenular keratitis. In these, however, the association does not depend on reflex action so much as on the extension of the disease along the mucous membrane.

More essentially reflex in character are those cases of asthenopia, glaucoma, blepharospasm, accommodation pareses, and concentric narrowing of the field of vision, which have been noticed by various surgeons. Hack even went so far as to believe that scotomata and organic changes in the disc might develop as the result of such reflex trouble.

Although I have seen glaucoma associated with nasal trouble on the same side, I have not been able to satisfy myself that it was more than a coincidence ; but Lennox Browne reports an undoubted case of glaucoma cured by the removal of nasal polypi.

I have met with several cases of asthenopia in cases of errors of refraction, where the correcting glasses have failed to relieve the symptoms until nasal troubles were attended to, but so far I have not seen a case of asthenopia cured by nasal treatment alone.

Two cases are on record of the exophthalmos of Graves' disease cured unilaterally by the cauterisation of the nasal mucous membrane.

Ear.—Most ear troubles associated with nasal disease can be traced to the direct connection existing between the nasopharynx, Eustachian tube, and tympanum, but in all probability, in many of these, reflex irritation plays a certain part, by means of vasomotor influence. In this relation the following experimental data are interesting:—First, of division of the cervical sympathetic, causing congestion of the auricle ; second, of division of the trigeminus, causing inflammatory changes in the tympanum. Tinnitus and earache have been said to result reflexly from the nose without any inflammatory condition of the ear being present.

Noland Mackenzie has described earache, itching of and secretion from the external auditory meatus, as occurring rarely in hay fever; and I have noticed similar phenomena in a few cases of this disease.

General Nervous Symptoms.—Headache and neuralgia have frequently been noticed to be associated with and dependent on nasal troubles, the commonest intra-nasal cause of these being, according to White, a hypertrophy of the middle turbinate body. The aprosoxia or mental debility, first described by Guye as occurring in adenoids of the nasopharynx, is too well known to need more than mention, although its exact pathology is as yet uncertain. There is occasionally also to be noticed in children suffering from these growths a great restlessness and irritability of temper, which disappear after removal.

Epilepsy and chorea have been noticed in cases of nasal disease, and undoubted cures have resulted from the treatment of the nasal condition.

Lennox Browne mentions a case of mania in which removal of a nasal polypus was followed by direct relief of the mental disorder, and some cases of melancholia and hysteria have been similarly attributed to nasal causes.

Heart.—Dyspnœa, palpitation, and præcordial discomfort have been said to have occurred as the result of nasal disease, and to have been relieved by the suitable treatment thereof.

Stomach.—Indigestion, flatulence, vomiting, and gastric pain have likewise been recorded as reflex nasal neuroses.

Sexual Organs.—The observations of Noland Mackenzie have done most to elucidate the association between certain physiological and pathological conditions of the nose and genital apparatus; Fliess, Joal, Arviset, Isch-Wall, and others, have also dealt with the subject.

Fliess records cases of painful, profuse, and irregular menstruation dependent on intra-nasal causes; in fact, he has gone so far as to map out a genital zone in the nose—viz., the inferior turbinate body and the tubercle of the septum.

Mackenzie has noticed severe coryza and asthma associated with excessive sexual indulgence, and attacks of hay fever appearing only at the menstrual period.

I have noticed in several cases of hay fever that the attacks either developed or became exaggerated during menstruation. Similarly, nasal cough has become much worse then, in two cases; and one cannot fail to be impressed by the swollen and engorged condition of the erectile tissue of the nose during such periods; cases of ozœna, too, very frequently become worse at these times.

To quote in a condensed form Mackenzie's conclusions, the intimate relationship between the two systems is shown by the following physiological and pathological facts:—

(1) In a certain proportion of women whose nasal organs are healthy, engorgement of the nasal cavernous tissue occurs during menstruation, the swelling subsiding with the cessation of the catamenia; and in some cases of irregular menstruation in which the menstrual period is omitted, at such times the cavernous tissue becomes swollen and turgid. The periodical erection may be inconsiderable. and may give rise to little or no inconvenience, or, on the other hand, the swollen bodies may occlude the nostrils and awaken phenomena of a reflex nature, such as sneezing, coughing, &c. In some cases there seems to be a direct relationship between the periodical engorgement of the nasal erectile tissue and the phenomena referable to the head that often accompany the menstrual function.

(2) The presence of vicarious nasal menstruation which may be accompanied by other phenomena referable to the nose, such as sneezing, &c. Hæmorrhage from the nose may occur also as the vicarious representative of menstruation during pregnancy, as the herald of the menopause, or after removal of the uterus or its appendages, and may make its appearance in boys at or near the age of puberty.

(3) Occasional dependence of phenomena referable to the nose during sexual excitement—bleeding from the nose, sneezing, &c.

(4) The possibility of irritation and congestion of the nasal membrane preceding or exciting the olfactory impressions that form the connecting link between the sense of smell and erethism of the reproductive organs exhibited in the lower animals, and in some cases in man himself.

(5) In some women suffering from nasal affections, the disease is aggravated during menstruation, or may only become noticeable at such times, and the co-existence of uterine disease sometimes may have a great influence on nasal diseases.

(6) Excessive venery and masturbation are said to irritate or aggravate nasal complaints.

When we come to consider the pathology of the nasal neuroses, we must remember that the erectile tissue which is found in the turbinate bodies (the so-called "corpora cavernosa") is particularly a "tissue of sympathy" with the other erectile tissues of the body, and one playing a ready part in the evolution of reflex action. From its physiological character, demanding a free nerve connection, it is by the fifth nerve brought into relationship, not only with the more direct connections of that nerve, but also indirectly with the sympathetic and vagus. Hence the power of its evolving various reflex phenomena.

Given, therefore, the erectile tissue of the nose, a proper nerve apparatus, an irritant in the shape of some pathological intra-nasal condition—is this sufficient to evolve such reflex phenomena? One is compelled to answer in the negative. Our clinical observation tells us that another factor is wanting; and that is, an unstable condition of the nervous system.

How often does one find pathological nasal conditions existing in the otherwise healthy, without causing the slightest reflex or other trouble, while, if one examines the clinical histories of his nasal neuroses cases, one will find that nearly all, if not all, show some trace of a nervous system below par.

My own experience has been that a large proportion of such cases have been in women, and that not only have they physically been below the "line of health," but they have had a neurotic history.

And so, in the treatment of these cases, it is necessary not only to attack the disease, but above all to treat the patient, to improve the general condition, to add tone to the nervous system, so that it may be strong enough to disregard these "pin pricks," which, without that beneficial inhibitory influence, might be sufficient to bring about explosive reflex phenomena.

NOTES ON CASES OF PREVENTABLE BLINDNESS.

By C. E. Barnard, M.D. Aberd., Hobart.

By using the term "preventable," I am wishing to call general attention to the many cases of blindness that come to a specialist too late to be of any service; whereas if these cases had been seen earlier, and had received judicious treatment, there is every reason to believe they would have recovered. No doubt this is the experience of all ophthalmic surgeons. If so, so much the worse, as it would show what a large amount of preventable blindness there must be in the colonies. We all lament seeing these cases, but are powerless to prevent their occurrence. And yet it may be possible, by calling attention to this state of affairs, to induce people to take better care of their eyes, and in this way to lessen the number of cases of blindness; for the patients themselves are very often woefully to blame in the matter—either through neglecting simple precautions or from ignorance of the best way to act, or probably they failed to see the importance of seeking skilled advice until greater or less blindness has overtaken them. In this latter case, should benefit or relief not be immediately received, even under the best advice, surprise and bitter disappointment are keenly felt.

At any rate, taking my own experience as a guide, I think I shall show that there is a lamentable amount of blindness among the people which might have been prevented. I am not referring solely to the more complicated diseases of the eye, such as the varieties of glaucoma, but to the more simple complaints which are so frequently met with; and which, if treated judiciously at the onset, would most likely recover. Such complaints as infantile ophthalmia, ulcers of cornea, keratitis, iritis, &c., are easily curable in the earliest stages, but if allowed to run on become most destructive. I shall now relate short notes of several cases that have come before me in order to prove my position. I need not enter into many details; nor need I go into particulars as regards treatment, &c.. as all this would be superfluous to the object of my paper.

Case 1.—An infant, six weeks old, is brought to me with cornea opaque and sloughing—evidently the result of purulent ophthalmia—too late to save vision. The mother had been trying a lotion, but had not been told of the dangerous nature of the complaint, and so had not used the lotion diligently enough to avert complete blindness. As we all know, clear sight can only be saved in cases of purulent conjunctivitis by prompt and energetic measures being taken at the onset.

Case 2.—An elderly man comes with blindness in one eye (left). A whitish mass is seen blocking up the pupil. He had been told that it was a case of cataract. It was plainly evident that it was not cataract, but a case of plastic iritis, where the edges of the iris had become adherent to the anterior capsule of the lens by means of the whitish mass, thus blocking up the aperture of the pupil and producing total blindness. Early treatment in this case would have very probably prevented the occlusion of the pupil, and have saved a certain amount of useful vision.

Case 3.—This is an instance where treatment was successful in preventing blindness ensuing in one eye (R.) after total blindness had been brought about in the other (L.) by neglect or improper treatment. This was a case of iritis in both eyes, but occurring in left one first. This left eye became quite blind, and it was only when the right eye began to be similarly affected that she came to me. Even then adhesions of the iris and the capsule of the lens had already taken place, showing that there was no time to lose. The disease was arrested in time to save useful vision in this eye. It is probable that proper treatment would have saved vision in the left eye, but apparently its dangerous nature was not perceived until too late.

Case 4.—Was a case of simple conjunctivitis in the first instance, but which was allowed to run on without being checked until the cornea had become involved. Blindness from opacities of the cornea was only averted by prompt and careful treatment, but not before some amount of damage to the eye had been done.

Case 5.—This is another case of damage to the cornea of one eye (R.), caused by hot particles being blown against it from a raging bush fire. The patient came to me when suppuration had commenced in tissues of the cornea, so much so that perforation from sloughing seemed imminent. The damage done to the cornea was evidently considerable, and the case looked hopeless. As a matter of interest I should like to mention how the sloughing process was stayed, and the cornea was saved, as the result surprised me. I applied the hot cautery to the spot that seemed on the verge of perforating. There was an improvement by the next day, and this continued until the cornea ultimately recovered. It cleared up so well that scarcely a nebula is perceptible; but, nevertheless, vision is impaired in this eye.

CASE 6.—This is a case of total blindness coming on suddenly in one eye. Upon examination I found cupping of disc, and other signs of glaucoma. Although blindness in this case came on in one day, according to what I could learn, yet there was a history of previous attacks of pain, and some amount of loss of vision in this one eye. At any rate, there were warnings enough to show that this eye was slowly deteriorating. An early iridectomy might probably have saved total loss of sight. I admit there is often a difficulty in persuading patients, as in this case, to undergo a severe operation in the early stages of glaucoma, with the purpose of averting impending blindness, which it is difficult for them to realise or even to imagine as threatening them at this time. It is only when the danger is obvious that they readily submit, and then it may be perhaps too late to save vision.

CASE 7.—This is an instance of late performance of iridectomy with an unsatisfactory result. The patient had a severe illness at the time when one eye (R.) was suffering from glaucoma. Nothing was done to the eye during the illness, which lasted two months or more. When examined then, vision was lost, and ultimately the eye had to be removed, as the other eye was beginning to show signs of trouble. This eye improved at once in consequence.

This list could easily be added to, but I think I have related enough to show that blindness in many instances might have been averted had skilled advice early in the course of their complaints been available. And then under the term "preventable" blindness, I might mention those cases where workmen frequently receive injury to their unprotected eyes when working at their trades. Many a patient has his eyes injured by flying chips striking them, but the blow might have been rendered harmless by the use of some kind of protection to the eye—such as fine gauze, or even plain glass spectacles. This would be a simple precaution; but it is too seldom used—men choosing to run the risk of injury rather than wear articles over their eyes that might bring on them some amount of ridicule. At least, this is a reason often given me; but, perhaps, it is because protectors are in the way of their business, preventing them seeing clearly. However, it is sad to see a workman lose his sight, or even partially so, for the want of some simple precaution.

THE OCULAR SYMPTOMS AND SIGNS OF LOCOMOTOR ATAXIA.

By H. W. MARTINDALE KENDALL, M.R.C.S. ENG.,
Wellington, New Zealand.

I propose giving the notes of a case of ocular disturbance in tabes lately under my care, and finally making a few remarks on the subject.

Mrs. V., at fifty-nine, consulted me in May of this year, on account of her failing vision.

She states that she has been more or less continuously treated for locomotor ataxy for fifteen years past, and dates her trouble to an involuntary and prolonged immersion in cold water, after which she was ill in bed for some months with nervous symptoms. She was married early in life, and had one miscarriage, and one child which died in infancy. Her husband died at thirty-eight years of age, of paralysis. The vision of her right eye began to fail about seven years ago, and she noticed about this time that the corresponding upper lid seemed to droop. She was able to work—*i.e.*, sew in colours, and read (presumably latterly anyhow with the left eye)—until about eighteen months ago, when her glasses had to be greatly increased in strength.

For the past fortnight her vision has been more rapidly failing, and everything now looks foggy and indistinct. At an early period, she states, her vision was much confused by reddish-coloured specks, which gradually disappeared.

She has never seen double, and lately her general symptoms and pains have improved. Her present general condition is one of pronounced ataxia.

Ocular Signs.

Right Eye.—External appearances. Slight apparent ptosis and divergence. Slight epiphora. Here it may be noted that there are no signs of facial paralysis.

Left Eye.—External appearances. Nothing abnormal noticeable.

Both Eyes.—Lateral associated movements equal and good. Nothing abnormal noticed about upward or downward movements of globes.

There is insufficiency of right on convergence.

Tension of both eyes about normal.

Pupils are semi-dilated, the right being the smaller. Their respective measurements are 4 and 5 mm. Both are fixed and fairly circular, and neither act to light or on convergence and accommodation. They act well to atropine, and fairly so to eserine.

Vision.

R.V.—Counts fingers at 12 inches. J 20. Central vision only for white. Colour vision absent centrally and peripherally.

L.V.—Counts fingers at 24 inches. J 18. Field of vision for white (10 mm. disc) is considerably contracted as per perimetric chart attached. Colour vision: Red and green are called yellow. Blue and yellow are at once recognised, and have a field about as large as white.

There is a slight perceptible hippus of right pupil.

Ophthalmoscope.

Right disc small and glistening white without trace of cribriform tissue. The vessels are narrowed, especially the arteries, which are very tortuous at their exit from and near to the disc. No white lines accompany them. Macular area is normal.

Left disc—White, with well shown cribriform tissue—vessels tortuous, but larger than right. Macular area normal.

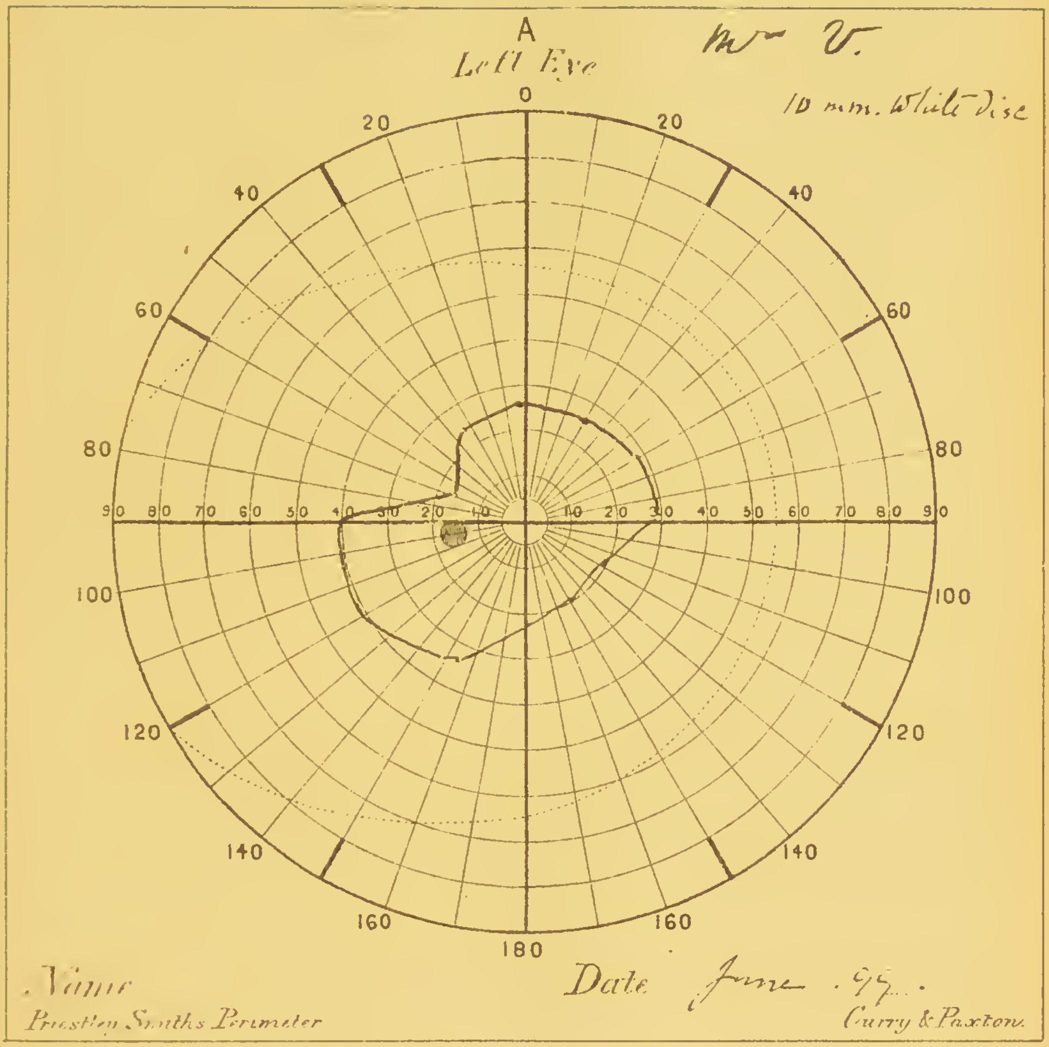
Urine normal.

There is slight deafness on both sides, but no signs of distinct implication of auditory nerve or labyrinth. Nor has she any anosmia, gustatory, or laryngeal symptoms, nor any sensory disturbances, ocular or otherwise, of fifth nerve.

Treatment.

During her illness she has apparently had all varieties, medicinal and otherwise. I put her on hypodermic injections, strychnine, galvanism, nourishing diet, warm clothing, &c., and ordered dark glasses.

The treatment she seems to have appreciated most is that of a mental healer, who, by laying on of fingers, has, she says, just before she came to see me about her sight, got rid of her pains, and enabled her to feel the ground with her feet. This is a curious coincidence, for it is a recognised fact that in tabes an increase in failure of vision may be accompanied by the relief of general symptoms.



To illustrate Dr. Kendall's paper on Ocular Symptoms in Locomotor Ataxia.

Remarks.

There are some interesting features in this case :—

1. The condition of the pupils, which in the right eye, taken with apparent ptosis and appearance of divergence, might easily be taken for an oculo-motor paralysis. The movements of the globe, however, are against this, and the insufficiency of convergence is due to this eye being amblyopic.

The mydriasis caused by atropine would tend to negative paresis of the sympathetic. The question of the paresis was contemplated from the drooping of the right upper lid; but sympathetic ptosis is usually binocular; and, moreover, myosis might be expected to be present. The mydriasis of atropine can only result from paralysis of the pupillary branches of the third nerve with excitation of those of the sympathetic.

3. The condition of the pupils as regards action to light and accommodation. In this case the true Argyll-Robertson pupil is not present.

4. The size of the pupils, an interesting point in tabes.

5. The disappearance of bodily pains, &c., on increase of ocular symptoms; rare, however, in a case so advanced.

6. The occurrence of the disease in females, it being much more common in males.

7. The history of spots interfering with vision. These subjective spots are scotomata, and indicate insular points of atrophy in the optic nerve, and are to be diagnosed from muscæ or opacities.

8. The ophthalmoscopic appearances. The right disc with the condition of vessels more resembles secondary atrophy, with the exception of the absence of white perivascular lines. The left disc is more like that of a primary atrophy, although the vessels here are smaller than one would expect.

Gowers says that occasional attacks of congestion may occur during the course of the atrophy, tending to the development in the disc of a gelatinous material which tends to compress the vessels. This would doubtless account for the condition of the vessels in Mrs. V.'s case.

9. To distinguish a case of this sort from peripheral neuritis, it may be noted that in the latter disease primary optic atrophy never occurs.

10. The interesting points in colour vision. In this connection may be suggested the diagnostic points between optic atrophy and other ocular troubles, rapid diminution of colour perception being pre-eminently associated with the progressive lesions of the optic nerve that lead to blindness.

11. The history of the husband's illness and the patient's pregnancies as against or added to that of her sudden immersion in cold water.

GENERAL REMARKS.

Optic atrophy, which occurs in from 10 per cent. to 20 per cent. of tabetic cases, may commence at any stage of the disease. Usually seen in the pre-ataxic stage, it may occasionally precede by from two to twenty years all spinal symptoms, and as a rule it progresses to total blindness, but may be arrested or even improve.

The failure or increase of failure in vision is usually gradual, but may come on fairly quickly, as in Mrs. V.'s case. This is probably due to an inflammatory exacerbation in the interstitial process.

The spinal symptoms may, moreover, if the atrophy occurs in the pre-ataxic stage, decrease, become stationary, or even be averted. And we have seen how amelioration of spinal symptoms may take place on increase of the visual defect (atrophy). Though it seems usual for the optic nerves to become affected simultaneously, it is not the rule. The atrophy may be considerably more advanced in one eye, and one optic nerve may suffer alone. It may here be mentioned that the flashes of light often experienced in this disease appear to correspond to the lightning pains felt elsewhere. The pallor of the discs is due to wasting of their capillaries, and is in proportion to their previous vascularity.

The vessels, which are not much narrowed, show by their course that the shrinkage of the disc is a recession from its edge, and compression of the vessels may result from the deposit of a gelatinous material on the disc, due to temporary congestions.

Decreasing vision may escape the patient's notice for some time if the central acuity remains good, notwithstanding the fact that a large amount of peripheral contraction may be actually present.

Central vision may fail first, though usually there is a previous diminution of the peripheral field and loss of colour vision.

Atrophic progression is probable when a more or less concentric restriction of the field of vision is taking place, added to a colour defect.

Optic atrophy in tabes is a peripheral degeneration of the optic nerve, and may be compared to the degeneration of the peripheral spinal nerves in this disease, notwithstanding the absence of retinal degeneration.

Primarily there is a wasting of the nerve fibres and elements, followed by a great increase in the interstitial connective tissue, forming thick tracts of gelatinous aspect.

The ocular paralyses of ataxia are present in about 30 per cent. of cases, and may be divided into transient or temporary and permanent paralyses. They may have a peripheral or central origin. A history of diplopia would be obtained.

The transient palsies, the ext. rectus being the muscle most often affected, generally occur in the earlier stages of the disease, and are apparently functional as distinguished from the permanent or lasting palsies, which occur more commonly in the later stages. In these permanent palsies the whole oculomotor nerve may be concerned, or perhaps only one muscle may be implicated, as the levator, causing ptosis.

We would suspect a central origin where there is a combined palsy, as in loss of accommodation and convergence, though the internal recti act in movement to one side.

There is apparently, however, a capacity for recovery in some of the molecular changes of the nervous lesion, for we know that the light reflex may return after many years. Reflex irido-plegia is common in general paralysis of the insane, which disease is often combined with tabes.

Nystagmus, only noticeable when the eyes are in motion, may occur, and may fairly be called ocular ataxy.

Twitchings of the orbicularis muscles, credited to a slight facial paresis and epiphora, are occasionally seen in tabes.

Pupils.—In tabes we expect to find myosis, which is often extreme. A myotic pupil may possibly act to light and convergence, and may or may not be present in the Argyll-Robertson pupil. Disease in the front part of the aqueduct of Sylvius is supposed to cause paralysis of the pupil-dilating fibres, and so give rise to myosis. The radiating fibres of the iris are supplied by the sympathetic, and atony of them is caused by its impairment. This sympathetic disturbance can be shown by the refusal of the pupil to dilate when the skin of the neck is painfully stimulated. The pupillary action may be, however, occasionally though rarely unimpaired. In about five-sixths of these ocular cases the reaction of the pupil to light is lost.

Light perception will be present unless destroyed by atrophy of the optic nerves. The true Argyll-Robertson pupil is generally understood to be one which, usually contracted, contracts still further on convergence or accommodation, but which does not respond or contracts only slightly to the stimulus of light. The size of this pupil may be variable from its usual condition of contraction to normal or even to dilatation; or there may be inequality in the size of the two pupils, which may be circular or not. The symptom may be an early or late one, and is important. It may not always be present, and may be monocular. It does not appear in peripheral neuritis. The origin of this condition is probably nuclear, as it may be monocular, and this view would be strengthened if there is loss of convergence with that of accommodation, though the internal recti act in lateral movement.

"Meynert's fibres," or those connecting the third-nerve nuclei with the corpora quadrigemina, have been credited as being the locality of disturbance in this condition of pupil, but if this were so the symptom would be bilateral.

Sometimes a condition of pupil exactly opposite to the Argyll-Robertson is met with. Mydriasis may also occur, and is either irritative or paralytic; the former being a spasmodic condition showing a moderate dilation with a tendency to contraction to light and on convergence, without dilating to psychical or sensitive stimuli. It is a premonitory sign of ataxia.

The paralytic condition of mydriasis is seen as part of a third-nerve paresis, and we have also the dilated pupil due to non-conduction of light stimulus found in atrophy of the optic nerve as seen in my case.

Occasionally it may be seen that, after the pupil has lost its reaction to light, hippus may continue as described in Mrs. V.'s case. Hippus signifies the oscillation of the pupil on the stimulus of light from dilatation and contraction to gradual cessation of action; and when the pupil has finally become immobile, its condition may be considered homologous of the tabetic loss of tendon-reflex.

Inequality of the pupils, called anisocoria, is always pathological, being caused by a disturbance in centrifugal channels (the third nerve and its nucleus).

If we get the rare symptom of an unimpaired iris sphincter together with paralysis of accommodation, it is usually in the later stages. In the later stages also is occasionally noticed a drooping of the lids, usually binocular and generally accompanied by myosis. This condition, called sympathetic ptosis, is supposed to be due to sympathetic paralysis.

The tension of the globes is often lowered in tabes. As regards diagnosis, the loss of iris reflex, generally though not always present, is very important. It always indicates nerve degeneration, and may well suggest antecedent syphilis. The importance of testing the knee jerk in many ocular cases cannot be passed over.

The optic atrophy of tabes is primary, as distinguished from the post-neuritic atrophy of cerebellar tumour.

Treatment.—In addition to that employed for the general complaint, it is as well with regard to the ocular condition for the sufferer to avoid smoking, alcohol, and sexual excitement. Mercury would also seem to be harmful in the later stages.

I trust the imperfections of my paper will be excused, as I have written it only with the idea of doing my little towards the work of the Section as requested in your circular. I have also carefully tried to avoid anything that does not directly affect the oculist, as it is not my desire to trespass on the domains of general medicine.

CASE OF MONOCULAR DIPLOPIA.

By J. Lockhart Gibson, M.D. Edin., M.R.C.S. Eng.

Cases of monocular diplopia are sufficiently rare to be interesting, and the one I am about to record seemed to me peculiarly clear. A medical man, aged thirty-six years, consulted me in August, 1897. His right eye had been injured years ago by a racquet ball. There had evidently been hæmorrhage into the retina, and scarring remains between the disc and the macula, and including the macula. There is a central scotoma, but the eye sees $\frac{6}{36}$ by dodging. It is the left eye that concerns us.

Two months ago he found that he could not play tennis as he had previously been able to do. He did not hit the balls straight. A month ago noticed that he had double vision, and that closing the right eye had no effect upon this. The double vision was noticed first at a concert; the singer appeared to have two heads, one being over her right shoulder. The double vision had persisted, and his sight was noticeably less good than it used to be. He had been smoking 1 oz. of tobacco daily until last week or two, when he stopped absolutely.

Examination.—Left eye, which he is quite sure saw $\frac{6}{6}$ until lately, but not $\frac{6}{5}$, sees $\frac{6}{12}$. Each letter appears double; the real image being clear, and the second image much fainter and below and to the left of the real one. Media clear. By direct examination, fundus healthy, and seen clearly as to horizontal vessels with no glass; vertical vessels not seen as well until — 1·5 D placed behind my mirror, though nearly well enough with — 1 D.

Correction — 1·25 D cyl. axis 3° to right of vertical, gave vision $\frac{6}{6}$ as an immediate result, and with immediate disappearance of the diplopia. He was ordered this cylinder for the left eye, and a plain glass for the right for constant wear. In eleven days reported eyes quite comfortable, reads four letters of $\frac{6}{5}$, sees double at once if glass removed. In March, 1898, vision remained the same. Excessive smoking had been resumed. Was excessive smoking the cause of the acquired myopic astigmatism?

It is quite possible that an individual not trained to observe accurately and minutely as this patient had been might have acquired as suddenly as much myopic astigmatism without being troubled by double images, though suffering from markedly reduced sight. The only explanation which seems worth entertaining is that the clearer of his images was obtained from the focussing on the nearly horizontal or emmetropic meridian of his fundus of the rays which diverged from the nearly horizontal plane of the object looked at, and the second or less clear image by the attempted focussing on the nearly vertical or myopic meridian of his fundus of the rays diverging from the nearly vertical plane of the same object.

As a result of the deliberations of the Sub-committee appointed, it was decided that Dr. Lindo Ferguson, President of the Section, report the following recommendations from the Section to the general meeting of Congress:—

1. That in the opinion of this Section it is highly desirable, both in the interests of the children themselves and of the future of the race, that the incidence of educational work should be much lightened for girls between the ages of twelve and fifteen years by reduction of the school hours and home lessons, and that their physical development should be aided by appropriate outdoor physical exercises as part of their school course.

2. That it is desirable that school teachers in training schools receive such instructions as may lead to their recognition of symptoms of their pupils indicating defective vision, hearing, or other physical condition calling for medical attention.
3. That the attention of the colonial Governments be called again to the resolutions passed by the Intercolonial Medical Congress held in Dunedin in 1896 on the subject of the standard of vision in sailors and railway men, and that the urgency of their adoption should be again emphasised, with the recommendation that a uniform standard for all the colonies be fixed, not lower than the highest standard at present demanded in any of the colonies for both form and colour.

The recommendations were adopted unanimously.

SECTION OF MIDWIFERY AND GYNÆCOLOGY.

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PRESIDENTIAL ADDRESS.

By M. U. O'Sullivan, L.R.C.P. and S. Edin.,
Hon. Surgeon to Women's Hospital, Melbourne.

Gentlemen,—

Let me, before the academic duties of this Section commence, take this opportunity of expressing my high sense of the unexpected honour conferred upon me by your central executive in electing me to preside over your deliberations. I appreciate with just pride the confidence placed in me, and I can only say that I shall endeavour to justify the choice. You will, of course, recognise at once that the president is only a unit, and that, if the work of the Conference is to be fruitful of good, members must attend the meetings regularly, and join in the discussion, for it is only by subjecting the products of individual labour and experience to an honest, intelligent, and fearless criticism that we can hope to utilise the material at our disposal to the best advantage. It is only by the free interchange of thought and practice that we can hope to apply our knowledge to the service of the thousands of poor suffering women who seek our skill and assistance. We are all students in our craft, and every day brings forth something new.

As we stand on the threshold of another century and calmly contemplate the one that is just passing away, we cannot but be struck with its amazing and unrestful progress, its varied and useful discoveries, its many crowning intellectual achievements, its untiring enterprise, its marvellous advancement in every department of applied science. Its later decades have been made memorable by a positive and continuous advance in all branches of medicine and surgery, but in none is this more evident than in the special section embraced in the title of our department. Its closing years mark an epoch in obstetrics and gynæcology as transcendent for the welfare of suffering womankind as they bespeak the glory of the future. It is, I think, peculiarly appropriate to this occasion that we should to-day gather round its dying bedside and look back with reverence on those years that must ever remain a golden era in the science and art of gynæcology. Modern specialism in medicine dates from the period when the French schools, early in this century, took up the question of pathological anatomy. This was the physical basis on which the work of segmentation began, but, about thirty years ago, the literature bearing on female affections was but scant and ill-digested. Hitherto it was tacked on as a mere neglected appendage of obstetrics, taught only in a perfunctory manner in our medical colleges, at the close of the obstetric course, by the professor of midwifery, and treated in a haphazard way in the medical divisions of our hospitals.

Hitherto the gruesome inscription " All hope abandon ye who enter here" might have been aptly placed above the door of every special ward and operating room. But the scene has changed, and we are the happy witnesses of an ever-advancing wave of progress.

The vistas that are opening up before us—with a still improving technique, with a more thorough and accurate knowledge of pathogenesis, asepsis, and sterilisation; with the development and growth of organo-therapy; with the prospect of an early solution of the ætiology of malignant processes—foreshadow the dawn of an era that is destined to grow bigger and bigger with the triumphs of our speciality.

Australasia—during the early years of her professional existence—suffered from the crushing disability of being far removed from the great laboratory of European research, but, surcharged with the potential force of a vigorous youth, she, to-day, though lacking the grace and dignity of age, moves steadily and independently along the lines that make for progress.

The profession in this continent has, through the influence of specialism in recent years, attained a position and a status which it never before enjoyed, and I am sure no one will now contest our right to the vanguard in the battle for pre-eminence that is being waged all along the lines of specialisation. But, while we take a just pride in our proficiency and in our contributions to gynæcological progress, let us not forget the debt we owe to those pioneers, most of whom are happily still amongst us—Balls-Headley, Foreman, Chambers, Moreton Tracey, &c.—whose masterly hands first planted the flag of our speciality on the bosom of this fair land, and who still watch with anxious interest the mutations of its progress: those men whose best energies have been so successfully devoted to the broadening and refining of the ever-fertile field of female special ailments. To them, I am sure, will ever be accorded the recognition and respect of their fellow-workers in this country.

Now, gentlemen, while our labours at this Conference are congenial, and our purpose clear and distinct—while our work will, I am sure, be characterised by enthusiasm and wholeheartedness—let us remember that professions, like individuals, have their besetting sin. And it may be well, for the time is opportune, to pause and reflect in a philosophic way on a few of those shortcomings that have in some degree marred our progress in the past, and may, if unremedied, continue to affect the destiny of our speciality in future.

I would, in the first place, fain direct attention to the tendency in our "specialism" of being too "special," or, to put it more tersely, of being "lopsided," in the discharge of our functions.

There would seem to be a decided leaning in the present trend of gynæcology to leave the medical side of the speciality undeveloped—to sacrifice a careful clinical study of disease, and a careful and exact determination of diagnosis and prognosis, to the glory of immediate results in abdominal surgery.

Modern specialism would seem to be threatened with the danger that tradition might hamper its progress—the danger of emulating characteristics of former civilisations, by following in their footsteps, in the process of specialisation. In the early days of Greece and Rome we find professional segmentation carried to its utmost limit, until each resultant speciality lost the potential power essential to its healthy existence and perpetuation.

Baas tells us that in the time of the Empire there were aurists, uroscopists, catheterists, clysterists, bleeders, hernia doctors, milk doctors, herb doctors, movement curers, gynæcologists, &c.—

> "Think, gentlemen, of poor humanity in the days of yore;
> For these were specialists and nothing more."

Immediately after its recognition as a special department of scientific medicine—while our speciality was yet in its infancy—the vast amount of work that fell into comparatively few hands gave an enormous impetus to surgical gynæcology. Most of it was the more serious class of surgery that could never, at this time, have been efficiently done outside the compact dominion of the elect. Men whose reflexes were, so to speak, all operative, responded to the stimulus of skill and opportunity—men whose tendencies, whose habits, whose desires were all surgical, ascended from one brilliant success to another, taking advantage of every occasion, embracing every opportunity, until they built up a reputation and a prestige that overshadowed the medical man, and led to his entire abandonment of women's diseases, and, worse still, to the almost complete neglect of the medical aspect of our speciality by specialists themselves.

If proof of my contention be required, I would ask you to look down the long roll of gynæcological celebrities, and see if the fame of everyone does not rest on his manipulative dexterity and surgical powers.

I would ask you to inquire into the ideal of every aspiring specialist, and see if his interpretation of gynæcologist and laparotomist—of good operator and good surgeon—are not synonymous.

I would recall to your minds the general attitude of the gynæcologists towards statistics of pathological conditions of uterus palliated or cured without operative interference.

One need only look over the list of papers at any gynæcological congress to see how exclusively the surgical aspect of this new science has been occupying the minds of its exponents.

Let no man charge me with a desire to detract from the brilliancy and solidity of the work done by the pioneers of our art, or to minimise the unprecedented successes achieved by our fellow-workers in this virgin field.

My sole desire is to broaden our limitations and extend our sphere of usefulness and refinement; to prevent the process of specialisation being carried to complete segmentation; to assert the principle that we are an integral part of the general medical profession, and that the success of specialism is primarily the product of sound general medical culture; to foster the spirit of universal medicine, so that the connection of individual parts, the unity of the whole, may not be lost sight of.

I am sure no true scientist can take exception to an academic survey of a state of things that is likely to affect the permanence, and influence the enduring greatness and nobility, of a speciality that has helped more than any other to dispel the lingering clouds of medieval mysticism, and deliver the healing art from out the long dark centuries of superstition and ignorance—a speciality that to-day sheds its effulgent rays in the darkest corners of the earth, and brings unspeakable benefits to suffering humanity.

In view of this tendency it seems to me that a very definite and decided *rôle* falls to the lot of gynæcologists in this as in every other country.

While we in Australasia take credit for our present status and past achievements—while we accept, without qualification, the useful inventions and suggestions of other countries—it devolves upon us to resist, as far as possible, the pressure of our environment, and steer clear of the pitfalls of fashion in surgery, by scattering broadcast the seeds of an enlightened and attractive comprehensiveness, by inculcating at the bedside a knowledge of what should ever be the highest ideal of our art—an exact interpretation of indications.

Then, indeed, will promptness of action take the place of expectancy and helpless indecision in our after-treatment. Then there will be no more sundering of links in the chain of our records, and our future will be worthy of a glorious past.

Perhaps the most regrettable result of this "lopsidedness"—of this strange, inexplicable infatuation for the surgery of the peritoneal cavity—is the neglect of reparative and plastic work. While the profession and the world marvelled at the unexampled achievements in the abdominal operative work of recent years, it could not be denied that there was but little being done to perfect the surgery of the vaginal walls, cervix, and perineum.

The humbler plastic work would seem to be overshadowed by its exalted but no more important partner in this section of our jealous art. It has dropped almost entirely out of our society discussions, and is, to-day, no more than an incongruous mixture of many methods, differing widely in principle and hopelessly divergent in practice. Distressing conditions were not corrected, or only in an imperfect and inartistic manner, because gynæcologists were not giving the subject the careful attention its importance demanded.

No doubt many factors contribute to this widespread apathy and want of thoroughness. The surgeon may lack the delicacy of manipulation, the sense of proportion, the special aptitude for coaptation and adjustment of parts, that are so essential to success in the execution of all plastic procedures.

But the unwelcome thought obtrudes itself upon me that the more serious impediment to perfection will be found in a failure to appreciate the part placed by the cervix in gestation and parturition, as well as its tendency to mal-development and disease; in an incapacity to estimate the *rôle* of the component parts of the perineum in maintaining the pelvic floor in its physiological integrity; and more particularly in a failure to grasp the results of injuries and the mechanics of the production of different pathological conditions resulting therefrom, as well as the most approved method of their correction.

Let me instance the mechanics of the production of rectocele due to the laceration of the posterior vaginal wall, involving the pelvic fascia, and the treatment generally adopted.

It will, I think, be now admitted that the latter injury constitutes the chief pathology of this lesion; and keeping well before us the golden aphorism of all scientific medicine, "*Sublatâ causâ, tollitur effectus*," it must be admitted, too, that the rational relief of this complex condition can be effected only by "catching up the retracted pelvic fascia at such a point and in such a manner as to take in the slack throughout the pelvis" (Emmett). Thus only can dislocated pelvic organs be restored to position, and rendered capable of untrammelled function; thus alone will the sagged and pouching posterior vaginal wall be brought forward into normal contact with the vesico-vaginal septum, the displaced anus lifted upward, and the separated levator ani muscles brought again into physiological apposition.

How discouraging to think that we are still face to face with crude and unfinished methods. The popular perineal procedures of to-day—the fanciful denudations of variable areas of vaginal walls, the suturing together of labia, and the bunching up of tissues—cannot, by any stretch of imagination, be construed as the reality. They have no rationale, and give but scant relief.

Successful imitation is rendered possible in every department of art and literature by careful study of the best methods. The artist, in looking at a picture or statue, takes in at once any defects in the work before him, but these opportunities were obviously impracticable in plastic surgery.

Few have had the privilege of seeing the peerless Emmett, but those who tried to imitate his inimitable and faultless methods had at least the satisfaction, though they may fall far short in their efforts, of having a correct conception of the highest ideals in tracheloplastic and perineoplastic surgery. To perform this class of work after the artistic style of its distinguished master would be claiming more than the average gynæcologist could modestly hope to accomplish.

But there was another difficulty that beset the student in his endeavour to achieve success, more particularly in perineoplasty—the difficulty of formulating a perfect technique from diagrams or pictorial descriptions; it is, therefore, I think, obviously essential, if he would become expert, that he must embrace every opportunity of seeing and assisting in the execution of the most approved methods—Emmett's, Hegar's, Tait's, &c.—methods that have stood the test of time, and the stress of countless innovators and would-be modifiers; that he must become thoroughly acquainted with the groundwork of the conditions he is dealing with and the means indispensable for their treatment.

No language can be fully understood without a knowledge of its grammar, and no art can be successfully pursued by anyone who is ignorant of his subject and of the nature and use of the tools necessary for the proper discharge of his functions.

In the interests of prophylaxis, I would here direct special attention to the important part played by the cervix in the pathology of very many local and general health- and life- destroying conditions.

It was the seat of numerous ills—it was the portion of the uterus which gave most trouble in menstrual life: in health it controlled the entrance of pathogenic micro-organisms to the uterine cavity; but in disease it only too frequently became the storehouse of infectious germs.

It was the part most subject to injury during child-birth and the many progressive morbid conditions that followed in its train:—Catarrh, chronic metritis, hyperplasia and elongation, ectropion and adenoma, nervous disturbances, tubal involvement, sterility, and malignant degeneration. It is obvious, then, that we have here again a wide field for plastic and preventive gynæcology of the most necessary, fruitful, and progressive kind; and I have the temerity to venture to assert that when the profession in this continent—where cervical injuries are so frequent, and cancer grows apace—is prompt and efficient in dealing with such lesions, that it will not only contribute very largely to the future success of this growing branch of our speciality, but that it will also rescue it from the opprobrium attaching to apathy and indifference, and remove a condition that must ever be regarded as a fruitful source of ill-health and a menace to life.

There is another aspect of this subject of cervical lacerations that suggests itself to me as worthy of some consideration—an aspect that has always been the cause of a great deal of misunderstanding in the profession, has created much hard feeling amongst patients, and has so frequently brought them into unforgiving conflict with their obstetrical attendants—that ethical aspect contained in the few simple words, "Neglected at confinement." I am sure it has fallen to the lot of most of us to have experienced cases where the thoughtless utterance of a specialist, whose unguarded and ungenerous opinion, "Neglected at confinement," given perhaps years after the occurrence of an unavoidable trauma, has done gross injustice to the general practitioner; where the existent condition should, in all honesty, have been charged to the patient's neglect of herself rather than to the carelessness of the obstetrician.

If we would crush out for ever such contemptible dishonesty, and make the "medical thief" a thing of the past, the profession would do well to tell their patients that it is beyond their power to prevent laceration, and that it is by no means considered a wise procedure to sew them up at confinement.

If women were so enlightened, it would be impossible to poison the mind of a patient against the man who helped her conscientiously through her wearisome confinement.

The last but by no means the least serious weakness in the stronghold of our speciality is embraced in the classification and treatment of "diffuse peritonitis," and I would lay special emphasis on the inefficiency and indecision that so often characterise the treatment of the post-operative variety of this fell disease. A bacterial classification was beset with many difficulties. Although it was best for all practical purposes to accept, without reservation, its bacterial origin, there remains a goodly percentage of cases in which the most searching examination failed to disclose the presence of pathogenic micro-organisms.

In a series of 110 cases recently classified by Flexner, there were twelve which he was obliged to place in the idiopathic group. Again, there was the intensely septic variety of diffuse peritonitis, where the fulminating nature of the disease had hitherto baffled our best efforts, and caused death before the colonies of bacteria could be established. But the strongest argument that can be adduced against a bacteriological classification is the generally conceded doctrine, that the normal condition of the peritoneum must in some way be modified before bacteria can cause a peritonitis—that bacteria, unaided by suitable physical conditions, are comparatively harmless.

Looking at the matter from a symptomatological standpoint, the same difficulty surrounds the situation. Our past efforts to connect the presence of any specific bacterium with a definite characteristic chain of pathological phenomena have been fruitless.

The symptoms and physical signs did not conform to any stereotyped expression. They were as varied as its causes.

The classification founded on the character of the exudate was of little clinical value; has been a fruitful source of error in the past, and had in my opinion led to many serious blunders in after-treatment.

I have long been of opinion that if we would simplify a complicated situation and dispel the confusion that has long hung round it—if we would aim at formulating a sound and useful classification of diffuse peritonitis—we can only do so effectually by drawing our conclusions from its ætiology. It was always either traumatic or consecutive; and included all septic inflammations arising from the wounds of the peritoneum—whether accidental, operative, or secondary to pre-existing disease processes.

A painstaking investigation of the clinical history would determine the structure or tissue primarily involved, and this after all was the point of greatest import to the precise and careful surgeon.

Judging from the literature at our disposal, surgeons had not always a clear conception of what was meant by "diffuse septic peritonitis"; they did not always seem to appreciate the full force of the term, which was interpreted by Tietze as that form of peritonitis in which there was little or no exudation, severe symptoms of intoxication, and terminating rapidly fatally.

That men were honest and tried to be accurate went without saying, but how can we reconcile the report of a series of eight cases—six operative recoveries—by McCosh, with the experience of Senn who tells us that he has opened, drained, and washed out the peritoneal cavity in many cases of diffuse peritonitis without a single successful result? The answer was not to be found in superior operative skill and more perfect technique of the one man, but rather in an analysis of the cases. The six recoveries as recorded by McCosh, and classified under the head of "general septic peritonitis," were ordinary cases of more or less localised inflammation with purulent effusion and some degree of sepsis—not at all the type of cases referred to by Senn, who discriminated between this variety and the diffuse septic peritonitis as described by Tietze—the patients dying within twenty-four hours of profound toxæmia, with a dry peritoneum.

Diffuse post-operative peritonitis, with which I am chiefly concerned, was perhaps the most serious condition that demanded our attention. Here, not only had we a primary inoculation, but the conditions were all most favourable to germ culture, quick absorption, and general dissemination.

The patient was precipitated, within a few hours, into a state of the most grave and utter hopelessness. Here the surgeon was brought face to face with a crisis that taxed to the utmost his resources, and that has heretofore been looked upon as almost beyond the scope of successful treatment; and here is exemplified most plainly yet another lingering weakness in our already much-perfected methods of combating disease and death.

With regard to the treatment of such situations as the latter, I have no hesitation in stating that I am a ritualist; that I pin my faith, firstly, to prophylaxis, to absolute asepis, a perfect technique, and skill in operating. Surgery can do much towards preventing this malady, but very little towards saving life after it has once been developed.

The medicinal treatment may be almost summed up in free catharsis. But already there is a prospective hope that serum-therapy will ere long establish its reputation as an approved and successful method of treatment, and that abdominal surgery will be rescued from the grasp of this exacting foe.

Early bacteriological examinations will be instituted in our hospitals, to settle the question of diagnosis and the indications for treatment.

Serum of a standard preparation and strength—suited to individual cases—will be used, and victory once more looms in the promise of the near future.

If the disease has already established itself, there can be no second opinion—in the light of all contemporary knowledge—as to the propriety of its early operative treatment. Without operation, death is almost inevitable, and timely operation may succeed in arresting its further progress; in cases advancing to general diffusion, and even in diffuse cases, it may help to save life.

Such direct treatment has not up to the present given satisfactory results, but I think it may be fairly assumed that this failure is mainly due to the fact that intervention is generally too late, and I have no doubt that all our future surgical efforts will be directed to the early opening, the copious lavage, the thorough cleansing, and the free drainage of the abdominal cavity.

The reluctance that has heretofore been shown by the profession to deal thus radically with diffuse peritonitis would seem to be a legacy begotten of the widespread fear which formerly existed with regard to any disturbance of the peritoneum; but knowing, as we now do, that all the untoward results of previous surgical interference with this delicate membrane were due to coincident infection rather than to the trauma, this is the strongest proof we can have of the necessity for the prompt and thorough removal of those very pathogenic elements which are to-day, as they were in the early years of abdominal surgery, the source of this dread disease.

And now, gentlemen, I will conclude by proclaiming, as a set-off to the trifling debit balance of the shortcomings which I have thus hastily depicted, that the "credit" side of our triumphs is full to overflowing.

The debt that woman owes to advanced surgical gynæcology is beyond all computation. Vaguely conceived and hopeless ailments of a few years ago are now diagnosed, and cured as a daily routine.

Let me instance the single life-menace of ectopic gestation. Who is there amongst us that has not fallen upon a number of such cases—all formerly fatal, but now entirely restorable to health and life. If suffering woman would demand a more detailed statement of her indebtedness, if she would have further proof of our *bona fides*, of our capacity and readiness to deal with life-taking afflictions, I would remind her of the countless numbers of her sex rescued from death by pedicle-torsion and intra-peritoneal rupture—of the number of lives given back to a world of joy and gladness, by the specialty of which we are the Australasian representatives.

PSEUDO-DIPHTHERITIC ENDOMETRITIS VAGINALIS.

By Frank S. Nyulasy, M.B., Ch.B.,
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Fortunately this diphtheroid inflammation (as I would prefer to call it) is rarely seen nowadays, but it was very prevalent in times gone by, for Winckel found 52 (!) lying-in-women affected with œdema and ulcers at the vaginal orifice among 172 patients in the Berlin University Lying-in Establishment during 1861 to 1862; and at the Rostock Institution, in 1869, he found the same condition recorded 59 times in 816 childbeds.

The disease was commonly mistaken for true diphtheria of the genital passages, but while presenting similar appearances the two conditions are totally distinct.

True diphtheria of the genitals is, of course, due to the Klebs-Lœffler bacillus, and has its own specific remedy, while the pseudo form is caused by a mixed infection with streptococci and others of a varying kind, and has a treatment which will be described hereafter. Only four cases of true diphtheria, bacteriologically verified as such, are on record up to date. They were reported by Nisot, Bummim, Haultain, and Whitridge Williams respectively. True diphtheria of the genitals is, therefore, extremely rare, but the pseudo form, according to Whitridge Williams, of the John Hopkins University, is frequently met with. In this opinion I am not quite at one with him, although I agree that it is much more common than the true form.

I have met with four cases of pseudo-diphtheria of the genitals in hospital practice, and as they all presented fairly uniform symptoms, not quite agreeing with those usually described, and as the treatment, though perfectly successful, was not nearly so severe as that suggested by recent writers on the subject, a brief record of my personal experience in dealing with these interesting cases may not be out of place.

As to *Causation.*—I traced the origin to infection by contaminated instruments, chiefly douches. And it is my opinion that the condition is most apt to arise, in hospital practice at least, where the most rigid and up-to-date aseptic and antiseptic precautions have been neglected for a length of time.

Symptoms.—The first symptoms to attract attention are usually startling. A rapid rise of pulse and temperature, usually followed by a rigor, when the temperature reaches the highest point. Some writers like Winckel, in Germany, and Henry Tellett, of the Rotunda Hospital, Dublin, say the fever is moderate, but in my case it was high. The local signs are greyish-white patches on the vagina, usually the posterior wall, and especially where lacerations are present. The patches soon coalesce and spread, and may uniformly cover the whole posterior vaginal wall, or extend even further than this. The edges of lacerations become swollen, painful, and sloughy-looking. The discharges poured out upon the affected surfaces are of a peculiarly acrid character, and cause *excessive* redness, tenderness, and pain of the parts below. The severity of the pain is soon made manifest on any attempt to examine the parts, especially if a speculum is used, for the patient will then cry out in a most distressing way.

The patches or puerperal ulcers, as they are sometimes called, are neither so thick nor so dark as those of true diphtheria, nor are they in the form of a well-defined membrane like those of that disease. I have a theory that the patches in puerperal cases are really caused by the unusually irritating nature of the discharges from the germs present, the germs themselves having become previously modified in the direction of increased virulence by the concentration of a number of septic cases which in the first place may be quite mild, the only condition then required for virulent development being some neglect in aseptic and antiseptic technique. Having arrived at the full stage of their developmental energies, the germs secrete discharges of a more than usually irritating nature, which, so to speak, "scald" the parts of the mucous membrane over which they pass. In the majority of cases, the uterine mucous membrane will be found to be affected at the same time. On exposing the "os," there will probably be seen a thick purulent discharge pouring from it. The discharge is odourless as a rule, and not foul and fœtid as usually described. On passing a blunt curette *very gently* over the uterine surface, the same milky white patches will be brought down, and very free bleeding will follow this simple procedure, showing, of course, the great vascularity of the affected parts. The whole lining membrane of the uterus may be softened and necrosed.

As to treatment, considerable differences of opinion exist even among recent writers. Thus Grandin and Jarman, of the New York Maternity, state that "where the process is just beginning, the whitish patches may be touched to advantage with the actual cautery, or they may be scraped off with the curette and a solution of nitrate of silver, 60 grains to the ounce, may be applied—in case the infiltration has extended widely before it is discovered, we believe that the best measure is thorough cauterisation of the surface with the actual cautery. The surface is then kept powdered with iodoform or bismuth, and carefully watched for the detection of evidence of spreading, when the same cautery may be used." Even more radical treatment has been occasionally carried out, for Dr. Barton Cook Hirst, in an article on "Cœliotomy in the child-bearing period," states that "one is most likely to perform an unnecessary operation in diphtheritic endometritis. The writer has thus erred several times. By the time that symptoms justify surgical intervention in this condition it is always too late." My own opinion is that when promptly diagnosed and treated neither cauterisation nor cœliotomy will ever be required. When the uterus is affected at the same time as the vagina, that is in the majority of cases, I first douche the parts, then with a *blunt* curette *very lightly and gently* scrape the uterine surfaces, douche again, pack with iodoform gauze and liberally dust the affected parts of the vagina with iodoform. If this treatment is promptly carried out the patient will, probably in most cases, be well on the way to recovery in twenty-four hours, and the effect on the relief of pain is truly wonderful.

I append a few brief notes of four cases which all occurred in close succession about four years ago at the Women's Hospital, but I have seen none since. In one way I regret this, for my orders for the taking of "swabs" for bacteriological examination were for some inexplicable reason neglected. The vast improvement effected in the general management of the midwifery department of the Women's Hospital has completely banished all conditions which have the slightest approach to those above described.

Case I.—J.B., æt. twenty years, primipara, admitted 14th March, 1896. Discharged 27th March, 1896. Had normal delivery of a living male child weighing 8 lb. on the evening of 14th March, 1896. Next morning, 15th March, temperature at 8 a.m. was 101°, at noon was 103°, at 6 p.m. was 103·8°, at 9 p.m. was 104·2°. The pulse was 118, and 126 at night. On the following day (16th March) the temperature was 103·2° in the morning, and the pulse 118.

On examination diphtheroid vaginitis and endometritis were found, and I at once adopted the treatment above described. In the evening, the patient was much better in every way, the temperature had fallen to 101·4° (nearly 2°), and the pulse to 106 from 118. Next day the temperature was 99·4° morning, and 101·6° evening, the pulse being 96 at both periods. On next day, the morning temperature was normal, the evening 99·6°; and the patient then progressed rapidly, being discharged on the thirteenth day after admission.

Case II.—The second case was one that I had to curette on the ninth day, when the temperature was 104°, for adherent bits of placenta. The temperature fell a degree daily on the average, reaching normal on the thirteenth day; on the fourteenth day it was still normal; on the fifteenth day it was 100°; on the sixteenth, 101°; and on the morning of the 17th instant it suddenly shot up to 105°, and was accompanied by a rigor. I then found diphtheroid vaginitis and endometritis with thick odourless pus welling out of the uterus, but there was no fœtor and little or no tenderness of the uterus to external pressure. The above-described treatment was adopted, on the following morning the temperature was normal, and reached only 100° in the evening. Next day everything was satisfactory, and so continued till the patient's discharge.

Case III.—The third case was a young primipara who had perineal laceration, and had to be curetted on the tenth day, her temperature being 104° in the afternoon (from retained placenta). The temperature then gradually fell to 101° by the seventeenth day, when, on the evening of which, it suddenly rose again to 103°. There was diphtheroid deposit on the lacerated vagina and on the endometrium accompanied by a purulent odourless discharge from the uterus. The same treatment was applied as in the last case, and recovery was equally rapid.

Case IV.—The fourth case was curetted for adherent placenta on the fifth day, when temperature was 100°, but improvement was not obtained, for there was a daily rise till 104° was reached on the ninth day, when a rigor occurred.

This want of improvement after the curetting was distinctly traceable to an infected douching apparatus. When the temperature was at its height, and after the rigor, examination revealed diphtheroid deposit on the vaginal laceration and endometrinm with thick, odourless pus welling out of the uterus. The necrosing endometrium was at once gently removed with a blunt curette, the uterus packed, iodoform dusted into the vagina, and the temperature fell a degree daily to normal. This concluded the record of what, to me at least, were four very interesting and instructive cases.

CONSERVATIVE GYNÆCOLOGY.

By Joseph Foreman, M.R.C.S., L.S.A.,
Sydney.

Several here are old enough to remember the rise of gynæcology, at any rate in these colonies, and nothing to my mind indicates the advance it has made and the larger grasp of the subject than the wave of conservatism which has only commenced to flow. The old order and the new are strikingly instanced by a case of my own about fourteen years ago. A patient had a cystic ovary and chronic salpingitis with recurrent attacks of peritonitis, and I wanted to remove both ovaries at the operation, but had to promise to leave one at the risk of a second operation. The patient went home some months afterwards, and being ill went to a very distinguished gynæcologist, who remarked, when told that I had removed only one ovary, "That is very bad surgery." I must say that she did me the justice to tell him that the fault was not mine. That was the teaching some years ago, and in our knowledge of the different organs was quite correct. Age and a wider experience have caused us to modify our views. When young we are most of us very radical, and like to make a clean sweep of everything, but as we grow older we take quite a different view. Now we look on the ovary not only as ovigerous, bnt also as a ductless gland secreting a substance called spermin. At the last meeting of the B.M.A., at Portsmouth, in August, 1899, the president of the Gynæcological Branch (Dr. Bantock) states in his address that there is not an atom of evidence to support the idea; and Bland Sutton, in the discussion on the natural menopause compared with the menopause produced by removal of the uterine appendages, remarked that the functions of the ovary were not merely ovigerous—that Graafian follicles were to be regarded as mucous crypts and the ovary as a ductless gland of the same type as the thyroid. Here you have two distinguished men only a few months ago giving fresh diametrically opposite views, and their practice corresponds. For my part I am strongly of Bland Sutton's view, for, whatever the cause, there is no doubt the removal of the appendages gives rise to disturbances which can only be accounted for by the removal of something from the system necessary for its healthy nutrition. Looking back over my cases there are some which I wish I had done differently. There is one case particularly in which, though the patient is well, still there is always that feeling about her that she is not as other women, and I have noticed that in several. This view that they take of themselves is one of the finest considerations for conservation, for we cannot look on our patients as cases, but to use our skill to make them happier, and I am sure no woman can look with pleasure on the loss of organs which constitute such a factor in the enjoyment of life. It was this mental effect that caused me over ten years ago to leave part of an ovary where the other had to be sacrificed, and I have, to the best of my power, carried out that practice ever since, and the more I do it the more firmly I am convinced that it is correct. An amusing incident with regard to this happened a few months ago. A practitioner from one of the northern towns wrote to me to inform me that he had delivered a woman of a fine boy from whom I had previously removed both ovaries. I looked up the notes and found I had removed a cystic ovary on one side and half the other ovary only. Of course, I got a good deal of blame, or the hospital did, for the way that ovariotomy was performed, but the patient did not regret it, nor do I. To remove an ovary, because the other is diseased, will, I hope, soon be spoken of in the past tense, for though I have done it I consider, with present knowledge, it would be unjustifiable, and in case a second operation is necessary it is very safe. Purulent tubes do not of necessity require removal. Some little time ago I operated on a newly-married woman who had double salpingitis. I had to remove the right tube—the left I opened and syringed out with peroxide; she got quite well, though whether she has become pregnant I do not know. I well remember another case where the right tube was cystic and the left had become infected from the abdominal end—the inner third being quite healthy. I syringed that out also with the best result. There were three other cases of pus tubes which were saved. I am giving these instances to show that it is done here, and to hope that we shall soon look on anything saved as one of the greatest feats of surgery.

Another instance in which conservatism might be practised—viz., fibroids. The fact of having a fibroid is sufficient for some to suggest an operation, which in the great majority of cases is, I am sure, questionable. Only the day before I left Sydney, I saw a patient who knew eight years ago that she had a hard lump in her abdomen. Eighteen months ago she had a little bladder trouble, and went to consult a doctor who told her she had a tumour which would grow and kill her, and wanted to remove it. Another wanted to remove a portion only—the ovaries, I suppose—and the rest would disappear. She had a multiple fibroid, which seemed to give very little trouble—it did not interfere with her work; she was forty-three years old, and consulted me chiefly for fright. The advice was to do nothing. Surely with a little management how many cases of fibroid might we not tide over the menopause without danger. In young women, I think we have no option but to operate, but then a myomectomy is to be preferred, or if that is not possible then we should endeavour to leave one ovary.

There is another class where conservatism is badly called for—I mean Emmett's operation for lacerated cervix. The first idea a student starts with —I suppose it must be inborn—in the outpatient room is that the cervix is lacerated, and it is the most difficult thing to get him to another way of thinking. I am one of those who consider that a lacerated cervix is no disadvantage, rather the contrary, provided there is no ectropion, and it has not gone so far as to cause a miscarriage in a subsequent pregnancy, yet how many cases we see operated on, or wanted to be so, without this being the case. I have no hesitation in saying that when these two conditions do not exist that some other cause will be found to account for the patient's troubles.

I may say, in conclusion, that this subject is chiefly brought forward to provoke discussion.

DR. WORRALL (Sydney) said he had not ventured to leave pus tubes, but he had often removed the tubes and left the ovaries or one ovary. Tubes which contained pus could scarcely be expected to perform their functions in a normal manner, and leaving them, even after washing them out with peroxide, would imperil the life of the patient, unless the pus was sterile, as it often was, fortunately. With regard to lacerated cervix, he believed the parts should be restored to their natural condition by trachelorrhaphy. If this were not done, secondary changes with dependent symptoms almost invariably occurred.

DR. PINNOCK (Ballarat): As to the class of case of laceration suitable for operation, if operators would only follow the lines laid down by Emmett himself there would be very few unnecessary operations performed. Next comes the importance of removing cicatricial tissue. This, in my opinion, is the crux of the operation, as it is the immediate cause of all the reflex neuroses which make the woman's life so miserable. This cicatricial or sclerotic tissue when present is practically the dividing line between treatment by dilatation and curetting, and the operation formulated by and named after Emmett. Where it exists and is thoroughly removed, I know of no operation in the range of surgery which gives such speedy and gratifying results. I always leave a strip of undenuded cervical mucosa in preference to inserting drain tubes or gauze plugs, and have never had stenosis and consequent dysmenorrhœa resulting.

DIGITAL DILATATION OF OS UTERI IN LABOUR.

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New Zealand.

Three years ago, at Dunedin, I had the honour to read a paper under the above title. The subject-matter created an animated discussion, the majority, if not all present, being opposed to the procedure on the grounds that it entailed an additional risk in the way of tears of cervix. I was quite sure in my own cases there had been no tearing, but at that time had no definite proof. I have now traced as far as possible the patients whose cases I narrated in my 1896 paper, and the following is their history:—

No. 1.—In good health; has never consulted a doctor since; she is well known to me.

No. 2.—Has been attended twice since; once on 9th July, 1896; of this labour I have no record except—$CHCl_3$, instruments—(full notes are in my lost casebook); and once on 12th July, 1898. From case-book: Been in labour some hours, os flaccid but not dilated. Presen.: Hand and head high up; liq. amnii drained away; turning rather difficult; mother and child did well; there was no tearing of cervix.

No. 3.—Has left district; up to 1896 when she left, although I always attended at house, she never complained.

No. 4.—Attended twice since, once on 13th September, 1896—$CHCl_3$, forceps; once on 18th October, 1898, in case-book. Third, girl, $CHCl_3$, instruments; very easy; no tearing of cervix.

No. 5.—No trace.

No. 6.—Have attended at patient's house several times since. She is in perfect health—rides, golfs, cycles, &c.

No. 7.—No trace after two years; was well up to then.

No. 8.—Has been attended twice since, once on 27th October, 1896. Foot presen., no record; and again on 9th November, 1897, normal presentation, $CHCl_3$; forceps, easy; girl; mother and child did well; no cervical tear. She is at present at fourth month with her fourth child.

No. 9.—In her last confinement died from exhaustion ten minutes after child was born after a lingering labour. I did not attend her. This case, I may add in parenthesis, is an illustration, to my mind, of prompt and rapid interference. I first attended her in her tenth confinement; she had never taken $CHCl_3$; her two previous doctors always said she was too weak, had always lost a great deal of blood, and been very weak afterwards, making invariably a prolonged and tedious recovery; her medical attendant had most anxious times with her. I knew all about her history from my partner, who dreaded to attend her. When she engaged me I told her I wanted to be sent for early. She lived about half-a-mile away, and I made up my mind I would give $CHCl_3$ and interfere early, for I argued that there would be less danger in so doing than in allowing the second stage to go on for from eight to twelve hours, as in her previous confinements. In her subsequent confinement she was attended by another doctor, who only knew the history that the patient gave, and the lingering labour plus the $CHCl_3$ were too much for her.

No. 10 was confined again on 4th May, 1898. Following from my case-book: Mrs. T., 2-para, hæmophilia; came down from up country to be under my care; stayed in a house under charge of my own trained nurse. $CHCl_3$. digital dilatation; forceps; all over in half-an-hour, but no bleeding at all; removed placenta by passing hand into uterus; no evidence of tear in cervix.

This patient, in December, 1896, came down and saw me. She had been ordered in to Wellington Hospital for operation for what the doctor who saw her (according to her statement) said was the biggest tear in womb he had ever known or read about. Anyhow, the fact remains, she was sent down to Wellington Hospital to be operated on for repair of the cervix. As her father lived at the Hutt, she stayed a night at home before going to the hospital, and she and a married sister came to see me the next morning when *en route* for Wellington. The married sister was very indignant with me and talked in rather a threatening manner. However, on cross-examination, I discovered that the medical man who sent her down had only seen her once; had only made an ordinary P.V.; had not put patient under $CHCl_3$, nor even, so she stated, passed speculum. Moreover, she told me that after leaving Hutt, some thirty days after her accouchment, she had a very bad fall and was laid up for a week.

I persuaded her not to go to hospital that day. I made a P.V., but neither by my touch nor with my sight through speculum could I discover any tear; as a matter of fact, the os seemed much more perfect than is usual after an ordinary labour; she had, however, subinvolution of uterus. On 15th December she was put under $CHCl_3$ and I curetted her; at the time when the cervix was pulled down it was carefully examined, but no trace of tear was to be seen, nor any old cicatrix; this was confirmed by the doctor who gave $CHCl_3$.

No. 11.—Is perfectly well; rides, plays golf, &c.

No. 12.—Is quite well, does her very hard work in a bush home, has never been laid up with illness.

No. 13.—Was confined at seventh month on 30th August, 1897; presentation—arm and head. Turning; no cervical tearing.

No. 14.—No trace.

No. 15.—Is quite well.

It will thus be seen that at any rate in my fifteen cited cases no trouble has followed. In 1896 I have no records of my cases; but in 1897, 1898, and up to 22nd May, 1899, I have performed digital dilatation thirty-five times—in one case twice on the same patient. I will read the notes of these two cases as copied from my casebook :—

9th August, 1897.—Mrs. A , primip., commenced labour at 8 a.m. on 8th August; sent for 5 a.m , 9th August, head high up, os admitted two fingers, rectum loaded, ordered enema; at 10·30 a.m. os no bigger, rectum still loaded, although enema had acted, pains very strong and frequent, ordered enema to be repeated; sent for at 3·30 p.m., pains violent, head still high up, os still undilated, there had been copious discharge after second enema had acted; there was no presenting bag of membranes. As patient was a strong woman, though she was beginning to show signs of exhaustion, having been over thirty hours in labour, I gave $CHCl_3$ and rapidly dilated digitally; the os was very stretched and inelastic; as soon as possible I dilated with forceps blade and blades and then applied forceps, head was tightly wedged, delivery was accomplished, all the parts were worn out, the perineum was torn, put in three catgut ligatures. Child a boy, weighed 10½ lb.; mother and child did well. On 22nd May, 1899, I attended same patient, first seen 11 p.m., had been off and on in pain for three days, and been fairly bad all the 22nd; head low down, os admitted two fingers, no presenting bag of membranes, $CHCl_3$; dig. dilat., forceps. Mother and child did well; there was no evidence of tear in cervix, perineum intact.

Ten other cases were confined a second time between 1897 and 1899, and in none of them was there any evidence of tearing.

In all the cases there had been early escape of liq. amnii with consequent delay in dilatation of os.

It is in cases such as these, especially when living some miles away from help, that digital dilatation should be practised; the procedure in my hands has proved absolutely safe, and I have adopted the method ever since 1879. With regard to the method, I use very little force, certainly not as much as Nature does. It is very difficult to describe what one does so as to convey an adequate idea of the procedure. Suppose os admits forefinger; in such a case I slightly hook the finger inside os, and with one or two sweeping pulls all round, taking care to, as far as possible, exert equal pressure at all points, I soon dilate up to two fingers. As soon as ever possible the second finger is introduced, and the two fingers are opened; little force is required to dilate up to three fingers; as a matter of fact, very little force can be applied; the three fingers are used in the same way, and by the time the fingers are opened as widely as possible the head is well engaged, and the os can be slipped over the head by rapid sweeps all round. Should, however, the head not engage owing to its size and shape, there is no difficulty in introducing the five digits into the os. I find, however, that it is often very much better to finish dilatation with one blade of forceps and the fingers, sometimes with both blades; and for this reason, that unless the patient is very deeply under $CHCl_3$ the introduction of the hand P.V. causes restlessness. I need scarcely add that I proceed with the utmost caution, but feel sure the so-called dangers of tearing are very much exaggerated, and that no one exercising the ordinary care that ought always to be exercised under any condition, no matter what it springs from, need be afraid of injuring the patient. Nature, we all know, is not always careful, and not always kind. In this age, when women do not like to endure pain, and rightly enough demand $CHCl_3$ in their confinements, the practitioner will find digital dilatation a great benefit in all cases where it is indicated. It ought to be a fundamental axiom in midwifery practice that interference is justifiable whenever the danger of its use is less than that of leaving the patient alone. This axiom I hope I conscientiously practise. It is very difficult to get out of the rut of ordinary routine, but, like Dr. Barr, of Leeds, heresy is attractive to me; and though I should be the first to give up digital dilatation had I one unsuccessful result, yet in my practice the procedure has more than realised my most sanguine expectations.

Dr. Worrall said he could not sanction the employment of this measure except under clearly-defined conditions, such as eclampsia, where it was of the utmost value. In most cases the head or breech of the child would dilate with less danger than any artificial or manual dilatation.

THE TREATMENT OF UTERINE MYOMA.

By Ralph Worrall, M.D., M.Ch. Dubl.,
Surgeon to the Department for Women at the Sydney Hospital.

The treatment of myoma of the uterus is palliative or radical; and just when, as the Americans say, one should be content with the former or resort to the latter, is a problem worthy of discussion.

It will, I think, be generally admitted that in the majority of cases no treatment at all is required, but, taking into consideration the low mortality rate at present attained, I should not hesitate to recommend radical operation under the following circumstances:—

1. Rapidly-growing tumours.
2. Fibro-cystic tumours.
3. Tumours associated with a considerable amount of ascites.
4. Tumours incarcerated in the pelvis.
5. Tumours which are making life burdensome to the patient by constant pain and interference with the functions of bladder and rectum.
6. Tumours which cause serious hæmorrhage not controlled by drugs or curettage.

The palliative treatment consists in what I call general and local hygiène, such as exercise, diet, avoidance of red meat, alcohol, and high living generally, attention to the bowels, the hot douche, and, in hæmorrhage cases, the administration of tincture of hydrastis. Under the head of palliative treatment we must include also curettage and electricity. I know there are some who say the latter is radical and not merely palliative, but what I have seen of its effects does not bear out this contention. In one case which came under my notice, the entire tumour came away in huge sloughs after the application of electricity, placing the patient's life for several weeks in imminent peril. Under these circumstances the treatment may indeed be called radical, but also highly dangerous and undesirable.

Curettage is a mode of treatment which is shorter, safer, less painful, less expensive than electricity, and in some cases, in which hæmorrhage is the chief symptom and the patient is approaching the menopause, it is well worthy of a trial.

We now come to the radical treatment by operation.

Believing that there still exists considerable diversity of opinion as to the relative advantages of different methods, I thought it might not be without value and interest if I made an attempt to elicit the matured opinions of the Australian surgeons attending this Congress upon the subject.

I shall mention first:

1. *Removal of the Appendages.*

This is the operation which in England has been most frequently resorted to. Thus in Dührssen's Gynæcology (1895) the translators, J. W. Taylor and F. Edge, say:—

"In all cases of myoma in which surgical treatment is advisable, this should be the operation chosen, and only those cases in which removal of the appendages is impossible should be submitted to hysterectomy."

I am glad to say that, notwithstanding this authoritative statement, I have only once performed this operation. It always appeared to me that to remove organs which were healthy in order to cure disease in another organ was not good surgery, but in addition to this prejudice the following case in the practice of my late colleague gave me a profound distrust of the procedure:—

Dr. Chambers removed the appendages for a myoma in a woman of thirty-five, and the good effects of the operation were immediate and marked. Three years afterwards, however, she was again admitted to hospital with the history that she had enjoyed excellent health until six months prior to her second admission, when hæmorrhage became again troublesome, in addition to which there was an offensive discharge, pain, and loss of flesh. Examination showed the tumour to have again increased in size; it was also absolutely fixed, and infiltration into the parametrium all round the cervix had taken place. It was only too clear that removal of the uterus was at this stage out of the question. A section cut from the cervix and examined microscopically showed unmistakably that sarcomatous change had occurred. The patient in a very short time died. As the danger of removing the appendages is not less than that of hysterectomy, and as the former operation does not certainly cure, I think we may place it in the class of obsolete operations. Of the intra-peritoneal treatment of the pedicle after the manner of Schröder, the same may be said.

The extra-peritoneal treatment by means of the serre-nœud or rubber ligature, I part with more reluctantly. I have used it in a large number of cases with very fair results, and in the hands of more than one operator, and over a large series of cases, it has given a mortality rate of not more than 5 per cent.—results difficult to beat. The recoveries, too, in my experience, are easy. It has, however, the following serious disadvantages: Great care is required in the after-treatment, which is prolonged to six or eight weeks longer; there is a weak spot in the lower end of the scar, and more or less ventral hernia in consequence. Lastly, it is inapplicable or used with difficulty and danger in a large and important class—viz., those in which there is sub-peritoneal development of the tumours. As Knowsley Thornton says, in Allbutt's System of Gynæcology, "It is the combination of ragged cavity from which the tumour has been enucleated and the sloughing stump which gives rise to the high rate of mortality." Nevertheless, I have on more than one occasion, with the aid of gauze packing, successfully used the method in this condition. Although I have now for several years discarded it, I think the surgeon who is inexperienced in abdominal surgery, and who is forced to operate with poor assistance, especially in dealing with a pregnant uterus, will probably be more successful by this method than by any other.

For tumours which do not reach above the umbilicus and are not merely subserous outgrowths, *Morcellement per Vaginam* has been largely practised by continental surgeons, and until I had discovered what I believe to be "a more excellent way," I was greatly pleased with this method, although there is hacking and hauling to a degree which makes one who sees the operation for the first time marvel that a patient could survive it, yet shock is slight and the mortality rate low. The explanation is, of course, that the parts so injured are all removed, and that those left are subjected to no traumatism whatever.

The strength and endurance of the operator, however, are severely tried by this operation.

I have operated upon five cases One died, from influenzal pneumonia, about two weeks after operation ; *post-mortem* showed an entire absence of peritonitis; the vault was closed, and everything in the pelvis looked perfect.

I present the specimen of the largest tumour which I have removed in this way. It reached to within 2 inches of the umbilicus, blocked the pelvis completely, and markedly depressed the vaginal vault. The cervix lay behind the pelvis high up, and was flush with the tumour; the patient recovered. The technique was the same in all : the bladder separated, the uterine arteries secured by ligature by means of my hysterectomy needle, and then the tumour cut away by irregular *morcellement* until small enough to be delivered, when the broad ligaments were secured from above down by ligatures.

In the two smallest tumours, anterior median section after the manner of Doyen proved sufficient to allow of delivery without *morcellement.*

Abdominal myomectomy is greatly lauded by Howard Kelly, who says, "No more important advance can be made than by extending the indications of myomectomy." I have operated in this way a considerable number of times, but in all my cases the tumour was either broad ligament or subserous. I have never ventured to attack the interstitial growths opening up the uterine cavity as recommended by him. With all respect to so high an authority, I venture to say that if this practice be widely adopted it will lead to a serious increase in the mortality rate. When it is recollected that it is unusual to find myomata anything but multiple, and that it is extremely difficult, if not impossible, to be certain that one has removed all the nodules, some very minute, scattered through the uterine tissue, the non-removal of which must lead to a renewal of the trouble; and when it is also remembered that the dangers of myomectomy are distinctly greater than those of hystero-myomectomy—it will, I think, be decided to restrict this operation to pedunculated and sessile subserous growths and broad ligament growths. By adopting hystero-myomectomy, and leaving one or both appendages and also the cervix, the interference with the genital functions is no more than if we had done myomectomy, with the single exception of the capacity to bear children, and this many patients would be pleased to lose.

The surgical treatment of uterine myomata was stationary for years, and operators were divided into those who believed in the extra-peritoneal treatment of the pedicle and those who practised the intra-peritoneal treatment after the manner of Schröder, or with modifications; then came a great advance. It was said with truth, "The cervix is the cause of all the trouble—why not remove it?" And thus came into existence the valuable procedure of "pan-hysterectomy, solely abdominal, abdomino-vaginal or vagino-abdominal." I shall not take up valuable time by discussing to whom belongs the credit of devising this operation, but as far as I can see the claims of American surgeons predominate, and I myself adopted it about four years ago after receiving from Dr. Florian King, of New York, his pamphlet upon the subject, and his frame for placing the patient in Trendelenburg's position. In passing, I may say that gynæcologists are under the deepest obligation to Trendelenburg, for it is only by the adoption of the position brought into notice by him that our most difficult abdominal operations are rendered comparatively safe.

In large malignant tumours, in tubercular disease of the uterus, and in some cases of cervical myoma, an example of which I show you, pan-hysterectomy is the operation which must be adopted.

I now come to the method which has revolutionised previously-held ideas upon the operative treatment of myoma uteri, and which in my opinion should be adopted, except under the conditions just mentioned.

I refer to sub-peritoneal hystero-myomectomy, first described, I think, by Baer, but since improved by Howard Kelly and others, and especially by our colleague, Professor Watson, of Adelaide, to whom many of us are deeply indebted for a knowledge of pelvic anatomy and surgery fuller and more complete than can be obtained from any other source.

On the 6th of January of this year, I did my first operation by Watson's method. The essential points of the operation are that the round ligaments, one of which at least can always be found even in the most complicated case, are ligatured and cut adrift as a first step; a transverse incision between these two points, through the peritoneum, is then made. The tumour immediately becomes more movable. The ovarian vessels in the infundibulo-pelvic fold on the most accessible side are then secured by themselves, the fold cut adrift, reflux hæmorrhage controlled by Doyen's forceps placed close to uterus; the broad ligament between the fold and the round ligament opened up by incising the peritoneum, the uterine vessels secured. The same steps are carried out on the other side, or Howard Kelly's plan adopted of cutting through the cervix and securing the vessels on the other side from below up (*i.e.*, uterine and then ovarian) instead of from above down. An iodoform gauze wick is passed through the cervix into vagina, the cup-shaped cervical stump united over the gauze, and peritoneum sutured by a continuous catgut suture from pelvic wall to pelvic wall, thus completely covering over the stump and all raw surfaces.

The advantages of this method over pan-hysterectomy are:—

1st. By leaving the cervix we avoid disturbing the anatomical relations of the structures in the vaginal vault. We preserve the key-stone of the arch, and thus prevent subsequent shortening and atrophy of the vagina and interference with marital relations.

2nd. There is less opening up of cellular tissue spaces and no hæmorrhage from branches of the vaginal, vesical, and hæmorrhoidal arteries.

3rd. The operation is less difficult, less prolonged, and attended with less shock.

4th. It is applicable to all cases, rendering operation for those which Lawson Tait called "the terrible cases"—that is, those with great sub-serous development—almost as safe as for the simple pear-shaped tumour.

I submit an epitome of all the cases so far operated upon by me.

Epitome of Cases Operated on by Watson's Method.

No. 1.—M.McK., forty-eight; married twenty years; never pregnant.

Symptoms.—Frequent retention of urine. Pain in abdomen, hips, and back for about two years.

Physical Signs.—Stout and flabby. Pelvis blocked with an irregular, hard, very fixed tumour, which markedly depresses the vaginal vault, and in which uterus is merged.

Operation.—6th February, 1899. Bladder adherent to parietal peritoneum. Tumour incarcerated in pelvis; an adherent R. appendage not removed. L. tube contained pus. No drain.

Death on fifth day of septicœmia.

Post-mortem by Professor Watson showed no peritonitis nor pathological condition, except thrombosis of veins around stump and nephritis.

No. 2.—E.A., aged thirty-two; single; never pregnant.

Symptoms.—Great frequency of micturition, "much bearing down and pressure feeling" for about two years past, cannot exert herself. Has lost flesh

Physical Signs.—Thin, weak pelvis blocked by a very irregular, hard, and fixed tumour, depressing vaginal vault, reaches just above umbilicus.

Operation.—28th February, 1899. Tumour a multinodular myoma blocking pelvis by a down growth which was slightly adherent. L. appendage not removed. No shock; no drain; recovery easy. Highest temperature, 101°. Highest pulse, 132—this rate for a few hours only.

No. 3.—L.B., thirty-one; married four years; never pregnant.

Symptoms.—Great difficulty in micturition for four years past; leucorrhœa, loss of flesh. Has not menstruated for three months past, previously menses regular, but not much in quantity.

Physical Signs.—Pale, wasted. A hard, uneven, movable tumour reaches just above umbilicus, not depressing vaginal vault.

Operation.—27th April, 1899. Both ovaries transformed into cysts the size of small oranges. Uterus the seat of a multinodular myoma. No drain; no shock. Operation lasted only half-an-hour. Urine was very scanty next day, and contained pus and albumen.

Death took place on sixth day from convulsions and coma with total suppression of urine, which had not been passed more than once in twenty-four hours since operation.

Post-mortem by Drs. Jamieson and Isbister showed field of operation perfect. No peritonitis, both ureters greatly thickened and slightly dilated. Bladder contracted and walls much thickened; both kidneys much undersized and very granular. R. pyonephrotic, L. hydro-nephrotic, very little renal tissue remaining in them. A careful urinary analysis before operation must have shown that the patient was dying of kidney disease, and that operation was thereby contra-indicated.

No. 4.—Mrs. C.G., forty-six; multipara.

Symptoms.—Losing flesh, great weakness. Menses very prolonged, sometimes continue from one month to another. Much leucorrhœa. Pain in lower abdomen, extending down legs. These symptoms have existed for two or three years past.

Physical Signs.—Wasted, anæmic, legs enormously swollen. Urine sp. gr. 1005; trace albumen. First cardiac sound weak and accompanied with a soft murmur. Uterus is merged in a soft œdematous tumour reaching to within 2 inches of umbilicus. Patient was kept lying down for five weeks and treated by cardiac tonics, massage, &c.

23rd May, 1899.—Operation, preceded by subcutaneous and rectal saline injections, hystero-myomectomy by Watson's method. Both appendages removed with the tumour. No shock. Highest temperature 100·8°, but pyrexia continued for five days and recurred on thirteenth day, when it lasted another five days, and was accompanied by pyuria. Possibly small abscess burst into bladder. Highest pulse rate, 96. Patient left private hospital feeling well, with urine clear, five weeks after operation. She reported herself three months afterwards as being much stronger and having gained flesh.

No. 5.—B.H., aged thirty-eight; two children; last twelve years ago.

Symptoms.—Excessive and frequent menstruation, with much dysmenorrhœa; has noticed a tumour in the lower abdomen for the past twelve months; it has steadily increased in size; for six months past has had increasingly frequent and difficult micturition. On several occasions there has been retention of urine; is losing flesh.

Physical Signs.—A hard fixed tumour fills pelvis and extends upwards to level of iliac crests. The entire vaginal vault is depressed, the cervix obliterated.

Operation.—June 1st, 1899. The tumour proved to be a cervical myoma with the body of uterus unaffected and perched on top of tumour. More than half the tumour was developed sub-peritoneally, and had to be enucleated before the mass could be delivered. There being no cervix, the operation was necessarily a pan-hysterectomy. Recovery was very easy, although the temperature went to 103° for a few hours after the gauze drain, which was perhaps unnecessarily used, had been withdrawn. Highest pulse rate, 100.

No. 6.—W.M., aged thirty-three; one child, five months old; labour easy.

Symptoms.—Has noticed a tumour in the lower abdomen since the third month of pregnancy; cannot lie except on back, as tumour rolls about and causes much pain; sense of bearing down and oppression on standing; tumour is increasing in size.

Physical Signs.—A hard, movable, slightly uneven, non-tender tumour occupies the lower and central abdomen. P.V., the uterus is drawn up and closely incorporated with the tumour.

Operation.—8th June, 1899. Hystero-myomectomy, Watson's method. The R. appendage was left. On section the tumour presented the appearance of brain matter, and contained several large cavities (myxomatous degeneration). Easy recovery. Highest temperature, 100·6°. Highest pulse rate, 112.

No. 7.—E.P., aged forty-five; single; never pregnant.

Symptoms.—Profuse and prolonged menstruation. Dull pain in lower abdomen. Great difficulty in micturition for a year past. Often retention of urine requiring catheter.

Physical Signs.—Well nourished. A hard, fixed tumour fills true pelvis and depresses vaginal vault to within 1 inch of ostium vaginæ. Cervix obliterated and high up behind pubes. Tumour extends upwards halfway to umbilicus.

Operation.—15th June, 1899. Tumour consisted of a myoma growing from R. border of cervix incarcerated in pelvis and firmly adherent to pelvic peritoneum and rectum. Several nodules are seen in the uterine body. Tumour delivered with difficulty and only after all the cardinal points on L. had been ligatured; R. appendage not removed. Gauze drain used on account of extensive oozing from raw surface in pelvis. Easy recovery. Highest temperature, 100·2°. Highest pulse rate, 108.

No. 8.—M.G., aged thirty-nine; multipara.

Symptoms.—Menses too frequent and excessive for two years past Has noticed a lump in the R. inguinal region for the past six months, which has steadily increased in size. Has lost more than a stone in weight during past six months.

Physical Signs.—The uterus is merged in a hard, irregular, considerably fixed tumour, which depresses anterior half of vaginal vault and extends upwards half way to umbilicus.

Operation.—22nd June, 1899. Hystero-myomectomy by Watson's method, longest diameter of the tumour is transverse. The bladder is drawn up considerably over anterior surface. No shock. Highest temperature, 99·2°. Highest pulse rate, 96. Easy recovery.

No. 9 —B.M., thirty-seven; single; never pregnant.

Symptoms.—Menses too frequent and excessive for past two years. Micturition unduly frequent. Pain in R. inguinal region. Has lost flesh and feels very weak; unable to work.

Physical Signs.—Uterus merged in hard, very irregular tumour, reaching more than halfway to umbilicus; irregular nodules project into posterior vaginal fornix.

Operation.—20th June, 1899. Hystero-myomectomy, Watson's method; R. appendage not removed. Firm adhesions in Douglas's pouch. No shock. Specimen a multinodular myoma, very little uterine tissue remaining. Easy recovery. Highest temperature, 100°. Highest pulse rate, 114.

No. 10.—Nellie S., thirty-two; married; never pregnant.

Symptoms.—Much pain in lower abdomen, especially when lying down. Noticed a tumour in this situation three years ago, which has steadily increased in size since.

Physical Signs.—Very hard, irregular tumour, closely connected with uterus; extends upwards nearly halfway to umbilicus.

Operation.—27th July, 1899. Hystero-myomectomy, Watson's method. Bladder drawn up over anterior surface of tumour. Both appendages adherent. Left round ligament covered in by adhesions, and not discovered until traced out from int. abdominal ring. R. appendage not removed. No shock. Easy recovery. Highest temperature, 99·2°. Highest pulse rate, 108.

No. 11.—Mary K., thirty-five; single; never pregnant.

Symptoms.—Menses excessive and painful. Pain and tenderness in R. side of abdomen, in which situation she noticed a lump three years ago which has very rapidly increased in size for the past six months.

Physical Signs.—Patient very stout A hard, irregular tumour depresses the R. and post-vaginal fornices, and extends upward nearly to the umbilicus.

Operation.—27th July, 1899. Hystero-myomectomy, Watson's method. Specimen consisted of two large outgrowths and several interstitial growths. L. ov. transformed into cyst size of hen's egg. R. appendage fairly normal, not removed. No shock. Easy recovery. Highest temperature, 100°. Highest pulse rate, 112.

No. 12.—Edith McG., thirty-five; married two and a-half years; no child, no miscarriage.

Symptoms.—Noticed lumps in the abdomen shortly after marriage; these have increased in size, and there has been considerable abdominal pain on exertion. Ten days ago (28th August) was seized with very severe pain in the lower abdomen, which lasted several hours. Menses regular and profuse up to 18th May, since which the flow has been absent.

Physical Signs.—Thin; pulse weak; an irregular growth occupies abdomen and reaches 2 inches above umbilicus. It is very hard and nodular in parts, but to left of centre feels cystic P.V. The inlet of pelvis is blocked by the hard fixed base of tumour, which can be in no degree pushed up. The cervix lies high up to the left, and is flush with the tumour. In the right kidney pouch there is a cyst the size of a large orange, somewhat movable, not tender; the colon is distinctly in front of this cyst, which was consequently taken to be a hydronephrosis, the diagnosis of the other tumour being "pregnancy complicated by myoma blocking pelvis and rendering delivery at term *per vias naturales* impossible."

Operation.—7th September, 1899. The cyst in right loin proved to be ovarian, lying behind and outside of colon, thus simulating renal cyst. The tumour consisted of the uterus gravid about the fourth month, and the seat of a multinodular myoma with extensive sub-peritoneal development anteriorly and laterally. No shock. Recovery easy. Highest temperature, 100·2°. Highest pulse rate, 100.

With the exception of No. 4, all the cases were operated upon in the Sydney Hospital. There was one other, in addition to those mentioned in the list, which also recovered, but the notes have, unfortunately, been mislaid.

I present photographs of the last eight growths; for these I am indebted to the senior resident medical officer of the Sydney Hospital, Dr. Herschel Harris. Prior to this it had not occurred to us to preserve the appearances of the growths in this way.

From the photographs and the descriptions, it is evident that most of the growths were complicated and difficult, and that a method of operation which gives ten consecutive easy recoveries in such conditions is one worthy of confidence.

THE SURGICAL TREATMENT OF UTERINE FIBRO-MYOMATA.

By E. T. Thring, F.R.C.S. Eng.,
Surgeon to Prince Alfred Hospital, Sydney.

I do not propose to attempt a review of the whole subject, but rather to place before you shortly the result of my own recent personal experience in the treatment of fibro-myomata of the uterus.

It will not be unprofitable to consider briefly the history of the subject. The first authentic case recorded in England of a deliberately planned operation for the removal of a large fibro-myoma is that reported by Dr. Clay, of Manchester, who in the year 1843 removed the tumour and uterus together with one ovary and Fallopian tube, in order to relieve pressure symptoms caused by incarceration of the tumour in the pelvis. The operation was done by the abdominal route—the cervix was cut across—the vaginal portion being left. The patient recovered.

In 1878 Spencer Wells recorded a series of twenty-four cases in which the tumour only was removed, the uterus being left. The results were appalling. Fifteen patients died and nine recovered.

In 1880 he reported another series of ten cases treated "artificially." The results were infinitely better—three deaths and seven recoveries. Writing in 1882, the late Dr. Graily Hewitt sums up the situation, as it then was, by concluding that removal of the uterus and tumour was safer than removal of the tumour alone. This observation refers to cases which could only be dealt with by abdominal section.

Twenty years ago the surgical treatment of fibro-myomata by the vaginal route was, as far as manipulation goes, very much where it is to-day. The great difference now is that we endeavour to make our patients surgically clean. Of late years Péan, of Paris, pushed the surgical treatment of fibroids *per vaginam* to what appears to be its utmost limit in his operation of "morcellation."

In 1872 Lawson Tait first deliberately removed the uterine appendages in order to cause atrophy of uterine fibro-myomata, and to control uterine bleeding. An essential feature of his operation was not only the removal of the ovary but also of the Fallopian tube close to the uterus. I mention this particularly because in some cases, owing to the relation of the tumour to the uterus, this was impossible, at all events when attempted by the old method of transfixion of the broad ligament and the formation of bunched-up pedicle by a double silk ligature. Also, Tait himself admitted that his method was useless in those cases of single, soft, rapidly-growing, or "œdematous" fibro-myomata.

More recently we have, in a way, gone back to the older methods, where abdominal section is required. We perform the operation of mymectomy in suitable cases, saving both uterus and uterine appendages; or, where this is impossible, we do as Clay did, and remove tumour and uterus, dividing the cervix and dropping the stump into the pelvis. The difference in our death rate is due, probably, more to the adoption of anti- and aseptic methods than to increased manipulative skill on our part. There is, however, this one great advance, that now we deliberately seek for and secure the uterine and ovarian arteries as a preliminary measure, thereby avoiding the loss of blood which used to be so serious a danger during the performance of abdominal hysterectomy.

I will only refer to the extra-peritoneal treatment of the cervical stump after abdominal hysterectomy as deserving of condemnation. It and the use of the serre-nœud are never necessary, and can only be used by those surgeons who fail to understand the surgical principles and anatomical facts which should guide them in the operation of abdominal hysterectomy.

May I digress for a moment, and say also that it is necessary to reconsider the position of vaginal hysterectomy, as now usually done for carcinoma of either corpus or cervix uteri. Here, again, surgical principles, as well as anatomical facts, both normal and pathological, are being daily ignored. Instead of a comparatively bloodless and also radical operation, chiefly by the abdominal route, being done in those few cases of carcinoma uteri which we see early enough to justify extensive surgical interference, there is a blind continuance of the customary vaginal operation, which is based upon a persistent refusal to recognise certain simple anatomical and pathological facts.

So far I have only referred to the operative treatment of uterine fibromyomata. Therapeutic and dietetic treatment is much the same now as it has been for many years. Treatment by electricity was in use before the year 1886, but was then placed upon a more exact and scientific basis by Apostoli. Certainly the results reported by Apostoli, Keith, and others are remarkable. For various reasons, however, the electrical treatment of pelvic diseases, and particularly of fibro-myomata, has to a great extent been abandoned. My own somewhat limited experience of electricity has not been on the whole satisfactory, and undoubtedly it is absolutely useless in many of the cases which are most suitable for removal by abdominal section.

In dealing with any case of fibro-myoma which may come before us, we have first to remember that it is a benign growth, and that, therefore, unless there is undoubted evidence that it is adversely affecting our patient's health and comfort, we are not justified in suggesting surgical interference. We may safely say that many cases of fibro-myomata are best left alone. There still remains, however, a large percentage of cases in which operative treatment is called for.

The often-repeated statements that "fibroids never kill," or "they will disappear at the change of life," are untrue in many instances. Fibroids *do* sometimes kill, or they may so delay the "change of life" that the patient dies first as the direct result of the pressure of the tumour on important organs, or arrives at such a state of misery that one is justified in advising operation. Dismissing those cases in which no symptoms are caused, or where they are so slight that palliative measures will suffice, let us consider the indications which justify interference:—

1. *Bleeding*, which may be so excessive that the patient's life is endangered both directly by loss of blood and indirectly by reason of the intense anæmia produced. I can recall three patients during the past year upon whom I was obliged to perform abdominal hysterectomy for the removal of large fibro-myomata. In each case the patient had had most alarming syncopal attacks which directly threatened life.

2. *Pressure.*—Under this heading we may include *pain*, which is, in most cases, due to pressure upon the branches of the sacral plexus by masses incarcerated in the true pelvis. There may be pressure upon the bladder, rectum, or ureters. In June of this year I saw and operated upon a patient, æt. fifty-three years, who still had copious uterine bleeding, but whose chief trouble was retention of urine, necessitating the constant use of a catheter, and was caused by the filling up of the pelvis and the pushing upwards of the uterus and bladder by a large single fibroid growing from the posterior lip of the cervix. Pressure on the rectum was also causing troublesome constipation. In another case I saw the patient, first, six years before finally operating. At that time removal was advised, but refused. Six years after, the patient could no longer endure the misery she suffered, and applied for treatment. The mass had enormously increased in size, and projecting spurs between the layers of broad ligament on each side had so pressed upon the ureters that the left kidney had atrophied, and was little more than a fibrous mass, the right kidney being greatly hypertrophied, and the corresponding ureter being dilated to the size of one's forefinger. The sequel of this was that, although the operation of abdominal hysterectomy was successful, the patient died at the end of about seven weeks as the result of the kidney trouble. In this case there is very little doubt that had the operation been done years before she would have still been alive.

3. *Degenerative and Inflammatory Changes.*—I have put these two conditions together because they so frequently co-exist. In my own experience, cystic degeneration of a sub-peritoneal pedunculated mass—one probably of several nodules of the "multiple hard" variety of fibro-myomata—is the most common. When this cystic degeneration has attained a certain stage of development, it almost always appears so to interfere with the nutrition of the mass that there is a resulting localised peritonitis. The same result follows hæmorrhage into a fibroid. This degenerative change most frequently occurs in the single soft "œdematous" variety. I have never seen suppuration in connection with cystic degeneration, though a case has been recently recorded.

4. *Inflammatory Conditions not Primarily connected with the Tumour.*—There are three distinct sites of infection from which may arise a localised peritonitis, complicating the presence of a fibro-myoma. The most common of all is an infective salpingitis, the result of gonorrhœa or some post-partum trouble. Much less often the vermiform appendix is the starting point of the peritoneal inflammation, and so draws attention to the existence of a fibro-myoma which otherwise would have escaped notice. In other cases the peritonitis trouble has started from the sigmoid or upper part of rectum, possibly as the result of a superficial ulceration caused by the retention of scybalous masses in the large gut.

It is important to recognise these conditions, because if it becomes necessary to remove a large fibroid on account of pressure symptoms, pain, or excessive bleeding, extensive pelvic adhesions form a serious complication, and add greatly to the gravity of the operation. In one instance only I have had to deal with a pyo-salpinx complicating an abdominal hysterectomy for a large fibro-myoma. It was successfully dealt with by a modification of Howard Kelly's method.

In cases in which the pelvic inflammatory trouble attracts attention to a fibroid which otherwise might have gone unnoticed, it is wise, if possible, to deal with the tumour or tumours by myomectomy, so as to avoid the probability of a second abdominal section later. Failing myomectomy, various courses are open. We may leave the fibroids and let the patient take her chance of their never causing trouble, or Tait's operation of removal of the ovaries might be done; or, as Mr Bland Sutton has recently suggested, a conservative hysterectomy, leaving behind one or both ovaries. This latter course appears to me to be the best and most scientifically correct, for there can be no doubt that the ovaries exercise a much greater influence over the whole organism than does the uterus, and one cannot but feel that it is the organ which is pathologically affected which should be removed, and not sound ones; more especially when one considers the important changes which take place as the result of removal of both healthy ovaries. The immediate risk to the patient is hardly, if at all, increased by such a course.

5. *Fibro-myoma complicating Pregnancy.*—Under such conditions noninterference is usually the wisest course to pursue, unless the fibroid is so placed in the pelvis as to obviously constitute an obstacle to the passage of the fœtal head during labour; or, there is evidence that degenerative change—cystic, hæmorrhagic, or malignant—has commenced in the tumour. If either of these latter conditions exist, we shall probably do best for our patient by first emptying the uterus, and then removing the tumour, either with or without the uterus.

The influence of pregnancy on fibroids seems to vary; possibly, according to the relation of the growth to the uterine wall. In some cases I have certainly seen a marked diminution of the tumour during the process of involution of the uterus. In one instance where a tumour the size of a fœtal head could be demonstrated post-partum, the whole mass apparently disappeared within the course of two years. On the other hand, pregnancy sometimes appears to give an impetus to the growth of the tumour. Possibly this takes place most often in those tumours in which the muscular elements are more numerous relatively to the fibrous.

6. *Tubal pregnancy* complicating uterine fibroid is, I believe, but rarely met with. I have only seen one instance which it may be of interest to refer to in some detail. Mrs. G L., æt. thirty-six years; first seen by me in June, 1899; married ten years; one child, four years old; no miscarriages. Gave history of menorrhagia after confinement. Was seen by Dr. Worrall in 1897, and a diagnosis of uterine fibroid made. Since June, 1898, there have been symptoms of increasing intra-pelvic pressure—*i.e.*, pain, frequency of micturition, and constipation. In April, 1899, a severe illness was ushered in by an attack of acute abdominal pain, and from this time on the patient was confined to bed. Pain severe and continuous. In June, 1899, she was looking extremely ill and wasted; temperature ranging from 100° F. to 103° F. Abdomen somewhat distended—a solid mass could be felt rising out of the pelvis towards the left side and reaching higher than the crest of the ilium. On the right side was another extremely tender mass extending up almost to the level of the umbilicus. *Per Vaginam:* Pelvis filled by a smooth, solid, immobile mass, with a boss about the size of a normal fundus uteri projecting forward and displacing the bladder. A diagnosis of uterine fibroid with some accompanying degenerative change causing peritonitis was made. The question of tubal pregnancy was discussed, but dismissed as being improbable.

Abdominal section was decided on in spite of the patient's bad condition, because it was obvious she would die if left alone. On opening the abdomen a considerable amount of dark blood clot was encountered, and on separating the extensive adhesions a right-sided tubal pregnancy was demonstrated. The clot and tube were removed, and then the question had to be decided as to the immediate closure of the abdomen, or the removal of the tumour and uterus, for the latter was so incorporated with the tumour that it was impossible to remove one without the other. As the patient's condition, though bad, did not appear to be much worse than at the start of operation, the uterus and tumour were removed by Howard Kelly's method of trans-pelvic section, leaving only the vaginal portion of the cervix. The boss referred to above had interfered very considerably with the bladder, and, to my intense disgust, on lifting up the anterior peritoneal flap before suturing the peritoneum across the pelvis, I found a rent in the bladder which easily admitted three fingers.

The bladder wall was soft and œdematous, and the vesical mucous membrane was of a dusky purple colour. The bladder wall was stitched up with a fine continuous catgut suture, the peritoneal edges being brought together over the stump of cervix in the usual way. The abdomen was closed without drainage. A soft rubber catheter was passed into the bladder per urethram and left for three days. The operation took from start to finish just over fifty minutes. Recovery was complete, and about one month ago the patient called on me to report progress before returning to her home in the country.

7. *Ovarian Tumour complicating Fibroids.*—I will refer to two cases in which this condition existed. In one instance there was an ovarian cystoma the size of a football, with hæmorrhage into it, the result of a twisted pedicle; the other ovary contained a cyst the size of a turkey's egg. This state of affairs simplified matters, because, both ovaries being diseased, they were removed together with the Fallopian tubes. The fibroid was left, and is now undergoing atrophy. Two years and a-half ago it nearly filled the true pelvis; now it can scarcely be appreciated by bi-manual examination. The second case also was one of double ovarian cystoma, but complicated not only by the uterine fibroid but by extensive and old-standing pelvic adhesions, the result of previous double salpingitis. In this case the only thing to be done was removal of uterus, ovaries, and Fallopian tubes. The patient recovered well.

8. In one instance only during the past three years have I met with carcinoma of the body of the uterus co-existent with uterine fibroids. The uterus with appendages, together with the whole broad ligament close up to the pelvic wall on each side, was removed, so as to include as great a length as possible of lymphatic vessels. The pelvic glands were not enlarged. The result so far has been good, but sufficient time has not elapsed (only about three months) to say whether there will be recurrence of the carcinoma.

I have mentioned these various complications and symptoms of uterine fibro-myomata because some one or more of them have been present in each one of the series of twenty-three cases of abdominal hysterectomy for fibro-myomata to which I am about to refer.

I have notes of forty-four cases of uterine fibroids which have been under my care during the past three years. Unfortunately, the notes of other cases seen during this period, some of which were operated on and others not, have been lost or mislaid.

Of the forty-four cases, only six patients were unmarried, and in each of the six surgical treatment was necessary. In five, abdominal hysterectomy was done, the tumours being of large size—three were of the single, rapidly-growing, soft variety; the other two were instances of the multiple hard kind. One of these latter had had an abdominal section done two years and a-half before I operated, but nothing was removed, the abdomen being closed and the patient left more or less *in statu quô.* The sixth case was in a single woman, who, besides having a fibroid the size of a large orange, also had extensive pelvic adhesions, the result of double salpingitis. Both sets of uterine appendages were removed together with the fibroid.

The average of the forty-four cases was just over thirty-five years; the two oldest being each fifty-three years, the youngest thirty-five years.

Six cases were complicated by pregnancy. In two of these the tumour was not in evidence by reason of symptoms, but was discovered because the patients miscarried before the end of the third month. In the other four cases the pregnancy went on and was not interfered with—three being delivered safely, and the fourth, so far as I know, is still going on. In this latter case it would, I believe, have been wise to have emptied the uterus early in the pregnancy, and then after a short interval removed the fibroid. I think this because for the six months before conception there had been a rapid increase in the size of the tumour, and this had culminated in an attack of acute localised peritonitis on the right side, accompanied by severe pain and rise of temperature. Probably this meant that degenerative changes had commenced, for the tumour was a single one of large size. According to Tarnier, the presence of fibroids complicating pregnancy leads to a great increase in the post-partum death rate both of mothers and children.

The one case of tubal pregnancy has already been referred to.

Of the forty-four cases, abdominal hysterectomy was done in twenty-three. and in each one only after careful consideration of risks and possibilities. In three of the cases the abdomen had been previously opened by other surgeons with the intention of removing the tumour, but the operation had been abandoned. The patients subsequently came into my hands during their search after relief from pain and discomfort. The fact that, after having had one abdominal section done without benefit, they were ready to risk another is, I think, sufficient evidence that their troubles were not slight. All three recovered.

In ten of the twenty-three hysterectomies, there existed extensive pelvic adhesions, and in one a pyo-salpinx.

There was one death, which took place on the fifth day after operation, and was due to the gradual onset of intestinal obstruction. The seat of the trouble was the sigmoid flexure, and the cause was, I believe, due to the pressure of a well-developed sigmoid mesentery which allowed the gut to fall over the brim of the pelvis, thus twisting upon itself. This was predisposed to by the removal of the tumour, which had for so long filled up the cavity of the pelvis. Had I realised sufficiently early what was taking place, I feel sure the patient's life could have been saved. As it was, I interfered too late.

Of the remaining twenty-one cases in which hysterectomy was not done, four, in which the patients were pregnant, were left alone.

In four, which were complicated by the co-existence of extensive pelvic adhesions, enucleation of the fibroids, through an abdominal incision, was done. The adhesions were separated, uterus fixed in good position, and in one of the four the uterine appendages were also removed. In one case the cavity of the uterus was opened during the removal of a fibroid. A gauze drain was passed from the uterine cavity through the cervix, the uterine incision and abdomen closed, and no trouble resulted.

In one case the patient had serious valvular heart disease; she had also suffered from recurrent appendicitis and the presence of a fibroid the size of a cocoanut fixed low down in the pelvis, and apparently growing from the cervix. The vermiform appendix was removed, as well as the uterine appendages, the tumour being left. This course was taken in order to save time and shock at the operation. The patient recovered, and the severe bleeding from which she had been suffering ceased. Two were simple cases of polypi presenting in the vagina. One was ligatured, and the other twisted off. In one of these cases there is a larger mass fixed lower down in the pelvis which is already beginning to cause pressure symptoms. However, since the removal of the polypus the bleeding has ceased to be excessive, and the patient declines further treatment for the present.

In two cases ovarian tumours existed, and the ovaries were removed, the fibroids being left to atrophy.

One patient out of the twenty-one died. She had a sub-serous pedunculated mass growing from the left cornu. This was removed by abdominal section, and adhesions separated. She developed influenza within a week of the operation, and died of broncho-pneumonia. Of the six remaining cases, two are intramural fibroids, and have been curetted in order to give temporary relief to the excessive menstrual bleeding. Three are small sub-serous growths attached to the fundus uteri and need no interference, for the present at all events. The last is one of the very few cases in which I have been able to satisfy myself by continued observation that the multiple tumours are disappearing as the menopause progresses.

In the cases of hysterectomy the principle followed was to control the blood-supply by looking for and securing the ovarian and uterine arteries, and removing the uterus and tumour by Howard Kelly's method of trans-pelvic section, modified in some instances to suit the necessities of the particular case.

No drainage was used unless there had been an extensive separation of adhesions, and the raw surfaces could not be satisfactorily covered by peritoneum. In these cases a gauze drain was left in for twelve or twenty-four hours.

In the last few cases all ligaturing and suturing have been done with No. 1 and No. 3 catgut, or fine wallaby tendon, no silk being used at all.

The abdominal incision has been usually sutured in three layers, catgut or wallaby tendon being used for the first two, interrupted horsehair sutures for the skin and superficial fat. The first or deepest layer consisted of peritoneum and sub-peritoneal fat together with what posterior muscular aponeurosis existed. The second layer consisted of the anterior muscular aponeurosis (*i.e.*, the anterior layer of sheath of rectus).

Adopting Professor Watson's suggestion, I have of late avoided the inclusion of the muscular fibres in the sutures, thus preventing damage to the contractile tissue.

In any given case of fibro-myoma in which we have after due consideration decided to operate, our object must be to do it in as conservative a manner as is consonant with efficiency and safety to the patient. If hysterectomy be decided upon, the simpler the method the better. It is for this reason that Howard Kelly is deserving of our thanks. His method is simple, and founded upon sound anatomical knowledge. I do not mean that we should slavishly follow his directions in all cases, but the great point is to control bleeding by deliberately securing the ovarian and uterine arteries, and, if necessary, looking for and so avoiding danger to the ureters. With regard to the uterine arteries, they can always be found if looked for, cleaned, picked up, and ligatured as one would a radial or a brachial artery. There should be no blind picking up of bunches of tissue, with the hope that the vessel is included and the ureter excluded.

The bladder can always be stripped forward out of danger, or if, as happened in one of my cases, it is torn, the accident is not a serious one.

If the rectum is seriously involved by adhesions, the opening of the broad ligament above and in front and the securing of the ovarian vessels allow of the posterior layer of the broad ligament on the left side being left attached to the rectum and pushed back with it away from the tumour, so lessening the danger of tearing the rectal wall during the separation of adhesions.

Finally, are we justified by results in treating fibro-myomata radically? The answer is, I believe, decidedly, Yes; for with improved methods and technique the mortality is small, and in the successful cases the remote results are good and lasting.

The two specimens before you are fibroid uteri removed in the way advocated above, and are shown simply as types of the class of case suitable for radical treatment. I will ask Professor Watson to be kind enough to demonstrate them, as he was present when they were removed.

[Both Dr. Worrall's and Dr. Thring's papers were illustrated by anatomical specimens of the parts removed.]

DISCUSSION.

Dr. Foreman, instead of reading a paper, said he had not written anything about uterine myomata, and what he had to say would be short. He only intended to add to the very able and practical papers just read by Dr Worrall and Dr. Thring, the description of a simpler method of dealing with hysterectomy, suitable in cases of pan-hysterectomy where the uterus did not rise above the umbilicus. The way he always made it a practice to perform the operation was to open the vagina anteriorly and posteriorly, separate the bladder, and then open the abdomen. If the bladder had not been completely separated, this was finished; then he got the uterus suspended by the broad ligaments only, and with Jessop's forceps he put clamps on the broad ligaments on either side of the tumour, which could then be brought out quite easily without trouble. The bleeding was thoroughly controlled by clamps, so that, leaving his loops, the forceps could be taken out easily, and then each loop was tightened. The vagina was then closed in the usual manner. By adopting this method he was able to perform his operations in cases of interior myomata in a short time, and at the same time bloodlessly. One thing he would direct attention to. It was necessary in performing operations by this method to have the forceps made long and particularly strong. Jessop's forceps were by far the best, but to be used successfully they required to be toothed, and this would prevent the one blade from sliding over the other.

Professor Watson, referring to the several specimens on the table, said that for operative purposes the tumours could be divided into three classes:—No. 1: Those which were more or less pear-shaped with the cervix free. In these the uterine arteries were accessible, and accordingly they could be attacked by the operator from any side or any aspect. No. 2: The cervical development, in which the pelvis was filled up by the growth over which the bladder was spread like a cowl, and where the uterine arteries were more or less inaccessible. The bladder in front and the rectum behind would be compressed, and more or less obstruction caused. The third, or intra-ligamentous variety, was not represented by any of the specimens present, but in attacking such a variety the more accessible round ligament (and not the infundibulo-pelvic fold) should be cut across, the ovarian vessels thus exposed, bunched, secured, and cut free. The usual mushroom-like pedicle of the ovarian vessels would thus be transformed into a dimple. The rest of the operation would then be easy by the application of the side-to-side method of Dr. Howard Kelly. He had seen the President remove such a tumour, which, if attacked by the ordinary method from above downwards, would have imperilled the bladder, and possibly the ureters and vaginal plexus of veins, &c. In a case like that of Dr. Thring's, where both uterine arteries were more or less inaccessible, he would ask Dr. Thring whether he used a clamp as preliminary to ligation, because it would be dangerous to pass a ligature with an aneurism or pedicle needle in such a condition He could see, from the appearance of the specimen, that Dr. Thring had used a clamp. They knew the infundibulo-pelvic fold was not always recognisable as such—at least on the left side, where it was often merged in the meso-sigmoid. The specimen he now referred to (Dr. Worrall's) represented a tumour in which pan-hysterectomy was almost the only operation possible. He would not say the only one, because, in a similar tumour [shown] of Dr. Thring's, a complete pan-hysterectomy had evidently not been carried out as in Dr. Worrall's case. There had only been a thin ring of flattened-out vaginal cervix surrounding a concentrically dilated canal of the same dimensions as the top of the vagina. In both cases the pelvic floor, after the removal of the tumour, was practically the same.

Dr. Thring said, in reference to Professor Watson's remarks on Dr. Worrall's operations, that practically it was Howard Kelly's method.

Professor Watson, continuing, said it was not quite the same, because the round ligament, and not the infundibulo-pelvic fold was the structure first attacked.

Dr. Thring said the principle of the operations was directly the same. The whole principle was to expose our vessels clearly and not bunch them up as Professor Watson had so frequently condemned. It was essential that operators saw the vessels they had to manage; the other method showed a want of surgical principles. He maintained the simpler the method of performing the operation the better. They did not require to use clamps, and all that was needed was ligatures, forceps, and scalpel. Neither were silk ligatures required, and there was no need to leave behind a bag full of sutures.

Dr. Moore said he had not had the advantage of a connection with a gynæcological department in a large hospital like the readers of the papers. He could add little to what had been said, with most of which he agreed. His position had always been to follow Howard Kelly as closely as possible. Professor Watson's chief point was to divide the round ligament, and that was recommended by Howard Kelly in the original paper. One point he wished to mention with regard to Dr. Worrall's paper, and that was the question of myomectomy, &c. He had came across two peculiar cases in his experience. One was that of a young woman married about a year. She had a large tumour rapidly increasing in size, and slowly destroying the patient, who was then five months pregnant, and it was thought that it was a hydatid or an ovarian tumour, and as the patient could not go on he decided to operate. In the other case matters were complicated: there was a tubal pregnancy and a myoma about the size of the fist attached by a broad base to the uterus. After removal of the affected tube and ovaries, he removed the tumour by cutting into the uterine walls. In the great majority of cases the tumours were multiple, and he thought that the removal of a large number was not good practice, but that it was better to remove the uterus, perhaps leaving one or both ovaries as recommended by Bland Sutton and others. At the same time he was doubtful as to the wisdom of leaving an ovary behind, especially as he had not seen the bad symptoms that this procedure was adopted to guard against.

Professor Watson said with reference to the cases mentioned by Dr. Moore he would cite one which he had under his notice. A young German woman, about twenty-five years of age, was married in the eighth month of 1898. Three months afterwards she went to Dr. Way, in Adelaide, complaining of rectal and bladder symptoms. He opened the abdomen and removed a tumour weighing 1 lb. 11 oz., also eight satellite tumours, each being up to the size of a walnut, from the pregnant uterus. The next morning a fœtus was discovered in the bed with a wound in its shoulder from a needle-prick. She menstruated once after leaving her bed and conceived in February last, and returned to town in May, when Dr. Way put in a pessary for a month and removed it, when the pregnant uterus had risen out of the pelvis. She was then in the eighth month of pregnancy, and fourteen days afterwards she was delivered of a young German.

Dr. Foreman said the papers he had listened to of Dr. Worrall and Dr. Thring were both of a very high quality, and of great value. Of late years a great evolution had taken place in this section of medical science. At the time when he first commenced his practice, fibroids were always looked upon by medical practitioners as very dangerous, and would never be operated upon if it could possibly be avoided. He had seen many cases of fibroids developed through reticence on the part of the patient, instead of fully explaining her symptoms, and he wished to direct attention to this, because it had influenced him to a certain extent in giving advice in other cases. It was very difficult for any medical practitioner to determine how to advise patients in cases of fibroids under such circumstances. The experiences he had encountered were not always the same.

Some patients suffered from fibroids, and by some means went through their trouble with comparative comfort. In some cases, on the other hand, he would advise the patient to undergo an operation. One case which had most impressed him he had to deal with when he was first connected with the Prince Alfred Hospital in Sydney. At that time it was generally recognised that very little was known about abdominal surgery, and he had no assistant in the hospital to help him. Dr. Chambers was the only one who could be said to have acquired any practical experience in this branch of the profession, and for certain reasons his services were not available. The case he had to deal with was that of a woman with a large fibroid in the uterus. This caused her to bleed so much that there was no other remedy but an operation in his opinion, and he came to the conclusion that it would have to be performed. At the same time he felt sure that if he did perform the operation in her condition she would be unable to get over it and would surely die. So he hesitated, and kept putting the operation off from day to day. However, the woman sank lower and lower, and at last became so feeble that whenever he saw her he would say to himself, "I must perform that operation to-morrow." And he believed he would have done so, in spite of the risk, had he not happened to hear from the sister of the woman that she was very unhappy in her home life, and wanted the operation performed so that she should not get over it. Then he at once decided that her case was not a fit one to be operated upon. After keeping her under observation for some time longer, she returned to her home from the hospital, and there had further attacks. This condition of affairs continued until the woman's change of life came, and with this a complete change for the better set in, and the hæmorrhage stopped. No further serious discomfort was complained of, and the improvement had continued, until now his patient was a strong woman with only the very smallest of tumours affecting her. Another case of a similar nature he would briefly mention. The patient was suffering continually from a large fibroid which made her bleed very much. He intended to operate, but was unable to in the end, as she was also suffering from mitral disease. Other cases he had seen where the patients had gone through a long course of pain from fibroids, and they lived without operation. On the other hand, only in one single instance had he found death arise from the effects of fibroids. In all the cases he had mentioned, experience went to show that in the treatment of fibro-myomata a great deal must be left to the judgment of the practitioner. He had found it was always difficult to go upon one line of advice in such cases, because the conditions were never the same. In one case he had treated the woman was suffering from dislocated liver. Her ovaries had to be removed, and she also had adhesions—in fact, she was the greatest heap of misery he believed he had ever seen. When she came to him she told him she was always in pain, and she added, "But notwithstanding this, life is sweet with it all," and he did not operate. With regard to the use of electricity in these cases, which members of the profession advocated in many parts of the world, he would give them his experience. He had wasted a lot of money over this without obtaining any beneficial return. He had tried electricity in his own house as well as in the Prince Alfred Hospital, and in both cases with like results. In Prince Alfred Hospital he had a battery of 250 cells put up—quite sufficient power to give the method a very fair trial. In his own house he also had a 100-cell battery put up, and had given electricity a good trial there. But in none of the cases did he obtain by this method any marked benefit, excepting one, and in that case even the same benefit used to be derived from other measures. The extra-peritoneal treatment was a very good plan in the early days. It was not done now, but it marked a step in evolution and showed the advance that had been made.

Dr. Morton (Fitzroy) said: Speaking in reference to what some medical practitioners termed the terrible effects of removing the ovaries, he had removed a good many, and could not remember one woman who said she did not feel the same as other women after the removal of her ovaries. In the majority of very bad cases where the ovaries were cystic he had no doubt that their removal did not occasion any difference.

The PRESIDENT (Dr. O'Sullivan) expressed his indebtedness to Dr. Worrall and Dr. Thring for the papers they had contributed. He regretted that neither of them had gone into the classification of fibroids, because on that basis all the treatment depended. With regard to the treatment of fibroids, whilst in accord with what had been said by Dr. Worrall and Dr. Thring in their papers, and by some of those who had taken part in the discussion, he would not concede the point that the majority of fibroids did not demand surgical treatment. For their demanding it, there were many reasons. All medical men, although radical in their ideas, he found at times believed in conservation, and especially was this remark applicable to this question. The healthiest rule to be observed in dealing with fibroids was to keep the patient under observation, and if she showed any symptoms likely to endanger life, then there should be an operation. It was not to the present only the medical practitioner had to look, but he should also look to the possibilities of the future. Keeping the patient under observation was the main thing. The dangers of waiting in respect to fibroids were many. There was a species of degeneration of the heart which was supposed formerly to be connected with fibroids. Now they found this came from anæmia, the result of loss of blood, and cachexia. The *laisser faire* policy was a bad one for this reason alone. Fibroids, too, had an ætiological relationship to malignant diseases. No one would now have the hardihood to say this was not the case. Fatty liver, various kidney diseases, such as pyelitis, pyelonephritis, and hydronephrosis, were frequently due to fibroids. This being so, and the complications of fibroids being so dangerous, every patient should be observed, and whenever necessary, early operation was demanded. Each method of the treatment of fibroids had its advantages, and to rigidly adhere to one method or another showed want of skill and experience on the part of the operator. It was impossible for any one method to suit every case. Both Howard Kelly's and Schröder's methods were good in their way, but one method should not be applied to the exclusion of the other. He would like to have heard a little in reference to the classification of tumours, because it would have influenced him in his treatment of the situation. The soft œdematous variety of tumours still kept on growing after the menopause, and he had recently operated on a person at the age of sixty-five. The hard multinodular variety of fibroid did not continue to grow after the menopause, and in cases approaching the menopause, where a woman was not suffering with severe symptoms, it was justifiable to direct one's treatment to tiding the patient over this period. Myomectomy was fast gaining ground with the advanced school of gynæcologists, and would, he ventured to assert, be always performed in the future as the operation of election. He had operated himself in a great many cases. Just before he left home he operated on a woman according to the method adopted by Goelet, an American surgeon of distinction. He removed a large fibroid about the size of a child's head, employing utero-vaginal drainage by a strip of gauze through the cervix, and closing the uterine wound with chromicised catgut and Lembert's peritoneal sutures. Goelet recorded thirty-three cases of utero-vaginal drainage, and his success in all of them had been phenomenal The man who neglected function in any form did not aim at the highest ideal of surgery. They had not only to save life, but function had to be saved wherever possible. It was not right to abrogate a woman's function at a moment's notice when the organs could be saved. All advanced surgeons made it a practice to conserve function as much as possible. He had performed myomectomy on a pregnant uterus with success, and there is a case on record where as many as seven fibroids had been removed by myomectomy, and the woman still bore children. Professor Watson had told them that bunching up the tissues in the process of ligation was an unsurgical operation. In one of his cases where he applied a ligature close to the pelvic wall through the infundibulo-pelvic ligament he found he included the ureter. With regard to myomectomy, this was, in his opinion, the operation of the future.

Dr. Thring, in reply to Dr. Foreman: The method of vagino-abdominal section, the uterine arteries being secured from below—per vaginam—and the whole of the cervix being removed, was not so easy or so universally applicable as the method advocated above With regard to the use of clamps of any kind, the method seemed to be distinctly inferior in all ways to the deliberate picking up and securing of the vessels. The principle to keep in view was to *see* what is being done. Dr. Morton spoke of the "terrible effects experienced by patients after removal of the ovaries," and said this did not occur in Melbourne. Like him, he had never seen any terrible results, but still, if ovaries were normal and healthy, it did not appear to be right to remove them unless compelled to do so. Nearly all the points referred to by Dr. O'Sullivan had been dealt with either by Dr. Worrall or himself in the papers they had just read, and in the way in which Dr. O'Sullivan advocated. He would only instance the classification of the growths into the two chief clinical varieties—the single, soft, rapidly-growing, "œdematous" variety as distinct from the multiple, hard kind of uterine fibroids; also, the case referred to in which the cavity of the uterus was opened during myomectomy, and was drained by gauze into the vagina, through the cervix uteri. With regard to the operation of myomectomy being *necessary* in almost all cases of pregnancy complicated by fibroids, he would very much doubt it, and thought that in most cases it was *unnecessary*. One could not help remembering that, although much good surgery had been done in America of late years, still many American surgeons strenuously advocated immediate removal of the vermiform appendix, even during a *first* attack; and this almost universal application of myomectomy during pregnancy seemed to savour of the same undue haste.

Dr. Worrall, in reply, said he thought Professor Watson was wrong when he said that the cervical tumour which he (Dr. Worrall) showed could be removed by any other method than pan-hysterectomy. In the case mentioned by Dr. Thring it was evident the difficulties of the operation had forced him into the vagina, and thus the operation became a pan-hysterectomy, as in his own case; but in reality there was no practical difference in the technique.

Dr. Thring said, regarding the question of dealing with the ovaries, he thought the points had been answered by Dr. Worrall and himself. Regarding myomectomy in pregnancy, he did not think, in the majority of cases, interference was necessary. He also thought that the American surgeons ran to extremes.

THE SAVING OF BLOOD IN GYNÆCOLOGICAL OPERATIONS.

By Archibald Watson M.D. Paris, F.R.C.S. Eng.,
Professor of Anatomy and Physiology in the Adelaide University.

Mr. President,—

Dr. Byrne, the courteous secretary of your Section, has paid me the compliment of inviting me to make some remarks on the saving of blood in gynæcological operations within the pelvis, on the principle, I presume, that "spectators see most of the game." Often, however, I have only appeared on the scene after the game was quite over, and in my opinion this latter factor should make any comments from one outside of your ranks appear less trivial than they otherwise might.

I am not here to enumerate all the accidents from hæmorrhage I have been personally acquainted with, even if I could remember them all individually, but I can say that most of them would have been averted had the operators appreciated at its true value the fact that the uterus and adnexa are supplied by four miserable little arteries out of all proportion in carrying capacity to the immense veins which return their blood to the right heart, and that with the arteries these veins form an unbroken vascular circuit (the utero-ovarian) in which interruption of the blood current at any one point fails to deprive any portion of the uterus and adnexa of its proper blood supply; whereas interruption at any two points blocks definitely the in-and-out flow of blood to all parts supplied by the excluded segment of the circuit. Interruption of the inflow, by blocking the arteries at two points and leaving the veins intact, permits the blood circulating in the part about to be removed to find its way back into the general circulation, where it is of much more use than imprisoned in a museum specimen by ligatures.

We may regard hæmostasis as either surgical or spontaneous; with the latter may be classed medical hæmostasis by the internal administration of drugs.

On the present occasion we have only to consider the blood supply of the abdominal wall in the mid-line of the body, and the utero-ovarian vascular arch which supplies the uterus and adnexa. In addition, we may be called on to deal with bloodvessels of new formation, as contained in adhesions.

It is in the abdominal incision that spontaneous hæmostasis most frequently has its indication, yet how often do we see more clips hanging on to the cutis fat and muscle than would suffice twice over to control the whole blood supply of the pelvic viscera?

The time thus lost before arriving at the real seat of war in the pelvis is too often not compensated for by the saving of so little blood as that encountered in a true linea alba incision where the cross branches of the superficial and deep epigastrics almost reach vanishing point. Except where diastasis of the recti muscles has been produced by repeated pregnancies or the pressure of a big tumour, even experienced operators in seeking the mid-line first open the sheath of a rectus muscle, but, knowing that the less curved of the resulting aponeurotic edges points to the linea alba, they are not an instant in doubt as to the mid-line and the urachus, which latter they leave with any omphalo-pubic blood courses on the side away from that in which they anticipate most difficulty in dealing with the pelvic contents later on. I have often seen the muscular branches of the deep epigastric, and sometimes that vessel itself, tapped by operators, who quote some great name as advocating entering the abdominal cavity through muscle and stripping adhesions, guided by what they term their "tactus eruditus."

I have seen the parietal serosa in hydroperitoneum stripped in the belief that general adhesions were present, and I have also seen the organised parietal exudate of an encysted hydrops digitally detached as far as the external iliac artery in the same belief.

By using a scissors instead of the sharpest knife in the separation of close adhesions to bowel, the muscularis of the latter is often laid bare, and, as it cannot be satisfactorily covered, it continues to ooze till spontaneous hæmostasis is established in spite of sponging and the application of styptics. If a sharp knife is used and its edge directed towards the surface of the part to be removed (ovarian cyst, uterus, &c.) a collar of adhesion can be left to cover over the bleeding surface of bared muscularis. In wire-drawn adhesions, the scissors has its use, because it is a loss of time to strip adhesions and later on have to remove redundancy and ligate afresh. Such loss of time mostly occurs in operations on post-operative hernia in which omental processes are adherent in sacculi.

In twisted ovarian pedicle of long standing, where the adherent omentum has come to be the only source of blood supply, the long stretches of newly formed vessels (caput medusæ) are sometimes needlessly stripped from the cyst, and then tied and cut off afterwards.

Some operators perform hysterectomy or ovariotomy on the principle that all adhesions must be separated, and the tumour delivered before hæmostasis of its natural blood supply can be attended to—whereas an ovarian pedicle can often be hooked up on the finger, and its blood supply blocked with two clips and cut free before anything else can be done, after which the cyst, like an empty Champetiere de Ribes bag, can be withdrawn from the abdomen, and existing adhesions dealt with as they come under visual control.

When a myomatous uterus is rotated, time is saved by leaving its delivery till after the ovarian vessels of the more accessible side have been secured and cut free. This is easily accomplished by drawing the corresponding round ligament forwards and double clipping and dividing it an inch from the uterus, thereby producing a rhomboid opening in the anterior blade of the broad ligament, and thus exposing the utero-ovarian plexus like a big, freely movable varicocele.

It is just the same in an ovarian cyst without a pedicle (or, for that matter, of any intra-ligamentous development)—the same vascular plexus is exposed, but the excluded segment, which includes only that portion between the uterine cornu and the pelvic wall, is necessarily less than where dealing with a myomatous uterus in which it comprises the para-uterine portion of the vascular circuit as well. Many operators transfix an ovarian pedicle (the blood supply of which is the vascular segment stretching between the uterine cornu and the pelvic wall) at no particular anatomical point, and interlock or apply a Staffordshire knot of thick silk, which comes to the same thing; they cut their own fingers in trying to tighten the ligature which before many hours is quite loose.

Other operators elbow the round ligament upwards and transfix below it; others again look for a clear space behind the round ligament; and it is often only after meeting several times with disaster that they cease to interlock or adopt the supplementary ligature of the infundibulo-pelvic fold as far as I know first advocated in Australia by yourself, Mr. President—or, better still, they follow Professor Penrose and ablate the tubes and ovary (ovarian cyst) between two ligatures passed by transfixion, and topsew the resulting opening in the broad ligament. Where the left infundibulo-pelvic fold is spread out and more or less merged in the meso-sigmoid, the ligature by transfixion of the pedicle comes too near the bowel and the ureter, and I possess specimens proving that obstruction in one or both of those tubes is favoured thereby.

Surgeons interlock because they fear hæmorrhage if the loops draw apart—as a matter of fact, there are no vessels in that part of the broad ligament to bleed. What often does happen, however, is that one loop pulls the other off (usually it is the outer one which slips off the infundibulo-pelvic fold); and the surgeon has to face the sorrowing widower and tell him his wife sneezed, and explain to his own *confrères* that the pedicle was œdematous.

The disadvantages of the supplementary ligature on the infundibulo-pelvic fold (one of which is that it does not command the uterine artery) are far outweighed by its advantages, which, however, are equally obtainable by separate ligation of the ovarian artery on the face of the stump.

The disadvantage of a supplementary ligature at the uterine cornu by transfixion (whether the round ligament is included or not) is that veins are pricked and too much tissue embraced, but, as in the case of the ovarian artery, it is easy to draw the uterine artery from the face of the stump and ligate it separately. I once performed an autopsy, on a case which died of sepsis, where the outer loop of an ovarian pedicle had been dragged off by the inner one, but where no drop of blood had been lost because of a supplementary ligature on the infundibulo-pelvic fold. I have seen the loop at the uterine cornu come off before the abdomen was closed, and a friend of mine has never made another pedicle since he found the pelvis full of blood (from slipping of the outer loop) when he was removing the last sponge. Both the above cases would probably have bled to death if the accident had occurred after they had been taken off the table. All these accidents can be prevented by cutting the round ligament and thus opening the anterior fold of the broad ligament and exposing the ovarian vessels. (This applies equally to the uterine artery in the whole of its course.)

What applies to an ovarian cyst applies equally to tubes and ovaries, and the easiest way to remove matted tubes and ovaries is to tackle their blood supply from inside the broad ligament and attend to their adhesions to surrounding parts afterwards—temporary, and at the same time preliminary, hæmostasis with Kelly's artery clips saves time in all these operations.

In consequence of cicatricial process, the ovarian artery recedes from the ovary in pachy-salpingitis, and if the surgeon likes he can tear the tube and ovary off in that condition It is done by first double clipping and cutting the isthmus of the tube so as to expose the covered-up ovarian ligament, which in its turn is double clipped, divided, and the ovary with the tube torn from the meso-salpinx—an anabranch of the ovarian artery always spirts, but is easily secured (pinched). In the normal adnexa of woman and the mare, the tube and ovary refuse to come cleanly away as they do in pachy-salpingitis, or in the normal tube and ovary of the cow. I have elsewhere discussed hæmostasis in abdominal hysterectomy (*Australian Medical Gazette*, 20th September, 1899), but I may repeat here that you can secure the uterine artery in any part of its course by dividing the round ligament and enlarging the resulting incision in the anterior blade of the broad ligament, and asking the assistant to pull the uterus to the opposite side.

If the pelvis fills with blood because you have inadvertently pricked or cut veins on its floor, ask the assistant to pull the uterus towards the ensiform cartilage, and place his finger on the abdominal aorta and another on the inferior mesenteric artery. The inferior vena cava must not be compressed.

In the same manner, to avoid inundation in cleaning out the pelvis in abdominal hysterectomy for cancer, place the patient in the Trendelenburg position, and after tying off both round ligaments and infundibulo-pelvic folds, and cutting them free with the broad ligaments and utero-vesical fold of peritoneum (after placing clips on the uterine cornu to control reflux hæmorrhage), pull on the obliterated hypogastric artery, which is easily found by looking at the umbilicus. This draws the anterior division of the internal iliac artery away from the veins and permits of its safe ligation, say, behind the origin of the sciatic.

The uterine veins then carry their own blood and that of the previously ligated ovarian veins back into the general circulation. In one case where Dr. J. A. G. Hamilton removed the uterus and tied the main stem of both internal iliac arteries behind the origin of the gluteal, the result was startling; the progress of the disease appears also to have been checked for the time being (it is now ten months ago). The collateral circulation of the bladder and lower rectum was soon re-established.

In conclusion, transfixion and mass ligature with necessarily thick silk is responsible for hæmorrhage, because of the liability to either slip or to permit of the artery retracting and producing a hæmatoma inside the broad ligament.

Venous hæmorrhage is the rule where a pedicle needle is used; fortunately it is not often deadly. This is one reason why ether should be preferred to chloroform, because it makes the veins swell up, and thus renders them easier of isolation.

More especially in abdominal hysterectomy for myoma is time and blood saved by clamping the uterine, if not the ovarian vessels (artery and veins), with Howard Kelly's clips, and replacing them with ligatures after ablation of the uterus.

In hystero-myomectomy the uterus should be pulled up, and absorbable ligatures (raw tendon) should be used. I have seen a tumour weighing 1 lb. 11 oz. removed through a transverse incision from a pregnant uterus, the cavity of which was inadvertently opened and the fœtus killed. The woman is now in the eighth month of a second pregnancy.

In Cæsarean section, pulling the uterus up, or any other manœuvre to prevent hæmorrhage, will tend to deprive the child of its blood supply; and in this, temporary hæmostasis should give way to lightning rapidity of operation. away from the placental attachment, if possible.

In conclusion, I must thank you for the attention you have given me, more especially as much I have said must have appeared very commonplace to you as experienced gynæcologists.

DISCUSSION.

Dr. Worrall (Sydney) said he felt it would be presumptuous on his part to criticise such an able and valuable paper as that they had just heard from Professor Watson. He must express his opinion that it was the most valuable paper that had been read, or that would be read, in their Section. Professor Watson, they must have all noticed, very modestly gave all the credit to other surgeons in the marked advance made of late years in this branch of surgery, although it was well known to them all that it was he to whom the credit was due for the advances that had been made in controlling the hæmorrhage which took place in pelvic operations. His original investigations and the methods he had adopted, and taught others to adopt, had been the means of saving thousands of lives. There was one point, however, in the professor's paper he felt that he had to take exception to. He referred to the remarks Professor Watson had made of packing sponge after sponge in the patient during an operation With regard to this, he must say that he (Dr. Worrall) attached the greatest importance to the operator not seeing the smaller intestines whilst he was performing a pelvic operation. He felt assured that the less seen of the smaller intestines the better. This was the advantage of putting patients in the Trendelenburg position. When a doctor had his patient placed in the way Trendelenburg recommended, it was the best position to keep the smaller intestines out of view, and he simply put in sponge after sponge until this result had been attained and the intestines were entirely isolated from the field of operations. There was nothing further that it was necessary for him to say about a paper which was likely to prove so useful to all who read it.

Dr. Thring (Sydney) said he felt exactly in the same position as Dr. Worrall regarding criticising Professor Watson's paper. The professor, with his vast knowledge and ability, knew far more than he did; and like many more, no doubt, whom Professor Watson had instructed, he felt he was only doing right in acknowledging his indebtedness to the professor for what he had taught him. He had been pleased to hear him say that the best course to adopt was to perform a clean-cut operation in the pelvis. In his experience he had found this to be so in nearly all his cases, and, when one knew his anatomy thoroughly, by using the knife he was likely to do far less harm than by bruising and tearing.

Dr. Moore (Melbourne) said that coming from Victoria he did not think it was fair at all that Sydney should be singular in according to Professor Watson all the praise; the medical profession were indebted to him as much in the colony of Victoria as they were to him in New South Wales or South

Australia. In fact, the professor started practice first in South Australia, then he came over to Victoria, and from that colony went on to New South Wales. When he (Dr. Moore) was performing any operation in Professor Watson's presence he must say that he did not like to see him bring out his note-book, for he knew then that the professor had seen something with which he did not agree. With regard to Dr. Worrall's remarks regarding seeing the smaller intestines, he doubted if the doctor was correct in what he had said. When Dr. Worrall spoke of packing the abdominal cavity with sponge he was quite right. But one sponge was quite sufficient to hide the intestines from view; so, in his opinion, Professor Watson was quite correct in declaiming about sponge after sponge being inserted. Dr. Thring had said the use of a knife was better than tearing or bruising; but he would say, in addition, that in using a knife he preferred the sharpest knife obtainable. In such a case the operator was likely to do less harm with this than with blunter instruments, or than with scissors, in places where he could see what he was doing. That was the great advantage of the Trendelenburg position—that it enabled one to see what he was doing during an operation. In making these few remarks he had risen more than anything else to express his indebtedness to Professor Watson——

Professor Watson: Quantum.

Dr. Pinnock (Ballarat) said he thought that a great deal of the tendency of the intestines to get in the way was due to the condition of the patient. If every one gave the patient a short preliminary course of nux vomica and ox gall, they would find that the intestines collapsed in a most gratifying way.

The President (Dr. O'Sullivan) said he was in the same position as his colleagues who had just spoken. He rose to express his great gratitude to Professor Watson for the many kind acts the professor had shown him in educational matters of anatomical surgery. In listening to Professor Watson's excellent paper, what first impressed him was his reference to the number of superfluous clips that it was customary for operators to use on the peritoneal wall. He thought it might be said that it was admitted generally by every surgeon that a great deal of time was wasted in operations in dealing with this wall. This was quite unnecessary, as there was no occasion to notice any infinitesimal amount of bleeding that might occur from small veins whilst performing peritoneal operations. The first duty they had to attend to was to find out where the supply came from, and then cut it off effectually by tying up the vessels—a matter there should be no difficulty about. With Dr. Moore he had to admit the warm feeling of discomfiture fall over him when he saw Professor Watson bring out his note-book during one of his (the president's) operations, because he knew at once that there was something wrong. With regard to packing sponges, it appeared to him from their remarks that Drs. Worrall and Moore were quite in accord. He did not infer from what was said that Dr. Worrall meant heavy sponge packing. The danger of that would be the injury to the peritoneum over the bowels. Such a state of affairs would be likely to lead to very serious trouble; and on that account the less the bowels were touched the better. But it was extremely improbable that any man would be likely to undertake an abdominal operation unless the bowels first had been properly treated. Regarding the relative use of the knife and scissors, he might say that in all cases he considered it was better to use the more delicate instrument, and he thought all would agree with him in this. Professor Watson's paper was just the sort of one wanted in a section dealing with midwifery and gynæcology. There was no question threw juniors off their balance so much as the question of hæmorrhage. When an operator knew the anatomical parts of the human body thoroughly, there was no difficulty, for he simply cut off the supply and the bleeding stopped. The subject of hæmorrhage *par excellence* came before them in every-day treatment, and this paper was likely to do them the greatest good.

TWO CASES OF DOUBLE UTERUS.

By R. D. Pinnock, M.D., C.M., Glasg.,
Hon. Surgeon, Ballarat Hospital.

No one will dispute the extreme rarity of these cases—at any rate, in Australia. One of the two which I now bring under your notice is the only one encountered by me in twenty-five years' continuous professional work. The other (for the notes of which I am indebted to the courtesy of Professor Watson) is the only one met with by Dr. Way, of Adelaide, during a long and busy practice. Dr. Way has been kind enough to ask me to include his case in this communication. At the last Congress held in Dunedin, New Zealand, the President, Dr. Batchelor, narrated two cases which were all he had seen or heard of during his very large experience of practice in New Zealand.

The notes of Dr. Way's case are appended at the end of this paper.

My patient, Miss S., æt. fifteen, a boarder at a convent school, is a well-grown, somewhat anæmic girl of studious habit of mind and gentle disposition, according to her teachers. She first menstruated at the age of twelve and a-half years. I was first called in on the 14th April, 1899, in consequence of the appearance of a purulent, malodorous, greenish, vaginal discharge. On questioning her, she said that she had missed her periods from January to March, 1899, without suffering from local or any other pain or discomfort. A digital examination was made under chloroform, and what appeared to be an enlarged, rounded, and semi-fluctuating uterus was immediately met with by the examining finger. The distention was most marked immediately behind the bladder and somewhat to the left of the median line. The cervix was quite obliterated, and the os could only be just reached with the tip of the finger high up and to the right. Pressure on the abdominal wall above pubes and half-way to umbilicus caused a free and immediate increase of the discharge. There was no admission of any pain after this examination. Up to this point I had not recognised the true nature of the case, but, realising that there was a collection of stinking matter in the uterus, I determined to dilate the cervix, wash out, and seek for the cause. The patient was accordingly put on gr. $\frac{1}{10}$ calcii. sulphid. every two hours, and a Blaud's pill three times daily after meals, and both vagina and rectum were thoroughly douched night and morning with a weak solution of Condy (warm).

On the 21st April she was again put under chloroform. Owing to the obliteration of the cervix and the high position of the os, much difficulty was experienced in hooking it down. When the os was brought into view it was discovered that the discharge was escaping from a small circular opening—about the diameter of a No. 5 catheter—in the cervical tissue half-an-inch to the left and somewhat in front of the normal os. The sound passed 3 inches into the right uterus, and 4½ inches into the apparent abscess cavity or left uterus, in a direction between the bladder and the right uterus. A three-bladed Sim's dilator was inserted into the opening, and on expanding the blades it was observed to split or tear laterally instead of merely stretching; and at least 6 oz. of stinking pus welled out. There was some bleeding at first from the tear. A Bozeman's tube was then inserted, and the cavity thoroughly irrigated with a warm 1 per cent. solution of boric acid. No drain tube was inserted. The patient suffered no pain after the operation. A vaginal douche of warm weak Condy's fluid was used night and morning, and the sulphide of calcium and Blaud's pill continued. In a week the discharge had ceased. No swelling was to be felt over the pubes or per vaginam, and she returned to her school work. She has continued to menstruate regularly up to the present time, and except for a mild attack of rheumatic arthritis of the left ankle joint, necessitating a week's rest and treatment during the latter part of August, she has enjoyed good health.

The double uterus is one of the forms of malformations of the uterus, of which Pozzi gives the following varieties:—

I. Absence and rudimentary development, in which the points of difference are of no clinical importance.

II. Single-horned uterus; developed from a single Müller's duct, the other one being atrophied.

III. Double uterus, where Müller's ducts have not become fused together, or are only united partially, each one being completely developed independently of the other.

IV. Fœtal or infantile uterus, where the uterus, being completely developed as regards its general shape, remains stationary, while still preserving the proportions and almost the dimensions which it had at birth.

The case related above comes under the third variety—viz., double uterus—which the same authority divides into—

1. The bicornuate uterus: In which the axis of the two halves of the uterus go off in different directions.
2. The bilocular uterus: The characteristic feature of which consists in the perfectly normal external configuration of the uterus, occurring with a cavity separated into two parts by a median septum, either complete or terminating more or less low down (uterus subseptus), or having a number of perforations in it.
3. The didelphous uterus: Where there are in reality two uteri separated as far as the fundus inclusively, each segment presenting, the appearance of a complete uterus.

Barnes says: "As regards menstruation, conception, and labour, the double uterus behaves as follows:—The constituent rudiments of uterus bipartitus are capable of conception. An accumulation of menstrual blood in the cavity of such a rudiment with dilatation of it may happen. Where such a rudiment is connected with a one-horned uterus by a perforated bond of union, it is capable of conception, and so even when it is solid. The gestation terminates as in tubal gestation by rupture, within three or four months."

"The uterus unicornis and the uterus bicornis, as well as the bilocularis, are capable of conception. In the two last, repeated gestations occur interchangeably—sometimes in one, sometimes in the other uterine half. When conception takes place in one half, a decidua is formed in the other half, and grows during the early part of the pregnancy equally with the pregnant half. Moreover, twin pregnancy may occur not only in one uterine half, but pregnancy has been observed in the two halves simultaneously. In this case one fœtus is usually arrested in development. In these uterine forms, the ruptures, abortions, and tedious labours observed are accounted for by the smaller mass of the uterus, and by the diminution of the uterine fundus. When there is atresia of one half of the uterus bicornis, retention of menstruation and its perilous consequences will occur."

Dr. Batchelor states that in both cases reported by him there was both double uterus and double vagina.

Professor Watson is satisfied that Dr. Way's case was a hæmato-kolpos, and thinks that probably mine was also. But, even at the risk of disagreeing with so eminent an authority, I feel compelled to the view that in my case the vagina was single—that the left uterine os was imperforate, resulting in a hæmatometra with dilatation, and subsequent intra-uterine abscess which perforated at the point of least resistance, which point, as described above, was probably at the normal position of the left os. It would be classified in Pozzi's subsection of bilocular uterus with complete median septum and occluded left os and single vagina.

Dr. Way's case was apparently a true didelphic uterus with double vagina.

There is very little information in the text-books about the history and symptoms of patients suffering from these malformations. It is, therefore, of greater importance that every case should be noted and published. I admit that I should have been quite at sea in the diagnosis had my friend Professor Watson not turned up in the nick of time, and put me on the right track.

Dr. Way's Case (reported by Professor Watson).—13th December, 1897. Virgo, æt. sixteen. *Hæmatokolpos lateralis sinister* (not the same thing as *Hæmatometra lateralis*). Catamenia at thirteen and a-half years, fairly regular until three months ago; in September, 1897, complained of pain while at the washing-tub; mother sent her to lie down; menses lasted five days on that occasion, discharge was scanty. Mother noticed lump low down in abdomen (September); got fairly well again, and missed menses in October. In November pain returned. In the interval of four weeks had trouble with micturition; was very ill for three days in November. Menses for December due the day after to-morrow (15th December). Note: they appeared on that day and lasted till 20th, and have been regular and painless ever since.

Diagnosis.—Parovarian cyst. On opening abdomen the uterus (right) appeared surmounting a smooth, pale, elastic tumour, thought to be a hydatid cyst. The isthmus between right and left uterus was mistaken for the left uterine cornu as the bowels overlay the left uterus. Wound covered with sponge and a temporary suture, and vagina opened and stringy pasty blood in large amount (15-20 oz.) evacuated. On inspection of abdomen after this, the second uterus (left) was discovered, and abdomen definitely closed; and then the operator returned to vagina and enlarged his incision to the left, evacuating a further 10 oz. of ropy stuff, which came out like a rope as thick as a woman's wrist. Cavity (right vagina) then washed out with lysol, and stuffed with weak iodoform gauze, as also the main (left vagina). The left os then became palpable for the first time. Dr. Way said he had not located the (right) one; but here is a diagram of what I saw in the abdomen. Examination under ether before operating disclosed a smooth, elastic bulging, principally on left side towards left iliac fossa, which could not be circumscribed with finger, whereas on right side it could be felt passing up to a tumour, felt supra-pubically and noticeable to the eye.

DISCUSSION.

Dr. Worrall said the subject that had been dealt with in Dr. Pinnock's paper became one of great importance under certain circumstances. He referred to difficulties in diagnosis to which it might give rise. He would mention an instance which came under his notice. A woman came to him in the early stages of pregnancy. After examining her he found a shamrock-shaped tumour. One portion filled the pelvis, and, of the remainder that on the left was the size of the fœtal head, while that on the right was the size of a hen's egg. On opening the abdomen, the cyst which presented itself proved to be a pregnancy in the left horn of a bicornuate uterus. The small body on the right was the non-pregnant horn, and the mass in the pelvis was a dermoid cyst. If he had not acted on a rule which should never be deviated from, of pressing the hand over the cyst before plunging in a trocar, he would have brought about a catastrophe. As it was he removed the dermoid in the pelvis, and the patient went on safely to term.

Dr. Moore said that, like Dr. Worrall, had he known this subject was to be brought up, he might have prepared the notes of an unusual case that had come under his notice. A girl of thirteen, suffering from painful menstruation, was found to have a tumour in the pelvis, which was thought to be a tubercular tube. On opening the abdomen, a greatly distended and adherent

FIG 1.

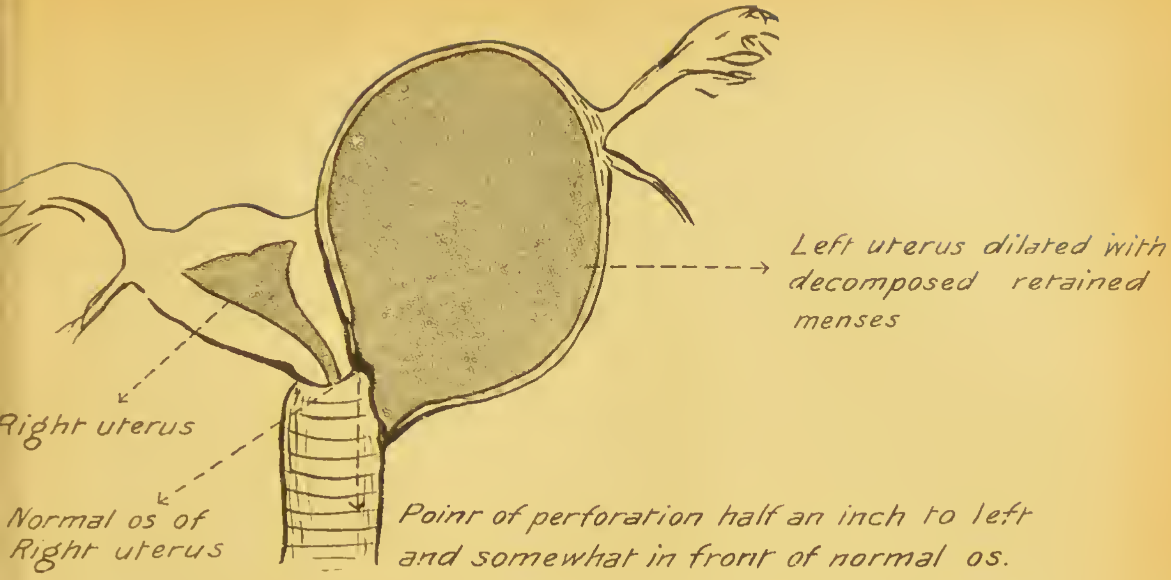
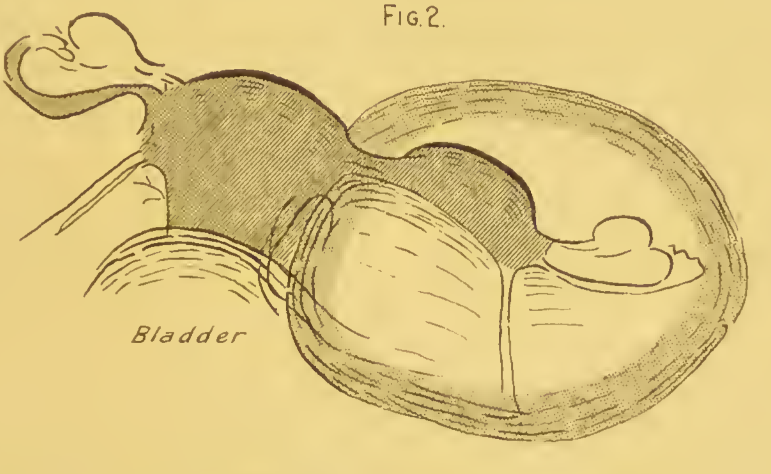

Left uterus dilated with decomposed retained menses
Right uterus
Normal os of Right uterus
Point of perforation half an inch to left and somewhat in front of normal os.

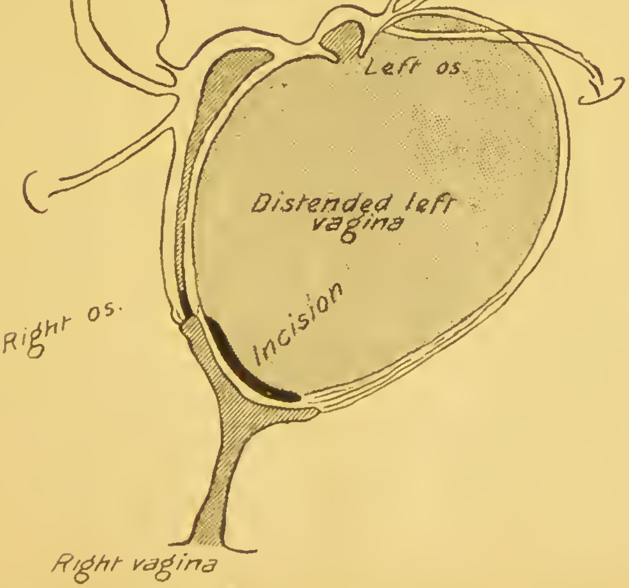
Diagram illustrating Dr Pinnock's case of Double Uterus.

FIG. 2.



Dr Way's case of Hæmatokolpos lateralis sinister.

FIG. 3.



Professor Watson's diagram explanatory of Dr Way's case.

tube was found. It contained a thick, tarry-looking material. The tube was found to be attached to the right side of the uterus, and no tube could be found on its left side. After some searching, a tube and ovary was found on the side of the uterus, and the tube was traced thence to the right side of another uterus. Nothing more was done on that occasion. But some weeks later, as the pain returned, the abdomen was again opened, and the uterus on the left side, which had no communication with the vagina, was removed. The patient recovered, and is now well. Prior to operation, on a final examination, one cervix could be felt a little to the right, and a tumour occupying the central and left part of the pelvis.

Dr. Thring said in his experience he had not often met with such cases, although they did turn up occasionally. One case he had, that of a young girl, showed the same symptoms as those mentioned by Dr. Pinnock in his case. He examined the pelvis, and found that was more elastic than cystic. From the examination it was difficult to decide what really was the nature of the trouble. Although the question of a double uterus was brought up, he decided against this theory, because he could distinctly feel the curves of the uterus. He opened the abdomen, and found a great mass, on the right side a large Fallopian tube of matter which had leaked away, and another knob perched on the top of the tumour. It really was a case of double uterus, which had caused peritonitis.

The President gave an account of a case of missed labour he had dealt with, adding, in conclusion, that the paper was most interesting from the standpoint of evolution.

FOUR CASES OF VENTRO-FIXATION OF THE UTERUS FOLLOWED BY PREGNANCY.

By Ralph Worrall, M.D., M.Ch.,
Sydney Hospital.

Retro-displacements of the uterus wholly or partially fixed are most advantageously treated by opening the peritoneal cavity. I make this self-evident statement, because, in my experience, it is not an uncommon mistake for practitioners, especially those recently qualified, to ignore partial fixation of the uterus and prolapsed adherent appendages, and proceed to treat such a condition by means of pessaries. By these measures symptoms can only be aggravated.

In passing, I should like to say that in my opinion some cases of retroversion with fixation present very slight symptoms, and require, therefore, no treatment other than local and general hygiène.

Ventro-fixation of the uterus is the most generally adopted operation for the relief of this condition. It is as certain in its results as any operation can be, but it is attended with two disadvantages—

1st. The slight risk associated with all operations in which the peritoneal cavity is opened.

2nd. The interference with pregnancy and labour which has been recorded in a not inconsiderable number of instances.

That the latter can be overcome, the following four cases are proofs :—

Case I.—15th August, 1895: Mrs. A.K., twenty-four; married three and a-half years; three miscarriages; one child, at full term, sixteen months ago, lived only a few weeks. Has had much gynæcological treatment, and worn various kinds of pessaries without benefit. Symptoms are: Backache, bearing down, especially at time of menstrual period. Dyspareunia and absence of sexual pleasure. Is very anxious for children.

Physical Signs.—Uterus retroverted with both appendages, which are considerably matted, especially the left. Uterus cannot be placed in normal position Considerable muco-purulent cervical discharge. Abdominal section preceded by curettage; adhesions separated with moderate difficulty. Left appendage removed, the ovary being enlarged and markedly sclero-cystic, and the thick tube having been damaged during the process of extraction. R. tube slightly thickened and ovary slightly sclero-cystic were separated from adhesions and raised up with the uterus, which was fixed by ventro-fixation and the abdomen closed without drainage. Easy recovery followed. On 18th December, 1896—that is, seventeen months from date of operation—patient miscarried at the fourth month, and made a good recovery from this after curettage to remove placenta. In September, 1897, she again became pregnant, and, acting on the advice of the family physician, who told me he had been treating the husband for syphilis for several years, I administered hydrarg. perchlor. throughout almost the whole course of the pregnancy. Delivery took place of a healthy child at full term in June, 1898, labour being so rapid and easy that it was over by the time Dr. Foster, of Waroonga, arrived. Convalescence was normal, and the child is at the present time well. I examined three months after delivery, and found the pelvis quite normal.

Case II.—9th May, 1896: Mrs. W.M., æt. twenty-eight; married three years; one child, 2nd June, 1894, died in 1895; no miscarriages.

Symptoms.—Leucorrhœa; backache; sometimes L. inguinal pain; wishes children. Has been treated with pessaries, by which symptoms were aggravated.

Physical Signs.—Uterus retroverted and considerably fixed. Both appendages prolapsed, matted, and tender; left moderatedly enlarged. Adhesions are felt in Douglas's pouch. Abdominal section preceded by curettage. L. ovary transformed into a cyst was removed with the tube, R. appendage and uterus separated from adhesions, and latter fixed by ventro-fixation. Easy recovery. Eighteen months afterwards, patient was delivered by Dr. Wilton Love, of Brisbane, of a healthy child at full term; pregnancy, labour, and puerperium all being quite normal. N.B.: Dr. Love tells me she has again been delivered without any trouble.

Case III.—Mrs. W.C., æt. twenty-seven; married five years; one child four years old. Has had treatment by pessaries, which has "made her worse."

Physical Signs.—Uterus retroverted and fixed, appendages low and matted.

Operation—10th January, 1896. Abdominal section; separation of adhesions; uterus fixed by ventro-fixation preceded by curettage. Dr. Elsner, of Moree, writes that patient was delivered of a healthy child on 17th September, 1897. Everything physiological, uterus in normal position two months after delivery.

Case IV.—Mrs. J.L., æt. twenty-eight; married ten years; two children, last seven years ago.

Symptoms—Menorrhagia, much bearing down. Has been treated by pessaries without benefit.

Physical Signs.—Uterus retroverted, considerably fixed. Both appendages moderately enlarged, low and matted.

Operation.—23rd January, 1897. Abdominal section, preceded by curettage, separation of adherent appendages. Uterus fixed by ventro-fixation.

10th October, 1899.—Patient reports that she was delivered in July, 1898, of a healthy full-term child, without medical assistance. P.V.: Uterus in perfect position and pelvis normal, except for slight thickening of appendages.

I have done twenty-three cases of ventro-fixation of uterus; so far the mortality has been *nil*. In all the uterus was in good position one month after having left the hospital—when patients came to report themselves. I have not been able to hear of the subsequent history, except in the four cases which I present to-day.

The technique was the same in all. Two through-and-through sutures of silkworm gut are passed through the parietes, through the uterine fundus for about half-an-inch in breadth, and an eighth of an inch in depth, and then through parietes on the other side. The first suture is entered on a line with the insertion of the tubes, and the second one quarter of an inch behind this. The peritoneal surfaces brought into contact are scarified. By passing the sutures in this way through the posterior portion of fundus rather than through the posterior wall as generally recommended, and also by restricting the adherent surfaces to an area of a quarter to half-an-inch, subsequent trouble during pregnancy and labour is avoided.

The sutures are removed on twenty-first day, but sooner if much cutting.

DISCUSSION.

Dr. Lightoller (Brisbane) said he would like to draw attention to the subject of anterior vaginal-fixation as a substitute in some cases for ventro-fixation. Each of these operations had its advantages and disadvantages. The advantages of vaginal-fixation were the simplicity of the operation; that it could easily be done by anyone who had seen it one or two times, and that you avoided opening the belly and the chances of ventral hernia. He was perfectly aware of some of the disadvantages that had been reported in connection with the troubles in confinement when this operation had been performed. These, in his opinion, had been much exaggerated. Dr. Herman, of the London Hospital, told him, a few months ago, that he had performed the operation over twenty times, and that he had been able to trace seven of these cases that had had children, and that in no case had there been any trouble He (Dr. Herman) also said that he was of opinion many of the complications had been brought about because the medical man attending the confinement anticipated trouble, knowing the operation had been done, causing him to feel unduly anxious and, perhaps, interfering earlier than would otherwise be necessary.

Dr. Lilian Cooper (Brisbane) said she had in her practice two cases of pregnancy following on the operation of ventro-fixation. The first case was one of badly retroflexed uterus. The woman had borne three children after her marriage, and after the birth of the third child a sterile period of ten years ensued. She (Dr. Lilian Cooper) operated on her successfully, and seventeen months afterwards a child was born at the full time with no complications whatever. The second case of retroflexion was not so bad as the first. The patient was delivered of a child soon after her marriage, and the infant only lived a few months. Since then she had had three miscarriages. She (Dr. Lilian Cooper) tried to raise the uterus by means of pessaries without success, and then did the operation of ventro-fixation—about eighteen months ago. Now her patient had become pregnant again, and was daily expecting to be confined. It was eight years since she had given birth to a living child.

Dr. Love (Brisbane) said the case which Dr. Worrall had operated on was a patient of his, and she had expressed to him surprise at the ease of her delivery. The labour lasted only one and a-half hours, when the baby was born before he could arrive. Since then his patient had given birth to another child, when the same easy delivery happened. Not being present he was unable to say how the first child had been born. The uterus was well forward, and quite movable and not apparently attached. The adhesions had lengthened, and his patient had suffered no inconvenience since the operation. In four or five other cases of ventro-fixation he had knowledge of where pregnancy had supervened since the operation, labour had always followed naturally. The ventro-fixation operation was certainly more painful for the first twenty-four hours afterwards, owing to the irritative contraction of the uterus. He had done vaginal-fixation in one case, and had found it incomparably more difficult than the ventral operation, his patient being a nullipara.

Dr. Thring (Sydney) said, on dealing with this matter of ventro-fixation, Dr. Worrall had brought up some very practical points. He agreed with his remarks about adhesions not being sufficiently recognised. He certainly could not sufficiently condemn pressure where there were adhesions. The more absolutely it was condemned the better, and in some instances, because the uterus was found to be movable, it was supposed there were no adhesions. He had not the slightest doubt that, as regards pregnancy, the after-effects of ventro-fixation were distinctly good. No trouble ensued to the patient when ventro-fixation was performed. One important point in favour of ventro-fixation, although it did occasion suffering after being performed, was that the mortality after vaginal-fixation was great, while the mortality in ventro-fixation was low; the suffering it occasioned was therefore justifiable. But he did not see the necessity of leaving the sutures in for twenty-one days. Regarding the question of anterior vaginal-fixation, it was not as easy as the ordinary ventro-fixation, whilst, as he had just pointed out, the results, as regards mortality, were worse.

The President said Dr. Worrall had done well in bringing up a paper which affected gynæcologists very much. But he must point out that the term "ventro-fixation" was now obsolete. Dr. Worrall, he had no doubt, referred in his paper to ventro-suspension as practised to-day by all the advanced schools of surgery. Ventro-suspension meant a suturing of the uterus, and not a fixation, so as to make that organ movable. There was nothing so important in this operation as keeping the uterus suspended. If you did not do that, you substituted one serious displacement for another. If you did not do more, you interfered with the functions of the bladder and bowels. He was in accord with Dr. Thring that the operation of vaginal-fixation had proved an entire failure, and had frequently been the source of extreme trouble in parturition. The operation was fraught with risk, and on this account had been abandoned by every man in the advanced schools of gynæcology. With regard to the future of the patient, should parturition occur, one little weakness in the situation was that no man could say with any exactness what the extent of the suspension or fixation would be after the operation. He was compelled to leave the question to the Fates. In performing this operation the buried silk suture recommended itself most strongly, but he had an objection to it, and that was that you could not exactly say whether it effected suspension or fixation. Dr. Prior, of New York, said where there were adhesions he preferred to shorten the ligaments. One point in favour of suspension of the uterus was that where you had to separate adhesions there was the danger of their attaching again. That difficulty had been overcome in America by operating. He had been very pleased to see Dr. Worrall bring up this question of ventro-fixation, for above all others it was one of the most important questions of the day.

Dr. Worrall, in reply, said that with regard to the relative advantages of ventro-fixation and vaginal-fixation, the President and Dr. Thring had answered Dr. Lightoller. In reply to the President, he intended to emphasise that his method of performing ventro-fixation was the way by which suspension could most surely be accomplished. The means of fixing the uterus meant really a suspension and not a fixation. If buried silk sutures were used, suspension would less surely occur, or less rapidly; but he had no objection to the buried kangaroo tendon. He had not observed that extra suffering after ventro-fixation of the uterus mentioned by Dr. Love.

RUPTURE IN EARLY ECTOPIC GESTATION.

By William S. Byrne, B.A., M.D., B.Ch. (T.C.D.),
Hon. Physician, Lady Bowen Hospital, Brisbane.

My object in bringing the subject of early rupture of the extra-uterine gestation sac before you is not with the idea of shedding any new light on this usually disastrous accident, but rather with the intention, firstly, of drawing the attention of general practitioners to the fact that, in my opinion, the condition is not at all so uncommon as it is thought to be, and to impress on each and all of us that unless we constantly keep in our mind the fact that at any time we may most unexpectedly be brought face to face with a case, and that if we are not particularly careful we shall in all probability miss the diagnosis until it is too late; and, secondly, to endeavour to ascertain by discussion if there are any new symptoms which will help in the diagnosis of early rupture, save the three great ones—namely, pain, shock or collapse, and the symptoms of early pregnancy.

These cases, as a rule, are not seen by specialists in the first instance, and a very great responsibility devolves upon the family attendant when he is called to a case which presents symptoms from which he should suspect the rupture of an early tubal pregnancy.

Clinically, we meet with two great divisions of early rupture—namely, with and without a hæmatoma. Of course, where a hæmorrhage is confined, and a swelling forms at once, a diagnosis is easily formed, which is generally correct; but in cases where there is no swelling to be felt, either abdominally or per vaginam, the diagnosis is not so easy, and it is to those cases I wish more especially to draw attention.

Now, what are the symptoms which would lead us to believe that we were dealing with a case of this kind—abdominal pain, collapse, and a history of early pregnancy? Possibly there may be a discharge of decidua; but unless we see the membrane ourselves, as happened in one of my cases, the reports of the patient or friends are not very exact or trustworthy, so we are practically bound down to those two symptoms—pain and a history of pregnancy—and one physical sign, collapse. There is often in this type of case no swelling to be felt through the abdominal walls, or by the vagina, or even by the bi-manual method of examination. If there is a decided history of the suppression of the menses for a couple of months, and a sudden intense pain in the abdomen, and well-marked collapse, our diagnosis is fairly certain, and one should not hesitate about operating at once—and this, one might call a well-marked case of hæmorrhage into the peritoneal cavity—but the symptoms seldom are so well-marked. In the first place, menstrual suppression in these cases is seldom absolute. An irregularity of the discharge, or a diminution, is more usual during the first couple of months of pregnancy, and it is just that time we are interested in. Again, the extent and intensity of the pain are very variable; there is not always that agonising pain which one reads of in books—in fact, in the five cases I have seen, the pain at the time of rupture was quite bearable. Then the collapse and its symptoms depend, in a great measure, upon the amount of blood suddenly withdrawn from the general circulation, but a great deal of it also is owing to the nervous shock, and this varies to a great extent in different women, and also varies in proportion to the extent of the rupture and its effect on the sympathetic nervous system. When a large rupture occurs the nervous shock is great and the hæmorrhage into the cavity of the peritoneum excessive and sudden, and in those cases we see the typical symptoms which we do not as a rule mistake; but in many cases—and I am not sure that it is not in the majority—the rent is small, the nervous shock slight, the hæmorrhage more of a trickling nature, the pain slight, and consequently there is very little collapse; but always—and this I would like to lay considerable stress upon—the patient presents an anxious and harassed countenance. In fact, in one of my cases it was the appearance and expression of the woman's face that first led me to a correct diagnosis. Now, if this class of case is the usual one, on the first visit, or even the second, or third of the family doctor, can one blame him for not at once noticing the gravity of the case? But sooner or later in the majority of cases his suspicions are aroused, and to dally then or to wait for more urgent symptoms is fatal treatment. In one of my cases I waited, and was sorry. I was sent for one afternoon to see a woman who was complaining of severe pain in the groin, and generally all over the abdomen, but more especially in the groin; there was no great sign of collapse. There was a partial suppression of the menses for two months, and the last child was born eighteen months before. When I saw her her pulse was quick certainly, but this would be attributed to excitement, and there was no vomiting—her only symptom was pain. Her facial expression, however, was one of intense anxiety. I was not satisfied, and brought a medical friend over to see her that evening, and mentioned my suspicions. I had examined her abdomen and pelvis per vaginam on my first visit, and again in consultation, but with negative results. We decided to wait, thinking that the symptoms were not definite enough to open the abdomen, and on my visit next morning found her quite cheerful, sitting up in bed, and wanting to get up. I congratulated myself then upon my hesitation in performing laparotomy, and looked upon her condition the previous night as having been caused by some digestive derangement. My complacency, however, was short-lived; late that night she was suddenly seized with pain and collapse, and died within an hour and a-half.

In another case I was sent for to attend a patient who was suffering from a supposed miscarriage at about the second month of pregnancy. The pain was intermittent and of the character of uterine pain; there was some slight bleeding from the vagina; there was no shock nor collapse, but a very anxious facial expression. I contented myself with making a vaginal examination, and found nothing, and bi-manually with the same result; but the patient was very fat, and one might easily miss a small swelling. I gave a hypodermic of morphia, and on my visit next morning found the patient much better. On examining the uterus again, I found something hanging from the os, and on withdrawing it and examining it found it to be decidual membrane, almost a complete cast of the uterus. Then my suspicions as to the true state of the case were aroused. However, I did not like to interfere actively, as I could find no swelling in either fornix, but determined to operate as soon as I could make fairly certain. This patient progressed most favourably for a fortnight, and then got up and, contrary to my wishes, went about the house. On the second day she was again seized with collapse and pain. I was sent for, and on my arrival found a large swelling over the left iliac region. I hoped this would control the hæmorrhage, and after consultation with one of my medical friends, as the woman's condition was good, and there was no indication of the bleeding still continuing, we kept her in bed and on opium. The swelling gradually enlarged, and in a week extended across the abdomen. In a few days more her temperature rose, and she presented all the signs of septicæmia, so I got her into a private hospital, opened the abdomen over the swelling, washed out a large quantity of clot, and inserted a drainage tube. The subsequent history was uneventful. In neither of these cases was there any previous sterility. One woman was confined eighteen months previously, and the other two years previously.

One case I saw in consultation was in too bad a condition from evidently sudden hæmorrhage for any operative procedure, and after carefully considering the matter we decided to wait. Gradual improvement ensued, and recovery.

Another case I saw in consultation. Here there was a sudden and severe collapse and shock, some pain, though not very great, a history of a two and a-half months' pregnancy, and, on bi-manual examination, a sense of fulness in one of the fornices. Here operation was urged, but from one cause and another was postponed by the relatives, and when, after thirty-six hours, I did open the abdomen, our diagnosis was quite confirmed, but the patient died on the third day from exhaustion. The last case was something similar; the patient was evidently dying at the time of operation, and the procedure was only undertaken as a forlorn hope.

Now, what are the lessons we are taught from these few cases? First, as to diagnosis—and one feels this to be the most important matter. Given a patient with abdominal pain, symptoms of collapse, and a history of early pregnancy, and with very little to help us on bi-manual examination, what are we to do? We may suspect, but certainty is out of the question until we open the abdomen. But, of course, if in addition to the foregoing symptoms we can feel a tumour in either fornix, our duty then is clear: we ought to operate at once. But, with regard to the former class, I do not know of any other condition which will give the same three signs. I think in such cases our duty is to put the facts and our doubts before the patient's relatives, and, while allowing them to decide the matter, to urge immediate laparotomy as a means of certain diagnosis; although some few cases may recover after a hæmorrhage into the cavity of the peritoneum. We never know when another and fatal hæmorrhage may occur, but we *do* know that another attack is very likely after days, or even weeks, and the safest and best method of treatment is to operate, and make absolutely certain of our position. The mere opening of the abdomen, according to our present methods, with careful asepsis, is not a dangerous procedure; and although I have often regretted not having opened the abdomen soon enough, I have never regretted doing so early. In fact, I am sure, if we are to have any measure of success in these cases, in early operation will be found the secret. Again, I would urge on the family doctor the necessity of always keeping in his mind's eye the fact that at any moment he may be called to one of this class of case; that if he is not very careful he may possibly treat the matter as one of possibly severe digestive disturbance, await the development of events, and, when too late, suddenly awake to the gravity of the patient's condition. I would urge him to recollect that in early operation lies almost the only hope of the patient's recovery, and not to delay active interference.

DISCUSSION.

DR. WORRALL (Sydney) said he congratulated Dr. Byrne on his excellent paper. It was of great importance to them all, and no one would say that this was an undeserved compliment. Dr. Byrne had urged early operation in cases of this nature, and he (Dr. Worrall) thought any surgeon should be ready to perform an operation in such abdominal cases. Regarding the failure which had overtaken these operations, the diagnosis of ectopic gestation might easily be mistaken for that of abortion. With regard to the presence of tumours, he found that in the worst cases there usually were none. In a case of that kind he had, he found only a boggy feel over the vaginal vault.

DR. THRING (Sydney) said he had to thank Dr. Byrne very much for reading what was really a very practical paper, and one that was what they wanted. He was perfectly sure they had more to gain by discussing their failures than their successes, and he wished more papers of a similar nature had been prepared. It grew monotonous listening to so many successful cases. With regard to the question of early operation in these cases, his experience was that it had saved many a life and hardly endangered any. One point he would mention which was not often recognised, and that was the matter of menstruation.

Dr. Byrne had raised the question whether he had done right to leave what was practically a cyst. He preferred to remove the damaged Fallopian tubes rather than leaving anything behind. In some cases he had seen some uncontrollable bleeding, where you had not the field of operations under your eye, as in operating through the vaginal vault instead of through the abdominal walls. You could not, nor dare not, thus remove all the damage from the vaginal vault. On the whole, this was rather an important point—this question of the vaginal or the abdominal route.

Dr. Lightoller (Brisbane) said he would suggest that in cases of early tubal rupture and in instances of tubal abortion it would be preferable, instead of opening the abdomen, to take the direct route through the vagina. Dr. Thring said his objection to this operation was that the field of operation was extremely limited. He (Dr. Lightoller) thought that although the field of operation was to some extent limited, still there was ample room for all practical purposes in the majority of cases. The uterus in this operation was retroverted, and the fundus, together with the tubes and ovaries, could be drawn through the vaginal wound into the vagina itself. They could then be inspected, tied, and cut if required, or any other treatment they might require could be adopted. He had seen this operation done a great number of times by three of the leading gynæcologists in Europe, and they all spoke most highly of it.

Dr. Garde (Maryborough) said he would refer to two cases of ectopic gestation. Both were patients of another practitioner. In the first case when he was called in there was immense collapse, and he had no difficulty in finding a swelling in Douglas's pouch. There was a dull percussion note over a large extent of the abdomen. He opened the abdomen and found a two months' growth in the left tube which had been ruptured. No difficulty was experienced in removing this, and he also removed both ovaries and tubes. There was also a large amount of blood in the abdominal cavity. In the other case—a patient of Dr. Luther's, he found the woman in a condition of collapse. After waiting for six days, as there were no bad symptoms he determined to let well alone—although the discharge of blood must have been enormous. With regard to the papers that had been referred to by Dr. Thring, a good number of them were of no practical good. If it were possible to do so at the next Congress, he would suggest that a sub-committee previously picked out from the papers sent in those likely to prove of any benefit, and then have them printed and read at their sittings. They would be likely to learn more from such papers than listening to these discussions. He hoped that in the future this would be done.

The President said he complimented Dr. Byrne on his artistic and practical paper. No one but an artist could paint so perfect a picture in such graphic terms, and he was surprised to find that a man so busy in carrying out his executive duties could devote time to elaborate so high-class a paper. Ectopic pregnancy diagnoses, most men were in doubt about. He had no hesitation in saying that the earlier the operation the better. Even when the patient was *in articulo mortis* there was no other case in which such arbitrary treatment was required as in ectopic gestation. These were the cases in which men of the advanced school invariably recommended bold surgery. He had operated in the case of a woman who, when he saw her, was utterly prostrate and pulseless. The time was so short that while he was proceeding with opening the abdomen the chloroformist was proceeding with transfusion. Saline solution was injected beneath both breasts, and there was no difficulty in completing the operation, with the result that the woman, soon after the removal of the affected parts, responded, and made an uninterrupted recovery. After primary rupture the practitioner must take the *tout ensemble* if he wanted to treat these cases successfully.

CASE OF COMPLETE HERMAPHRODITISM.

By A. F. Davenport, M.B. Lond.,
St. Kilda.

Child born in 1893, the sixth in family. Parents healthy and robust; father, a Congregational minister; no neurosis apparent in any other member of the family. At birth child weighed 10 lb., and on superficial examination was a large, fully developed, normal female; christened Mary. Was brought to me at six months, when I found it grown beyond all proportions, and with a hoarse masculine voice and hirsute development on chin, upper lip, and pubes. The clitoris was 3 inches in length, and remained erect during my examination. The urethra opened into base of clitoris, there was no vestibule, testicles, nor vaginal orifice.

At twelve months, I asked Dr. Snowball to see the case in consultation. We examined under chloroform. Concluded it to be a case of complete hypospadias, seeing that urethra opened into base of penis and still an absence of vestibule. Child was now accepted as a boy, and rechristened John. Its voice was now baritone; it had distinct whiskers and pubic hair, and presented the appearance of a boy of five years in size. Saw it again at intervals until it was five years old, when its appearance was startling. Its voice was a bass, and it sang many songs. It could have been passed off as eighteen years old. Its mind and intellect were very keen, and it was a great favourite with its family, but to the ordinary observer it was repulsive and uncanny. Its penis every time I observed it seemed in a constant condition of priapism, and it evidently practised masturbation. It had quite a respectable moustache and side-whiskers, and much pubic hair.

I am informed it died in an epileptiform convulsion. Am of the opinion that this was probably induced by the constant sexual excitement resulting from the great vascularity of its penis (?) and other organs.

CANCER OF THE CERVIX UTERI FROM A PREVENTIVE POINT OF VIEW.

By Emma Constance Stone, M.D., Ch.M., L.S.A.,
Physician to the Queen Victoria Hospital, Melbourne.

The triumphs of modern medicine have been obtained in the prevention of disease in the strictly medical aspect by hygienic measures; in surgery by the prevention of sepsis. Hygiene has practically banished some fevers, and aseptic and antiseptic measures have made possible the splendid results of recent surgery. If we were asked to name the diseases against which we ought now to wage the most strenuous war, we should probably place cancer and tuberculosis at the head of the list. In the latter disease the assurance of victory has been obtained in the discovery of the cause. The obstacles to mastery over it lie largely now outside the domain of medicine proper—in the ignorance and apathy of the public, and in the false sentiment which still makes isolation of tubercular patients a very difficult and delicate matter. The lives of healthy members of a family are still risked in the supposed interest of the invalid; and those who, with enlightened self-sacrifice, are willing to submit to the dictates of reason and science run the risk of being called heartless and void of sympathy. In this, as in other matters, the enthusiasm of the few must shame the apathy of the many, and bring about a time when to provide sanatoria and means of isolating tubercular patients will be recognised as one of the duties of the State, and not as the privilege of the charitable.

The pathology of tuberculosis is so far advanced that medical science can now confidently point out the necessary measures for stamping out the disease. We are far from that grand consummation yet, but science has put up the necessary finger-post. The destination and the way to reach it are set out. Can as much be said in regard to cancer? Unfortunately, it cannot. We are still ignorant of the cause of cancer, and can only state in a general way the conditions that usually precede an attack. This being the state of our knowledge, is cancer of the cervix uteri in any degree a preventable disease? If so, how can it be prevented? A discussion of the histological character of the tissues involved in the cancerous growth, as seen in actual practice, does not promise much help in our preventive treatment. Cancer of the virgin cervix is extremely rare, and when seen it appears to have been preceded by forcible dilatation or by erosion.

The latter fact lends some additional force to the view that all cancers of the cervix have their origin either in the deep connective tissue or in the newly formed glands found in the follicular and papillary erosions. The significance of this will be seen when we come to consider the preventive treatment. It would thus appear that cancer of the cervix may be said to be due to injury. In the parous the injury is mainly caused at childbirth; in the nulliparous it is due to friction, and the general systemic causes that lead to it. If this be true we ought to be able to largely prevent cancer by restoring the injured tissues to their normal condition, or as near this as the state of the parts will admit.

In discussing the preventability of cancer of the cervix, the predisposing causes—such as heredity, age, and the struggle for existence—should be considered. As far as I have been able to find out, no statistics based on a sufficiently large number of cases have been collected in regard to heredity; and the tendency seems to be to consider it as not an important ætiological factor. The question might perhaps be more definitely settled by an appeal to the experience of a number of the large life assurance societies. From a preventive point of view, heredity is not of great importance, as it is probably one of the two factors in the problem which can be least influenced. The other factor is age. Statistics clearly show that the most susceptible age is between forty and fifty; and next comes that between fifty and sixty. During these years patients should be warned to be specially on the alert; and in the question of treatment the practitioner should bear in mind the special liability of this period of life to this terrible disease. The struggle for existence and its privations clearly favour the development of cancer. We cannot change our ancestry, nor stop the march of time; but if hard work, insanitary surroundings, and poverty conduce to cancer, then we have another strong argument for changing the lot of the toilers. There can be no doubt that cancer is much more common among the poorer classes where there is less medical attention, less cleanliness and care on the part of the patients themselves, less readiness to seek early advice, and less vitality, due to the increased struggle. Cancer is on the increase. We pay the penalty in pain for working at high pressure; and those suffer most who live the most toilsome lives. When we are all leisurely socialists, one potent factor in the ætiology of cancer will have been removed.

In addition to the general causes of cancer, there are local pathological changes which are powerful for mischief in the case of the cervix uteri. These may be comprised under two headings—

(1) Erosions with cervical metritis and cystic degeneration.
(2) Contusions and lacerations at childbirth and their consequences.

For the purposes of this paper erosions of the cervix may be divided into two classes—

(A) Those which arise during general systemic debility, such as are often seen in single women and in nulliparæ.
(B) Those which arise from injuries received during childbirth—and this class is by far the more numerous.

In regard to the first class, I wish to emphasise the point that, however severe in character or of however long standing the erosions may be, they *very rarely indeed* take on a malignant form. In the course of ten years' watching, in hospital and private practice, I have never seen a case. This also is the experience of those whose opportunities for observation are incomparably greater than mine.

Howard Kelly says: "I recall only three cases of cancer of the cervix in nulliparous women in my entire experience, and in one of these the cervix had been forcibly dilated. Dr. T. A. Emmett told me that the only case of cancer of the cervix he had ever seen in a nullipara was also one where forcible dilatation had been practised."—Kelly: Operative Gynæcology, Vol. II., page 306.

In regard to cancer of the cervix then, nulliparæ may practically be said to be immune. The unfortunate sufferers from this form of cancer belong almost exclusively to the second class—viz., those who have been subject to the contusions and lacerations incident to childbirth. Notwithstanding the fact that cervical erosions, so-called, very rarely degenerate into cancer in nulliparæ. I would still strongly urge the cure of this condition as one of the preventives of cancer of the cervix. The immediate results of the disease call for relief; but, beyond this, there are two strong reasons for radical treatment. In the first place, the histological character of the growth—for such it is—proves it to be on tho borderland between the benign and the malignant. "Erosion" is a misleading name, for there is no loss of substance, though the growth looks like an ulcer. The normal squamous epithelium of the vaginal portion is replaced by a single layer of the columnar variety. There is a new formation of gland tissue. It is indeed a flat adenomatous growth, with the deeper layers infiltrated with leucocytes. Given such growth, one can understand that a forcible dilation of the cervix could determine its development towards malignancy. It is not unfair to assume that the rare cases observed by Kelly and Emmett ran this course, seeing that endometritis, which would need a dilatation for its cure, generally accompanies erosion.

The next reason for the radical treatment of erosion is that, should pregnancy supervene, the eroded tissne, with the accompanying connective tissue hyperplasia, is incapable of proper dilatation during labour; and laceration takes place with subsequent increase of the eroded surface. This condition is admitted by nearly all the authorities as a prolific source of cancer. Accepting this view, I would urge that another great preventive measuro is the repair of laceration of the cervix due to parturition. A recent writer has stated that "it is a cruel fraud to tell the patient that she must have this operation done to prevent cancer." It may be a fraud to say that she *must* have it done to *prevent* cancer; but it would, in my opinion, be the kindest truth to tell a patient with a bad laceration that she had better have it repaired for fear of cancer. Thousands of women, it is true, live to a good age and die of other diseases who have more or less lacerated cervices. It is probably equally true that very few die of cancer of the cervix who had not a laceration as the initial step.

Emmett's operation should not be performed without due regard to the condition of the lacerated cervix, and the symptoms arising from it on the one hand and the immediate danger to the patient on the other. The immediate dangers may arise from overlooking certain pelvic conditions contra-indicating operation. With ordinary care and experience this danger can generally be avoided.

The risk from sepsis should not in the present state of our knowledge be a formidable one.

There remains the danger from a general anæsthetic. I maintain that in nine cases out of ten this is an utterly unnecessary risk to run. If the operation is performed with the patient in the Sims position, so as to minimise the objection which women have to these measures, then both the preliminary curettage and the subsequent repair of the laceration can be painlessly and efficiently performed by the use of cocaine or eucaine.

In order that women may avail themselves of this supreme preventive of cancer of the cervix, I urge that the gravity of the operation should be minimised and all possible elements of danger eliminated. So much fuss is made over it by many practitioners that women regard it as a very serious and dangerous operation, and it is often made to appear so. The operator brings an anæsthetist, an assistant and two nurses, and completes the dishonest farce by insisting upon a night nurse. It is needless to say that there is nothing connected with that operation of any magnitude, except the fee. Such methods rob women of much-needed relief, and bring our profession into contempt.

In addition to the repair of the laceration, it is also essential that the cystic degeneration, which is often present, should be cured. This is sometimes a difficult matter, but in intractable cases a partial amputation of the cervix will always bring success. The next preventive measure I would urge is patience on the part of practitioner and patient during the early stages of labour. Give the tissues every opportunity of dilating sufficiently and naturally. Cancer of the cervix is undoubtedly on the increase. Aseptic measures enable us to interfere by mechanical means with far less risk than was formerly the case. Knowing the part played by injury in the production of cancer, is "meddlesome midwifery" responsible for some of the mischief? My experience has been that the worst cases of laceration have been due to the use of forceps at an early stage in order "to finish up the matter quickly." This has been done either to silence the appeals of the patient or to suit the convenience of the practitioner. To do it in response to the first, is an inexcusable weakness on the part of a doctor, who knows that much graver results than temporary pain can arise out of want of a little patience and fortitude. When it is done for the convenience of the doctor, it is an act which cannot be too sternly reprobated. In many cases that I have known, it has been unmitigated ignorance or brutality, or both.

I would further urge the routine practice of examining the condition of the cervix after involution is complete. I would especially urge this after instrumental delivery. In every case where there is a laceration, the patient should be informed of it, and advised either to leave it alone or submit to operation. Knowing as we do that grave results may follow from these injuries, our patients are entitled to know of their condition, so that they may intelligently watch themselves, and report any unusual symptom. For our own credit also this should be done; for nothing shakes the confidence of a patient in the skill and integrity of her family physician so much as to be told by some other practitioner that she has a bad laceration. She then remembers either that her medical attendant neglected to ascertain her condition or, having made an examination, failed to inform her of the result. So the one who neglects and the politic practitioner who thinks it best for himself to maintain silence are both found out, and not to their credit.

The last preventive measure to which I wish to call attention is the educating of women to take early alarm. All proper means, public and private, should be used by the profession to disabuse the minds of women of the dangerous belief, held by so many, that "leucorrhœa is normal to the married," and that "irregularities of all sorts are unimportant about the menopause"; and, on the contrary, to urge that advice should be sought early on the slightest appearance of any vaginal discharge, however simple. The women practitioners of Australia should perform a large part of this work of education; and it may not be presumptuous to hope that the advantage they possess may make relief easier for some of their sisters who need it.

POST-PARTUM HÆMORRHAGE: ITS TREATMENT—ANTICIPATORY AND ACTUAL.

By Ed. Luther, B A., M.D. (B.Ch., Etc., Dub. Univ.),
Hon. Surgeon to the Lady Musgrave Hospital; and Hon. Physician, Wide Bay and Burnett Hospital, Maryborough, Queensland.

The first requisite against hæmorrhage from the post-partum uterus is the maintenance of its firm uniform contraction and tonic retraction (Lusk).

In my experience the chief cause of post-partum hæmorrhage amongst Queensland women is the want of this muscular tone, for amongst our women one meets with a large number suffering from inanition and anæmia, especially during the summer months. Whether this anæmia is caused by malaria, anchylostoma, or just the enervating heat, it matters not for the purpose of this paper. But, whenever I am engaged to attend at the confinement of a woman anæmic in appearance, I always anticipate having some post-partum hæmorrhage, for with the anæmia you will certainly have uterine inertia and flaccid abdominal muscles, that may have just sufficient stamina to expel the fœtus, but none left for the expulsion of the placenta, or for keeping the uterus contracted afterwards.

Now, what I wish to bring before you to-day is that by a preparatory treatment we can ward off this evil, dreaded by most accoucheurs.

My method of treatment is first to try and cure the anæmia and debility by tonics of iron and strychnine, and during the last fortnight or three weeks of gestation to place the patient on the following mixture:—Extractum ergotae, 22 gr.; liquor strychninæ, 1 dr.; acid sulphuric dilut, 1 dr.; glycerin, 1 oz.; aq. anisi, 8 oz.; a tablespoonful to be taken three times a day. I have now been adopting this treatment for the past four or five years in all cases that have a history of having had post-partum hæmorrhage at their former confinements, or where the patient seemed debilitated or a likely subject for hæmorrhage, and in every case with satisfactory results.

One of the first cases on which I adopted this treatment was a Mrs. H., whom I had attended at two of her former confinements, and on each of these occasions she had severe post-partum hæmorrhage both before and after the expulsion of the placenta, and which I had great difficulty in controlling with hot-water douches and ergot hypodermically. Even then the uterus remained large and flabby, and occasionally there was an attempt at flooding up to the fifth or sixth day. As she lived some distance up country, I persuaded her to come into hospital three weeks before she expected her next confinement, and I placed her on the above mixture with the happiest results, for, instead of labour coming on a few days before her full time, as had occurred at her former confinements, gestation was prolonged to the 284th day and she had a perfectly normal confinement, the placenta coming away readily with the fourth pain (ten minutes after birth of child). There was scarcely any hæmorrhage, the uterus contracting firmly and never showing any sign of getting into that flaccid state that it had done on previous occasions. I was so impressed with the effect of this treatment that I again adopted it in one or two other cases that had histories of tedious labours and post-partum hæmorrhage, with the same good results; the patients themselves remarking that they were the easiest and quickest labours they ever had. Since then I have always employed this treatment in all cases that have histories of post-partum hæmorrhage and in those whom I would expect it from their appearance, and as yet it has never failed to do good.

About two years ago Dr. Lombe Atthill, of Dublin, published a paper on this treatment in the *British Medical Journal* of 6th March, 1897, a paper that is well worth reading by those who take an interest in this subject. I think I cannot do better than quote his concluding remarks, which quite coincide with my personal experience. (His mixture varies slightly from mine, as he gives the following:—Extract ergot liq., 3 drams; liq. strychninæ, 1 dram; infus. ergot, 6 oz.) He lays down the following as the result of his experience with this treatment:—

1. That when administered previous to the termination of pregnancy in the case of women in whom a tendency to post-partum hæmorrhage is known to exist, it tends in a marked manner to prevent the occurrence of hæmorrhage.

2. That when so administered in ordinary doses it does not produce any injurious effect on either mother or child, and that its exhibition seems to delay the commencement of labour in such case.

3. It tends to make the involution of the uterus more perfect, and lessens the chance of the occurrence of subsequent uterine troubles, many of which depend for their cause on imperfect involution of that organ.

4. It will not bring on premature labour or induce abortion unless uterine action has previously been set going.

5. In cases of threatened abortion, its administration frequently seems to act as a uterine tonic, and in some cases tends to avert the danger of a miscarriage, provided the ovum be not blighted.

6. That if the ovum be blighted, and specially if it be detached, ergot usually hastens its expulsion.

When post-partum hæmorrhage occurs in cases where the anticipatory treatment did not seem called for, and where, after first clearing out from the uterus all clots, retained pieces of membranes, or placenta, and douching with hot water, the hæmorrhage still continues, I have found that a hypodermic injection of a twentieth of a grain of strychnine, repeated if necessary in ten minutes, has a miraculous effect in causing the uterus to contract firmly, and thus arresting the hæmorrhage; its action being nearly instantaneous places it a long way in front of ergot, which is so slow as to render it practically useless in urgent cases. This hypodermic injection of strychnine has stood a good friend to me when ergot and all other remedies usually laid down in midwifery text-books have failed me; insomuch that I now never attend a midwifery case without first seeing that my hypodermic syringe is in order, and that I have strychnine (tabloids or in solution) at hand. I might also mention here that I have found strychnine very useful in assisting to start respiration in infants born in a state of asphyxia pallida, when the ordinary methods of artificial respiration have failed. A hypodermic injection of a hundredth of a grain will often act like magic. The first child I tried this treatment on I injected two minims of the B.P. solution; it certainly started respiration, but the infant very soon took tetanic convulsions, which I had to counteract with hypodermic injections of atropine; the child did well, but since then I always use the smaller dose. Of course, I have been speaking only of hæmorrhage occurring from the placental site, but one may also have severe hæmorrhage from lacerations of the cervix or vaginal walls, where ligaturing the bleeding vessels and suturing the parts will be the only treatment required to arrest the hæmorrhage.

I bring this method of treatment with strychnine and ergot before you to-day, as I am sure many others like myself have found that the ordinary treatment as generally laid down—namely, emptying the uterus of all clots and portions of placenta or membranes, ergot hypodermically or by the mouth, and hot and cold water douches—will act for the time, but they will not keep a flabby uterus in a firm tonic contraction for long, or in the same manner as when ergot and strychnine have been previously taken for some weeks, or when given together hypodermically after the placenta has been expelled.

A SHORT REPORT ON SERUM-THERAPY IN FEVER AFTER DELIVERY.

By E. Fairfax Ross, M.D., Etc., Etc., Sydney,
Senior Hon. Physician, St. Vincent's Hospital; and Senior Hon. Surgeon, Lewisham Hospital.

In January of this year I was called to see a woman, aged thirty-six, in her first confinement. She was attended by a most ignorant self-taught midwife, and had been in labour for over twenty-four hours, was making no progress, and was very exhausted. The midwife was absent when I arrived, and my examination revealed nothing abnormal; it was simply a case for chloroform and instruments. The midwife returned shortly, and, seeing the style of woman she was, I looked at her hands, which were roughened by hard work and dirt-marked in the usual way, and on her right index finger (examining finger) was a deep cut over an inch long, three or four days old and "discharging." She said she had made examinations, so I got another nurse to set to work to cleanse the vagina and external genitals with biniodide of mercury, hoping against hope to counteract the germs the nurse had introduced. The liquor amnii had long since drained away, and I delivered, with difficulty, a very large recently-dead child (dystocia due to large child, rigid parts, and inertia of uterus). A slight laceration of the anterior lip of the cervix occurred, but none whatever of the perineum, and the whole of the afterbirth came away naturally. I positively assured myself of this, having future complications in view. No flooding, and she rallied well from the chloroform, but suffered intense grief at the loss of her child. Now, I argued that if ever a woman ran risk of fever after delivery this woman did, so instead of waiting for symptoms I at once attacked the sepsis and procured some antistreptococcic serum, made by the Pasteur Institute, of Paris, imported by L. Bruck, and twenty-four hours after delivery injected 10 c.c. in the anterior abdominal region—infinite antiseptic precautions being used, though at this period she had no abnormal symptoms save those incident to difficult labour—viz., inability to empty bladder, and difficulty in turning and soreness. T. 99°, and P. 84, R. 18, and lochia natural. On my next visit at the end of twenty-four hours she was in much the same condition, but the pulse was 90 and R. 20; had slept ill and felt weak; I injected another 10 c.c. At the end of the next twenty-four hours she was very ill indeed; had passed a restless night, felt very weak, but had no pain save in the head and back. T. 104°, P. 120, R. 35, skin dry; abdomen distended and becoming tense; lochia very scanty; urine (drawn off) dark, and small in quantity; no milk in breasts—in fact, the breasts never gave any trouble. Injected another 10 c.c. and within twenty minutes the good effect was very marked, patient saying she felt much better and very comfortable; much cooler and inclined to sleep. For the five following days the fever continued with all the classical symptoms of sepsis; scanty lochia (not offensive); no milk; high fever; rapid feeble pulse; damp skin; delirium at nights, mind clear during daytime; profound weakness and feeling of faintness and sinking through the bed. I injected 10 c.c. every twenty-four hours; this always gave relief and remission of the bad symptoms for some hours; and also liquid nourishment (often vomited) and brandy were given at frequent intervals, and the bowels were kept open by salines, and no other drug but the serum used. After the eighth day she began to improve; the sepsis suddenly ceased, or seemed to do so; the temperature fell, nor did it again rise; and, save a rapid feeble pulse for another fourteen days, she made an uninterrupted recovery. She is now in perfect health save only a retroflexed fundus and a slight rectocele; can walk miles, and go about her domestic duties with comfort and pleasure. I am firmly convinced my patient was poisoned by the nurse, and I am convinced the serum saved her life. I do not confuse this case with fever due to retained placenta, such as we so often have sent to general hospitals, which may need removal of the generally decomposing placenta, and which always, in my experience, recover; such cases do not need serum-therapy, for the cause is different. Yet cases are often reported where the uterus is freed from the remains of the placenta and serum is also used, and to the latter the credit of the cure is wrongly assigned. In my experience in the one case you have simply to remove the cause for the cure to follow—in the other an antitoxin alone, and, with rare exceptions nothing else, will effect a cure.

EIGHT YEARS' MIDWIFERY PRACTICE IN NEW ZEALAND.

By J. R. Purdy, M.D. Aberd.,
The Hutt, New Zealand.

During eight years—commencing on 22nd March, 1891, and ending 6th March, 1899—I attended 453 cases. Records of all cases have been kept. Unfortunately, in the disastrous floods of 1898 I lost my 1896 casebook, and so far as that year is concerned have only the total number of cases attended. As the statistics seemed to me to open up many questions of interest to the profession, I thought I might this year, at Brisbane, fulfil a promise made three years ago at Dunedin, and read a paper on my results, to the profession.

Excluding 52 cases attended in 1896, I have records of 401 cases: 66 were abortion cases at from sixth week to seventh month; 335 at full term. Forceps were applied 160 times. Eighty-five primipara were attended, forceps being applied 64 times; 75 cases were over before arrival.

There were eight cases of ruptured perinea, three being in same patient. Two happened before my arrival; one shortly after my arrival, in a primipara, delivered naturally. (I was called in when at a medical case up country.) This was the worst rupture of the eight. Leaving five cases of ruptured perinea in 160 forceps deliveries, with the exception of one, a woman of twenty-four with a big child with well-developed head, all were over twenty-nine years of age, one patient being forty-two; all primiparæ with the exception of the one ruptured three times. All cases were stitched up with silk or catgut immediately, and with the exception of one did well.

There were seven cases of post-partum hæmorrhage. In four, the child was born before my arrival, so that there were only three cases in which (to use Playfair's words) "that most trying and dangerous accident" occurred after forceps or other helped delivery of these three. One, a primipara, had uterine inertia before she was seen, one had been in labour twenty hours, and one was a case of twins with ear and left shoulder presentations in a patient aged forty-one. There are no statistics available to show the frequency of post-partum hæmorrhage; but I feel sure many cases would have flooded but for early and prompt interference. Nearly all such cases are due to inertia of uterus—a condition that should, if possible, never be allowed to occur.

I believe in prompt examination of the uterus after the child is born. If all is well, I wait until pulsation in cord has ceased before separating the child from the mother, and then immediately remove the placenta by Crède's method. Of course as soon as child is born I keep my hand on the uterus and try to induce contraction. Should Crède's method fail, I have no hesitation in passing my hand into the uterus and removing the placenta. This method of procedure is condemned by very many authorities, but in my hands I have never had cause to regret using it, and very many times have felt I have prevented a profuse hæmorrhage.

There were three cases of placenta prævia, three foot presentations, three breech presentations, two hand and head.

CASE 64, 1898.—One very curious presentation of both feet and head. In my twenty years' experience I had never seen such a presentation, nor can I give any reference to it in any book on midwifery. Certainly in my case it was a most puzzling presentation, the feet feeling exactly like a tumour attached to posterior lip of cervix.

Six occipito-posterior; 2 face; 1 left shoulder; 1 transverse; 1 brow and face; 1 ear; 1 brow; 2 hydrocephalic monsters, on one of which craniotomy was performed, the other was delivered by turning; 2 craniotomies—one the monster already mentioned, the other not strictly a case of mine and not included in my total, as it was a case I saw in consultation; 7 high forceps operations; 3 cases in which Walcher's position was used; 2 cases of puerperal fever, both precipitate labours, and neither seen by me until three days after delivery. Both recovered. (One in a girl of thirteen.) One woman had goitre, child had also a large goitre which disappeared in three weeks; 2 ante-partum hæmorrhages.

Turning was performed thirteen times, for following conditions:—1 head presentation, difficult to apply forceps, being very high up—in this case I erred in trying to save the child, but I was anxious for many reasons to do so; 1 hydrocephalic monster dead; 2 hand and head; 2 high up presentations, where forceps slipped; 1 occipito-posterior; 3 foot and head; 2 ordinary presentations, but delayed labours, called in casually when far up country, and, having no instruments or $CHCl_3$ with me, had to turn; 1 feet and head.

Seven cases of twins with following presentations:—3 normal; 1 ear and left shoulder; 1 head and head and arms; 1 breech and head; 1 over before arrival.

Six children had been dead some days before delivery. Three children died; one a high-up presentation already mentioned; one in same patient, a foot presentation—she had been thirty-six hours in labour when seen, the membranes had ruptured early, and all liq. amnii drained away; one, a foot and head presentation, very difficult turning operation, child above average size.

Here let me remark that in my district the children are decidedly above the average weight laid down in the text-books.

No mother died either near or remote, and, with the exception of the three children already mentioned, no children died in birth. One died on the eleventh day, one on the fourteenth day; both hand-fed. No children were disfigured.

I very rarely saw, what at one time was most common—a caput succedaneum. I have had practically no trouble after labour, except in a few cases when I was not there before the child was born.

I have had no case of puerperal eclampsia, although I saw one in consultation—a very bad case; patient recovering after injection of pilocarpine.

I am well aware that a record of 453 cases is not enough to base statistics upon; only, as I have been twenty years in midwifery practice—I attended my first case in 1879—and, with the exception of one case of puerperal eclampsia at seven months, have never had a death, having attended over 2,000 cases, I think I may claim my record, dating back so long, when puerperal mortality was much greater than at present, is a very fair one.

Now, the question arises: To what do I attribute my escape from maternal deaths, and, moreover, the rapid recovery of my patients, the total absence—for I think I am justified in saying so—of any septic results, and the very slight mortality amongst children?

Firstly.—I am scrupulously careful to keep myself as clean as possible—you must pardon me for mentioning this, but I remember being hauled over the coals for omitting to state this in a so-called paper on antiseptic midwifery I read before the Leeds and West Riding Medico-Chirurgical Society in 1888. I am a great believer in asepticism. Latterly I have used Jeyes' fluid, but up to two years ago I used nothing but hot water. I do not believe in lubricants such as vaseline. I use Jeyes' fluid because it is lubricating to a certain extent; in future I intend to use lysol. I clean my forceps myself, and keep them wrapped in an ordinary diaper. They are always put in a jug of boiling water at least ten minutes before use. I make as few examinations as possible; if labour progresses satisfactorily I wait; if there is the slightest delay I apply forceps; of course, very many of my patients live up country, and I am only sent for when the natural means seem to be exhausted. I never allow nurses to examine patients either before or after labour. I never allow a nurse or anyone else to douche a woman after labour. I have fought about this for years, and am glad to see the profession are now giving it up, although most reluctantly. Binders are not put on, as I am certain they cause a good deal of mischief. Seeing the routine habit of bandaging immediately after delivery prevents the accoucheur from examining the uterus properly by manual touch through abdominal walls, I always say, if asked, "I will not have it put on until after twelve hours; after that you can put it on if you wish." As a matter of fact, binders are hardly ever applied afterwards, and if they are, are put on in such a loose manner that they might for all practical purposes be dispensed with. In spite of this I am sure my patients have a greater percentage of neat figures than the average. I never give ergot during labour. As soon as it is over I give 1 drachm P.D.'s ergot and five more doses at increasing intervals of one hour, sometimes mixing each dose with a tablespoonful of brandy.

I never leave a patient until pulse is normal and I get cricket-ball uterus; once that happens I leave with perfect confidence, even twenty minutes after delivery.

I allow no visitors under any pretext for one week. I allow my patients to sit up in bed on fourth day, and to get up on tenth day. I caution them to be extremely careful for six weeks. I generally give a purgative on second night. Usually Fordyce Barker's P.P. pill.

The term "milk fever" is never heard in my district now. I have only had one abscess of breast caused through the ignorance of a most incompetent woman.

In conclusion, let me say I am quite sure that it is a mistake to wait for Nature to do its best or worst before interfering. All the gynæcological cases I have seen after labour in my practice have been in patients who for some reason or other were not interfered with, had precipitate labours, or were not seen until after child was born. I see absolutely no reason whatever why forceps should cause any trouble if carefully and skilfully applied, and I could cite very many cases where early application must have prevented trouble. As a matter of fact, many practitioners use far too much extracting force. It is astonishing how little force one needs to apply if forceps are properly applied and tractors used. I have over and over again applied forceps not to hasten but to retard delivery—thus, I am sure, saving many a perineum from rupture. I have always condemned taking off the forceps when head is on perineum. I have always considered that was the most important time for their use, and I am glad to see modern authorities writing strongly on the point. Nature is not always careful, and the natural pains often force down the head without it taking the proper turn. Early diagnosis and patience are two main factors in the safe delivery of women in childbirth. The forceps should always be applied in the bi-parietal diameter of the head. This is nearly always practicable, but as a matter of fact the personal equation always must come in in labour, and each case must be judged on its own merits. Axis-traction forceps should always be used. Personally, I prefer and always use Neville's, having always found them most satisfactory. A standing rule ought to be that forceps should not be applied until the os is fully dilated; certainly if applied before os is fully dilated, very little force must be used, and the forceps should then be looked upon more as dilators than tractors.

Dr. Worrall (Sydney) said he complimented Dr. Purdy upon so ably treating this important subject. There was much to praise and endorse in his paper, but there was one point which he must take exception to and adversely criticise. That was the undue hastening of the third stage. No other part of a confinement was so important as this. His advice was to wait so long as there was no hæmorrhage, and not to prematurely force out the placenta.

Dr. Hardie (Brisbane) said he agreed with Dr. Purdy in all respects with the exception of one point. He did not approve of the use of the forceps at first. He did not use them so often now as he did ten years ago. One important objection to their use was that if he applied them to the head it was impossible to use them between the pains. If they were used during the pains probably the cervix would give way. He was very pleased to hear Dr. Purdy condemn douching after labour. When he first left college he used to practise this, and his cases were never out of fever. Now it was fifteen years since he had used the douche, and fever had entirely disappeared.

Dr. Davenport (St. Kilda) said he was quite in accord as regards douching. He had seen cases of sudden death through the practice, and two cases of fever which occasioned great anxiety, although eventually the patients recovered. He had given up the practice of douching altogether wherever the medical attendant was in charge of the case. But sometimes cases were in the hands of nurses who would act differently. But an idea had got abroad that if there was no douching the patient was badly used. "No douching," they would say, "the doctor does not know his work." If there was a fœtid discharge, douching would not remove it. Something more must be done. They also should wait for the delivery of the placenta by natural means. They should not wait until three contractions, and never with ordinary pains should there be any difficulty in pressing down the uterus and expelling its contents. Upon all these points he entertained very strong views.

The President said he was extremely gratified to find on this question of obstetrics so many medical men in harmony. It showed that there was a sound system at the base, and that all of them worked hand in hand together. He only referred to the practice of douching to condemn it entirely. It had been condemned by Schröder and all the leaders of the advanced school, and Schröder's imprimatur was sufficient to kill it. He emphatically condemned it under any circumstances. It was a most serious mistake to force a strong current of water into any injured tissue. Regarding the application of forceps, he took up the same position as that of Dr. Purdy. Those using them should know all about them, and those who did would say there was no safer method in parturition. To save the perineum could only be done by the judicious use of the forceps, and this was in every way preferable to the time-honoured method of pressure. Binding he also disapproved of. A great deal of damage to women in their confinement was done by tight binding. The midwife of the Sarah Gamp stamp used to earn her living by this. If she was unable to keep up the figures of her patients, she lost her reputation and means of livelihood. *Ergo*, she developed tight binding to the fullest extent. The result was that in many instances the uterus was forced into the pelvis, and afterwards proved a fruitful source of trouble. Dr. Purdy was not entitled to the claim that he was the originator of dispensing with binding. The Dublin School of Obstetrics were again to the fore in this matter. They originated doing without binding, and Dr. Purdy would have to divide the honours with them. He congratulated Dr. Purdy upon the pronounced success attending his efforts as an obstetrician. They were now at the end of their labours in this Section, and he must thank the members for their kind attendance. It would be very gratifying to their secretary, Dr. Byrne, to see so much interest displayed in gynæcological subjects.

Dr. Byrne said: As the Queensland representative of the Congress in this Section, he would like to say a few words before they separated. On behalf of the President of the Congress, Dr. John Thomson, he extended them a most hearty welcome. The Queensland members had worked hard to make the visit of the Southern members a success—(applause)—not only socially but intellectually. Being the first visit of the Congress to Brisbane, he was most anxious that it should pass off well, and if it had done so he was only too glad to know it. It was a great pleasure to the Queensland representatives that among the visitors they had such distinguished specialists in gynæcology as Dr. Foreman, Dr. Worrall, Dr. Thring, and the President, Dr. O'Sullivan. He would always look back pleasantly to their visit and the opportunities it afforded for the renewal of friendships made in his early days in Sydney. The Queensland representatives, too, would gain intellectually in their knowledge of gynæcology and obstetrics. By being enabled to meet men of mark and fame in their daily work, knowing them and seeing how they got on in the matter of practice, patients would not be so prone to blame the local men and say the Southern specialists recommended different action, though our local men could say now, "We know Dr. Worrall and Dr. Thring, and we know that they did not say so-and-so." He would only add, in conclusion, that he welcomed the specialists amongst them, and also all those who had endeavoured to make the Congress the success it had been.

Dr. Purdy proposed a hearty vote of thanks to the President, Dr. O'Sullivan, for his very capable occupancy of the chair, and also to Dr. Byrne for his equally capable carrying out of the executive duties.

This was carried by acclamation.

The President thanked them very heartily for their kind expression of approval. He did not want this testimony for any little effort he had put forth. Their active interest and considerate treatment proved beyond doubt that they had taken every means to make the Conference a success.

SECTION OF PUBLIC HEALTH.

PRESIDENTIAL ADDRESS.

By J. Ashburton Thompson, M.D. Brux., M.R C.S., L.R.C.S., D.P.H. Camb.,
Chairman of the Board of Health, Sydney.

I make my grateful acknowledgements to the Council of the Congress which has seen fit to call me to this Chair; and if the remarks to which I now venture to ask attention should fall below what is properly expected in a Presidential address, I beg that the defect may be ascribed to engrossing affairs rather than to lack of consideration. They concern the guidance of public effort towards the further prevention of consumption.

It is probable that mention of this theme forecasts to you a twice-told tale; but it may be that some things worth attention still remain to be said upon it, and the occasion appears to me to be this. Whatever may have been done by States already towards prevention of consumption, and whatever may have been already done by the charitable to alleviate the hard lot of the consumptive poor, it has lately been perceived that an unoccupied field remains in which organised associations of citizens may usefully engage. Accordingly, in several parts of the world, and in Australia no less than elsewhere, such associations have been formed. Now, in Australia it does not yet appear clearly that any definite line of action has been decided upon by executive committees. Prevention has been mentioned by them; but the practical step contemplated appears to be establishment of hospitals or homes for the treatment of consumption. These two notions of prevention and of treatment are not incompatible, it is true; but they are distinctively different in the aims they indicate, in the kind of hope they permit, and in the methods they imply. No doubt the establishment of a hospital for the treatment of consumptives is a legitimate and praiseworthy exercise of benevolence. But the new associations have their origin in a new motive, which consists in the conviction now generally entertained that the disease itself can be eradicated from mankind; or, to speak more prudently, that its present extravagance can be reduced to sober limits. There scarcely can be question, therefore, of the course they should take; they should concentrate their energies on prevention. But the prevention of consumption covers a very wide field in which the State, the medical profession, and the laity, acting under direction and in accordance with knowledge, have each their special place; and, as regards the latter, it is timely and useful to inquire what their place is:—

I.—In order to present the factors in the problem of prevention with some approach to clearness, it is necessary to review briefly what has been learned in the past; for consumption is a disease which has been earnestly and persistently studied with success during more than a hundred years, and consequently several facts and sound opinions, often thought to be new, are in reality but our heritage. A hundred years ago its ravages were much what they are at the present day, unless the statistics of that time warrant a belief that they were somewhat greater; for according to Woolcombe and to Southey, physicians who wrote during the first fifteen years of the century, phthisis carried off annually in Great Britain a fourth, in Paris a sixth, and in Vienna a seventh of the population. The now well-known local incidence of consumption was observed by Young, again quite at the beginning of the century. In his "Treatise on Consumptive Diseases," he pointed out that Bristol was the most heavily stricken place in England, and that the deaths were those of the inhabitants, not of visitors to the city; and he remarked that it had been chosen as a resort favourable to consumptive persons who hoped for recovery, as though in defiance of the natural indications. Yet the communicability of consumptive disease had been taught from antiquity; first by the fathers of medicine, and in every successive age by some among their more distinguished disciples—needless to mention their names. The failure of an opinion possessed of such vitality to win general acceptance was accounted for by Cooper, in 1840, on grounds which may fairly be said to correspond with those which the Report of the Collective Investigation Committee of the British Medical Association appeared to warrant in the minds of the majority in 1883—namely, that the usual prevalence of consumption was such as allowed the instances of its apparent communication between closely associated persons to be better accounted for on the doctrine of probabilities, than by appeal to an unestablished quality of the disease.

The well-founded teaching of the present day, that predisposition, and not heredity, is of importance to the maintenance of consumption, has also been anticipated. Velpeau and Breschet recorded their fruitless search for tubercles in the bodies of the still-born children of consumptive parents; while both Orfila and West testified that though they had occasionally found them in similar circumstances, it was but rarely. Hence it was inferred that in most of the numerous cases where tubercles were found in the bodies of young children, the process by which they were formed had originated after birth, parents transmitting to their progeny only the tendency to this kind of diseased action, and very seldom the disease itself. This opinion, it will be noticed, might have been given yesterday; and it was based originally on precisely the same class of observations as it is based on now. Perhaps it may be supposed that the special liability of young children to suffer elsewhere than in the lungs, and particularly in the lymphatic ganglions of the abdomen, is a novel observation, made only after the knowledge that tuberculosis could be communicated by the milk of tuberculous cows had been acquired. But it was recorded by Andral and by Laennec, and their results even go to support the doubt which has lately been raised by a long series of careful dissections, whether the mesenteric glands are primarily affected in a really remarkable proportion of cases. They pointed out that children were disposed to develop tubercle in a larger number of organs than were adults: they named the parts most likely to suffer in adults as first the lungs and then the small intestines; but in children, as first the bronchial glands, then the mesenteric glands, then the spleen, then the kidneys, and lastly the small intestine. Again, the present-day treatment of consumption consists in a liberal diet, fresh air and rest, or rather, carefully regulated exercise. But towards the latter part of last century, Salvadori was famed for his successful treatment by a liberal diet and gentle horse exercise; and then invalids flocked into the Tyrol to place themselves under his agreeable regimen, as now they flock to many places on the continent of Europe with a similar object.

Thus many of the clinical and pathological observations of a century ago, and of the intervening years, were sound. But although our knowledge of that kind has been extended, and although we have lately learned the nature of that specific condition in the absence of which there can be no tubercle, our treatment of the disease has not been lifted thereby from the contingent stage towards the heights of scientific certainty. *Post-obit* dissections teach, and long have taught, that many persons recover; but the most judicious physician of the day can no more foretell in which cases his art will succeed than could his remote forerunners.

But regarding the indications for prevention which were revealed by the same succession of venerable teachers, nothing of that kind can be said. Andral's list of the secondary causes of consumption needs no amendment. In his time, diathesis was regarded as all-important in relation to this disease. Yet he had the distinguished merit of pointing out that the tubercular diathesis need not be innate, but could be acquired; and the list of conditions under which it was likely to be acquired was given by him as follows—residence in impure air, or such as is not renewed often enough; crowding of many persons together; absence of the sun's rays; an habitually damp atmosphere; insufficient food; and excesses which exhaust the strength and nervous influence. It was as far back as 1843 that Baly published his celebrated essay on the "Diseases of Prisons," in which he showed, from the experience of Millbank Penitentiary at London, that imprisonment was there a cause of phthisis and of scrofula in persons who were healthy on reception. Further, he showed by inquiries made at other prisons of Europe and of the United States, that this influence of the then usual conditions of prison life was commonly manifested; and he concluded by defining its components as deficient ventilation, sedentary occupation, want of active bodily exertion, and a listless frame of mind. Again, the Barracks Commission, appointed to inquire into the causes of the vastly greater incidence of pulmonary affections on soldiers confined in barracks, than on persons at similar ages among the civil population, in 1858 reported their opinion that in civil life insufficient clothing, insufficient and unwholesome food, sedentary and unwholesome occupations, and the vitiated atmosphere of unhealthy dwellings, all contributed to the propagation of this class of diseases. But they pointed out that the food, clothing, and occupation of the soldier were not of a character to justify the imputation that they were among the causes of the heavy incidence of consumption on the troops, which was ascribed accordingly to "the vitiated atmosphere generated by overcrowded and defective ventilation, and the absence of proper sewerage in barracks"—to filth communicated directly and indirectly to the air.

Both of these inquiries showed, then, that the excessive incidence of the disease was intimately connected with defective ventilation, or, in the already quoted words used by Andral many years earlier, to "residence in impure air, or such as is not renewed often enough." We know that these lessons were at once applied to diminish the incidence of phthisis on the classes of the population just mentioned; and with such success that not merely did the excessive incidence cease to occur as soon as the supply of fresh air was brought much nearer to that which physiological science had shown to be requisite, but in the case of prisoners the disease itself may be said to have been eradicated, since at well-ordered gaols it no longer supervenes in those who are free from it on their reception.

Again, it was in 1860 that Greenhow, acting as a medical inspector under the Privy Council, made his first report on "Excessive Mortality from Lung Diseases," in which he inquired into the causes of observed variations in the district-prevalence of that class of maladies. He showed that the cause of variation was related to the nature of the industrial occupations in which the inhabitants were engaged, but more closely to the conditions under which they were obliged to carry on their work; and even that in different localities the morbidity of the male or of the female population was higher, according as men or women worked under the harmful conditions in the larger proportion. The proximate causes were precisely those assigned by Andral in the list already mentioned, and legal powers to obviate them were given in several Acts of Parliament, which took effect in the years from 1864 to 1867. Lastly, in connection with the historical period to which these remarks are confined, the independent investigations of Bowditch, in the United States (1862), and of the late Sir George Buchanan, in England (1867 and 1868), may be cited, which established the connection there is between damp subsoils and high phthisis death rates.

To these acute observations, and to practical adoption of the lessons taught by them, that steady diminution in the prevalence of phthisis which has gone on in England and Wales during the last forty-five years is attributed. Other and possibly even less direct causes have also been removed or modified; but they are still such as were mentioned by Andral. Their gradual diminution has been an outcome of those mechanical inventions, and of those altruistic sentiments which have lately given the poorer classes cheaper food of better quality, together with higher wages and more leisure, and which have acted with greater and greater effect in each successive decade of the last half century. For, if we still recognise a special proclivity to consumption in the offspring of consumptive parents, and in persons of a certain constitution, we are also better aware that a proclivity to it can be acquired, or, as we now say, that the function of natural resistance to the virus can be impaired. Baudelocque, and before him Andral. as we have seen already, had noticed this; and the latter, while pointing out that persons of what has been called the Venetian type were specially susceptible, remarked that often the disease also attacked persons of the opposite type—of dark complexion, jet black hair, and strong muscular development, if they were placed in favourable circumstances.

Thus, after having had our attention directed to conditions attaching (it may be said) to the house, and to the site on which it is built—to the danger which lies in dampness, darkness, filth, and a stagnant air not less foul in its kind than the soil of unsewered urban districts; now a glimpse is caught of the value of the personal factor in determining the occurrence of consumption. To-day, we see reason for supposing that the specific cause can scarcely operate in sound bodies though taken into them.

II.—The brilliancy of modern discoveries blinds incautious eyes to old truths, and often endangers sound practice. The tendency at the present day is to lay too exclusive a stress on the importance of conserving the expectorations of consumptives. The specific nature of tubercle was practically unknown down to the latest date thus far mentioned; nevertheless much had actually been done towards diminution of the customary death rate due to consumption. From this fact we are to infer that the causes of disease are seldom single; and in relation to applied hygiene it may be said that they never are single. It is true that the causes of consumption thus far mentioned can be strictly classed only as adjuvant or secondary: but in preventive medicine they are entitled to much greater consideration, and have a much higher practical importance then the scientific classification denotes. In themselves they have no power to cause consumption; and, conversely, the disease can occur in their absence, though only under circumstances seldom encountered in the ordinary course of life. Hence, in the environment in which consumption is prevalent, and in which it has to be fought, preventive schemes must fail which do not take cognisance of them all—of external conditions as existing in the dwelling, and of internal conditions as related to the nourishment and strength of the body, as well as of the virus itself.

We have now reached that epoch in the ætiology of consumption which was marked by Villemin in his paper on "The Causes and the Nature of Tuberculosis," which he presented to the Academy of Medicine of France in 1865, and which paved the way for the last and decisive discovery to which in this brief review there will be occasion to refer. Villemin stated the conclusions which his work warranted in quite unmistakable terms. They were—

(1) That tuberculosis is the effect of a specific agent, and, in fact, of a virus.

(2) That this agent must exist in the morbid alterations which it has determined by its direct action on the normal tissues of the body.

(3) That this agent, when introduced into a body capable of being impressed by it, must reproduce itself, and, at the same time, must also reproduce the disease, of which it is the essential principle and the determining cause.

These propositions were for long bitterly opposed, not only by the Academy which, two years afterwards, reported that Villemin had failed to establish his position, but also by foreign observers who adopted a similar conclusion on similar ground. This was, that though what was recognised as tubercle could be produced by inoculation of tuberculous matter, it could also be produced by inoculation of matter which was certainly not tuberculous, and which was, indeed, manifestly indifferent or even inorganic.

Three years later, in his "Studies in Tuberculosis," Villemin went even further than at first, and enunciated the proposition that inoculated tuberculous material does not act by the tangible and visible morbid product which is introduced, but by a more subtile agent contained in it which escapes the senses; he asserted that the body was but a soil in which this elusive agent could grow, just as the inoculated tuberculous products are not the virus, but merely carry it; and he insisted on the close parallel which he traced and demonstrated between tuberculosis and other diseases acknowledged to be infectious. Nevertheless, his further results were no better received by the Academy than those exhibited in his earlier communication; and among contemporaries who repeated his experiments and contradicted his conclusions were some of the most celebrated names—those of Lebert and Weiss, Colin and Fidoux, Burdon-Sanderson, Wilson Fox and John Simon, Cohnheim and B Fränkel, with many others, all of whom were masters in their time. As years went on, however, a larger and larger body of confirmatory evidence, and a larger and larger number of adherents to Villemin's doctrines accumulated. Hippolyte Martin furnished the best proof that tuberculosis could not be caused by inoculation of false tubercule; Chauveau established the possibility of communicating tuberculosis to susceptible animals by feeding; and Tappheiner demonstrated the possibility of communicating it by inhalation. Still, it was Villemin who first expounded and insisted upon the virulence of the expectorations of phthisical persons, and in his work on the "Propagation of Phthisis," he attributed dissemination of the disease to inhalation of dried and powdered spit by the healthy. Thus had been reached by means of that genius for experimental science, and for logical generalisation, which is the distinguishing characteristic of the French intellect, the knowledge of the ætiology of consumption by which we are now attempting to profit.

In order to carry conviction to the minds of the many, it remained only to reveal the agent or virus which, as Villemin had divined, lay hid in the tangible products of the disease-process. This, the crown of studies persistently pursued during more than a century, was achieved by Robert Koch, who, in 1882, was able to show the *bacillus tuberculosis*, and to name it the distinguishing characteristic and essential cause of tubercle. But it took time even for this demonstration to affect the general body of the profession. Perhaps the main argument used by the conservatives was that there was nothing to prove that the bacillus was not rather the result of the disease than the cause of it. But the discussion was not prolonged. The work already done by Pasteur, Toussaint, and by Koch himself, on anthrax and on fermentation, greatly smoothed away many such objections as had been encountered by Villemin.

It may be supposed that then the way had at last been opened for systematic and more precisely directed attempts to prevent consumption. But such efforts are impossible until those who help to form public opinion are either as well informed, or at least as convinced of the possibility of success as the medical profession itself. The public, however, were far from having reached that stage; nor, perhaps was the medical profession, regarded as a whole, possessed of the necessary degree of conviction. It was left for Cornet, of Berlin, to convey this; and if, as has been said, he succeeded only at the expense of arousing a fear of consumptives on the part of the laity which was mistaken, and which must have exposed a portion of the melancholy generation then extant to unnecessary hardship, it may fairly be asserted in reply that future safety could not have been secured at any less cost.

Cornet's share in this arduous business, therefore, had the greatest practical importance. It has already been remarked that Villemin, in writing on the propagation of consumption, had laid especial stress on the danger attaching to the scattered expectorations of the phthisical; and it is interesting and remarkable to note how that distinguished man had, in 1869, laid down those practical rules which at this day, that is to say, exactly thirty years later, are still our guides, and which are now in a way to become the common knowledge of educated mankind. He said in so many words that as long as the expectorations remained liquid they were hardly capable of doing harm; but that if they were scattered on floors they dried, were resolved into dust, and then caused the spread of the disease by inhalation. He pointed out, as places especially liable to become centres of infection by these means, prisons, convents, barracks, workshops, &c.; adding that if in some dwellings the floors are not covered with expectoration and danger from that source is consequently avoided, then the handkerchiefs usually substituted for spittoons, and body and bed linen which had become soiled, and had been allowed to dry, contributed their infected dust to the air when they were carelessly handled and shaken before being again wetted at the laundry. These are the points on which we lay greatest stress to-day. Now, what Cornet did was this:—He furnished the experimental proof that Villemin's teaching was correct, and furnished it in a way so striking and so convincing that, from the time his researches were made known, Villemin's teaching concerning the dangers of dried sputum has been generally accepted. But, besides proving that the dust of places inhabited by consumptives was often infective, Cornet also showed that it was not always infective, and that if the consumptive expectorated exclusively into a spittoon, and if he did not use pocket-handkerchiefs in the usual way, it was free from infectivity. This was just the kind of exception to his positive results which proverbially proves the rule, and establishes it.

Nevertheless, the fact remained—puzzling to those who noticed it—that hospitals for consumptives in which no special attention had been turned to conservancy of the expectorations, still seemed to be places of no especial danger to the healthy staff employed at them. But Koch himself had pointed out that infective sputa lost their virulence when exposed to light and air; and the free aëration and good lighting usual in such places furnished the conditions necessary to reconcile the two observations, and to show that they were but superficially opposed to each other. We are thus brought back to the point mentioned above, viz., that in applied hygiene the causes of disease cannot be regarded as single, though in strict sense the causes of specific disease necessarily are so. If the sputa of all consumptives could be completely gathered and destroyed, we are satisfied, no doubt, that consumption would cease to be the prevalent malady it is at present. But it is clear that such continuous and complete attention never can be secured. The defect, whatever it may be, must be supplied by other means, and one of those means is ventilation and lighting of the dwelling.

But, in practice, even ventilation and lighting alone are not enough. Doubtless, infective sputa are rendered harmless by exposure to light and fresh air, as Koch has shown; doubtless, also, under the most favourable conditions of such exposure they are rendered harmless in less time that in which they can resolve into dangerous dust, as Ransome, of Nottingham, has shown. No doubt it is to good ventilation and lighting that the harmlessness of hospitals for consumptives as places of residence for the healthy was chiefly due several years ago. And yet the practical hygienist is aware that such improvement in those respects as he can effect in old or ill-built dwellings under the conditions in which he has to carry on his work, would be productive of less benefit than it might yield if it were conjoined with improvement of other kinds. The latest labours of Ransome, who for many years has occupied himself with tuberculosis and its prevention, and whose repeated contributions on this subject exhibit that combination of sound scientific method and practical utilisation of its results which distinguishes the English character in such connections, have thrown light on some other conditions which favour continued virulence of the bacillus of Koch after its discharge from the body.

Ransome's observations, made in part with Professor Dreschfield and in part with Professor Sheridan Delépine, may be briefly mentioned as follows. They concerned, in the first place, the time during which, and the circumstances under which, the bacillus retained its virulence after leaving the body in expectorations from the lungs, and supported the following conclusions:—

(1) That finely divided tuberculous matter, such as pure cultivations of the bacillus, or fine tuberculous matter derived from sputum, in daylight and free currents of air is rapidly deprived of virulence.
(2) That even in the dark, although the action is retarded, fresh air has some disinfecting influence.
(3) That in the absence of currents of air, the bacillus retains its power for long periods of time.

And, secondly, they concerned the possibility of the bacillus leading a saprophytic existence after discharge from the body—that is, of its being able to proliferate at ordinary temperatures, and in other conditions commonly met with. They were as follow:—

(1) Any one of various organically charged vapours, whether coming from healthy or diseased lungs, from the air of cellars, or from the air of comparatively pure ground, forms an excellent cultivating medium for the bacillus of tubercle, when kept away from the disinfecting influence of air and light.
(2) This power of promoting its growth is particularly manifest when the supporting substance is common wall-paper, though it is quite apparent when very pure filter-paper is used.
(3) It is further proved that, on these substances, the growth of the bacillus may take place at the ordinary temperature of the dwelling-rooms, and hence that there is no safety against the increase of the bacillus in ordinary living-rooms in which active tubercular dust is present, and in which the natural disinfectants of the bacillus—namely, fresh air and light—are not present in sufficient amounts.

It is right to say that, while these latter results have not been contradicted, they have not as yet been corroborated. On the other hand, you are aware that the class-place of Koch's bacillus moves yearly further and further from the schizo-mycetes, and approaches more and more closely to the hypho-mycetes.

It is scarcely necessary to remind you of the difficulty met with in attempting to cultivate this microbe—that it grows in the laboratory only on specially selected media, and that it strongly prefers special temperatures;

but it should be pointed out, that these difficulties vastly enhance the importance of experiments which seem to show that it can grow with comparative ease apart from special media, and indifferent to high temperature, provided only it be furnished with the foul organic vapour of human breath, or with the foul vapour of damp subsoils. Such vapours commonly pervade much of the poor house property in every city; and in view of common laboratory experience, Ransome's experiments furnish just the evidence necessary to explain the persistence with which infection has been observed to cling to houses uncleansed after removal of the consumptives who first communicated it to them. So that, just as the necessity of attending to the state of the dwelling as regards light and air appeared before, so now it appears that the effort at prevention must not stop at such improvement of that sort as those can alone make who are obliged to live in houses requiring it. Nor would that measure of improvement suffice, though combined with all the care to destroy infective sputa which is practically attainable. More must be done. Drainage of damp subsoils, the sewering of unsewered neighbourhoods, the scavenging of filthy yards and lanes, and isolation of the interior of houses from the ground air, must be secured. Nay, more still; the interior of houses must be kept clean in the sense in which notable housewives interpret that term.

Thus was the ætiology of consumption slowly worked out, and thus, *pari passu* with the stages which marked discovery of the various factors in the complicated problem of its maintenance and diffusion, did we come into possession of the methods of prevention. Our teaching of to-day is based on no happy discovery, suddenly made, nor even on the genius of any single searcher. It is the result of a long-drawn series of practical demonstrations of the agency of diverse but correlated conditions. It advocates no nostrum. It inculcates the importance of conserving and destroying the expectorations of consumptives, but not that this alone is sufficient. The preventive scheme must have as many branches as there are diverse contributory causes; hence its execution requires proportionate knowledge, intelligence, and persistence. Be it added, sacrifice also; for at present it falls not within the province of compulsion, but of diplomacy.

III. The more important facts in the ætiology of consumption may be briefly stated as follows:—

First, the cause of infection is specific and single; those who suffer from the disease are the source of infection to others; those who are suffering from consumption of the lungs alone give off the infection in quantity, and in ways which have great importance to others; they give it off almost entirely in their expectorations. Secondly, the infection can maintain its virulence for long periods outside the body from which it has been thrown off; it gets access to others by the air, on which it is borne to them; it follows, consequently, that the common mode of infection *is not direct, from person to person, but indirect*, and that localities can be infected by the sick, and can retain their infectivity in absence of the person who has infected them. Thirdly, the infection is easily destroyed by sunlight and fresh air after it has been thrown off, and consequently special conditions are necessary to its long-continued virulence outside the body; special conditions of the uninfected body are also necessary to successful invasion of it; these two sets of conditions may be regarded as practically one and the same, for in the lack of fresh air and light, and in the dampness and the dirtiness which are necessary to long-continued virulence of the infection, the healthy body deteriorates, loses its powers of natural resistance, and becomes a prepared victim of the disease. These being the ascertained ætiological facts, they are also those on which preventive measures must be based. They indicate that cognisance must be taken of consumptives themselves, of the locality in which they contracted their illness, of the houses they occupy or have occupied, of their relations to others living in the same dwelling, and of the constitutional state of these latter with reference to the probability of their having or of their retaining a full measure of natural resistance. On consideration it will be perceived that these points mark the whole area covered by social life, or at all events, covered by town life; and phthisis is distinctively a disease of cities. It is in cities, as Professor Verneuil has usefully pointed out, that phthisis is more common, takes graver forms, progresses more rapidly, heals or even improves more slowly and in a smaller proportion of cases, and that slight attacks continue longer and more frequently tend to become grave; while, on the other hand, in country life remissions are often observed, and a chronic course which is often equivalent to recovery. The inference is, as we have already seen, that the general conditions of life in cities are the cause of the great prevalence of phthisis, and not mere propinquity of the sick to the healthy; and it follows as a corollary that the share which a Government can take in extirpating tuberculosis from any city it does not own must be limited. It can but legislate on broad lines, and leave the detail to be filled in by the people themselves. It is conceivable that some Governments might deal with all consumptives as we here deal with lepers (who, by the way, at the very worst reckoning, are far less dangerous than consumptives), and with all house property in a manner which is certainly desirable on a dozen counts connected with public health, but which owners would probably regard as destructive of all their rights. The death rate from phthisis, as far as we can see at present, would by those means be reduced very rapidly, and its reversion to the proportions it has at present would be prevented. This would be the direct course; but we know that the first of these speculative measures is impossible, and that the second is only not impossible. Nevertheless, though that which the State can do is necessarily limited, it is essential. Let us first consider it; then it will be easier to discern what remains in which some private association could usefully engage. In the first place, there can be no doubt that phthisis should be made a notifiable disease (as has been done already in South Australia), but notifiable only to central, and not to local, authorities. No other form of tuberculosis should be notifiable, and the register should be declared strictly confidential as regards names. Next, the State should provide for the free bacteriological examination of sputa; and this also should be a strictly confidential transaction between medical men and the central health authorities. Thirdly, it should make the disinfection of rooms from which consumptives have been removed, or in which they have died, before they are reoccupied by any person, obligatory on owners and occupiers of houses, and should require such disinfection to be done to the satisfaction of a legally-qualified practitioner of medicine, who should be required to certify to it in writing. Fourthly, district registrars should be required to notify every death from phthisis as soon as registered by them. Fifthly, good building laws should be enacted, and steadily enforced. Much beyond that I do not think the State should go, at all events, at first; but the central health authority should forward by post some brief pamphlet of instructions to the persons whose names are notified to it. That, I dare say, seems to some little enough for a Government to do. But it contains what is essential. It furnishes the means of learning what the number of sufferers is, and the facts as to incidence of the disease on classes, occupations, and localities; and this knowledge is the indispensable foundation of practical measures of prevention. In order to remove the all-important secondary causes of consumption most expeditiously and most economically, the faulty spot must be identified, and occurrence of cases of the disease furnishes the best index to them. Try to imagine, for a moment, a police charged to control unruly characters, but entirely ignorant of their appearance, habits, haunts, and journeyings. It is scarcely possible to do so. Yet that is the position of most States in relation to living consumptives among the population. The information furnished by a well-ordered scheme of notification, after being carefully compiled, abstracted, and charted, would in a few years furnish the requisite guide to effective action, and by being communicated to the public would gradually render effective action more completely possible. At this point it seems necessary to interpose a few words on the notification of phthisis, at which many medical men, and among them several whose opinions command respect, look askance. The objections suggested are diverse, and I venture to discriminate them into two distinct classes. One class comprises objections to notification itself, or rather presumed difficulties in the way of it; the other, objections which in reality have nothing at all to do with notification, but which relate to the uses it is apprehended might be made of the information furnished by it. For purposes of discussion and agreement that seems to be a distinction of some importance. In the former class are comprised difficulty of diagnosis, and reduplicated notifications of the same case. As to difficulty of diagnosis, a small proportion of notifications would, doubtless, be made in error. But it would surely be unpractical to renounce the benefits which notification can certainly yield, because a few cases, prematurely thought to be in the earliest stage of disease, would be wrongly returned, and because a few others, undiagnosable with certainty during life, would not be returned. After all, in the vast majority of cases, phthisis is recognised—even apart from bacterioscopy of the expectorations—with greater certainty than many other common diseases. As for repeated notifications of the same case this would be certain to occur rather often with a disease which follows a prolonged course for the most part, and in which sufferers commonly appeal to several different medical men for help. But this is merely a matter of money, and probably of a comparatively small sum; for the really important danger which attaches to it—that of falsification of the register, and, consequently, of the practical indications drawn from it—can very easily be guarded against. It cannot be supposed that the Government of any State would be deterred by the incidental risk of spending a thousand pounds a year unnecessarily from endeavouring to prevent a disease which costs it a thousand lives a year. The second class of apprehended difficulties fades away as soon as the uses to be made by the State of notification have been defined as they have been defined already above. The whole proceeding should be confidential. States have little to do with individuals; in this matter they could not act successfully except on the general principles by which States are usually guided in legislating. Lastly, on this topic it must be said that to profess to wish that phthisis were prevented—to admit its communicability from the sick, to admit that consumptives are the main cause of its maintenance and diffusion, to admit the facts of local incidence and of house infection (all of which things I suppose we all acknowledge)—and then to oppose notification on any ground short of demonstrated impossibility, seems to be sheer capriciousness or sentimentality. Suppose the State to have taken the action just described, does anything remain which might be done at once, and which is peculiarly within the province of a private association? I think there are two main things, and there are some which are subsidiary. The two things are these, and, though they may perhaps seem identical, it is very important indeed that they should be kept distinct—(*a*) provision of hospital accommodation, and (*b*) prescription of the terms of admission thereto. I spoke at first rather slightingly of hospitals for the treatment of consumption; and it is the case that, in relation to prevention, institutions primarily designed for the treatment of infectious disease deserve little consideration. The two functions are not mutually exclusive, of course; far from it. The difference between an isolation hospital and a general hospital really lies in the qualifications for admission. An association for the prevention of phthisis must provide itself with a hospital; but it must be primarily a segregation hospital. The qualifications for admission to it must, therefore, be—not alone the fact of suffering from phthisis *but the ascertained fact of living, while so suffering, under conditions which necessarily* *involve danger of infection to others.* This point will be recognised as vital to preventive schemes as soon as it is mentioned; and, therefore, no more need be said about it. I pass on to mention the subsidiary matters which may well engage the attention of such an association as being of a kind which cannot be transacted by officials of the State under a law; and I do so without staying to consider whether the State might not properly be expected to furnish the segregation hospital. Perhaps it might; but though it may probably contribute towards the cost it seems to be recognised that it is very little likely at present to provide one. The subsidiary matters are these: In order to decide which cases among all those which present themselves, or which are presented for admission to hospital, properly fall under the rule mentioned above, the association must visit many of the sufferers in their houses. We are here on delicate ground; but a discreet director and discreet visitors can doubtless be found. In many cases the visitors' report will require amplification or support by a statement of the case made by a medical man; and for this, it is not altogether unnecessary to add, a fair fee should be paid from the association's funds. Another matter is the dissemination of information. It is very well to distribute leaflets and the like; in some connections I have employed them very freely in New South Wales, and our poster on prevention of consumption hangs before you. But simple as such documents usually are, and entirely free from difficulty to the highly educated, there are very many people, and not alone among the uneducated, who are unable to put written advice to practical use without some verbal explanation and some slight demonstration. With the task of providing explanations, advice, and encouragement would naturally be combined that of furnishing spittoons for indoor and outdoor use to those who were unable or who seemed unlikely to buy them for themselves. There is yet one more thing that discreet visitors could do which eventually would be of high service. They should be possessed of sufficient knowledge of healthy house construction to record the character of all houses in which they found consumptives living; or, if they have it not, the association should provide means for instructing them in it. This, together with some record of the ravages often produced by the disease in families, would, if carefully compiled and published, do much towards that education of public opinion through which sound progress in a matter which in many relations of detail is both delicate and somewhat difficult can alone be made. I do not lay great stress on these subsidiary matters. I have no doubt that the main object of such an association should be to endeavour to secure hospital accommodation for all those whose continued presence at home necessarily involves danger to the rest of the household, and to take care that no others are admitted until all of them have been provided for. Observance of this rule would leave full scope for the treatment and management of those who were admitted; but it will be agreed probably that the treatment of consumption, though a charitable, and, in a measure, a useful work, should not be a prime motive with an association for the prevention of consumption. Lastly, every such association should take especial pains to avoid being mistaken for a charitable organisation; that imputation would be fatal to its efficiency.

It is not most usual to frame an address to be delivered on such occasions as the present on a single subject. Nor have I done so, for the prevention of phthisis can be accomplished only by approaching perfection in the "art of living in towns." Once more, I repeat, viewed from the standpoint of practical hygiene the causes of consumption are seen to be many; the disease is maintained and diffused by indirect and not by direct communication between the sick and the healthy; the conditions necessary to indirect communication lie in our surroundings, and if all those surrounding conditions which are requisite to the maintenance and diffusion of phthisis were removed, the general death rate from all causes out of the natural course would be remarkably diminished.

ISOLATION OF INFECTIOUS DISEASES BY LOCAL AUTHORITIES.

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At the request of the energetic secretary of this Section, I have ventured to write a short paper on the "Isolation of Infectious Diseases by Local Authorities," more with the view of inviting discussion and eliciting information from others on this important subject rather than with any idea of contributing original matter.

From the earliest times we read of isolation being practised in some form or other. When plague and pestilence visited the ancient cities, as many of the healthy as could escape fled, leaving the sick and helpless to die either of hunger or the plague. It was seldom the fugitives, through fear, ever returned, and in this way many cities became depopulated and fell into ruins. Equally inhumane was the practice of casting out the sick and leaving them to die. This was adopted more especially with lepers. We are told it was a command to the children of Israel that "they put out of the camp every leper," for "he shall dwell alone, without the camp shall his habitation be." Even during the reign of Edward III. a decree went forth "that lepers were within fifteen days to betake themselves to places in the country, solitary and distant from the city." There are no passages in history more pathetic than those recording the religious services with which lepers were bidden to inhumane exile. These afflicted beings were not only regarded as unclean, but with horror, as bearing the brand of a curse from heaven. In the present age man foresaw that neither of the above methods could be resorted to if the well-being of the community was to be conserved, and that the proper way to deal with infectious diseases was to remove the sick from the healthy into some special place where relief could be afforded the sufferer and protection to the community.

When quarantine was first established in 1403 by the proveditors of Venice on an island near that city, the first lazarette was built. It was at first, however, only used for persons suffering from plague, but, as quarantine laws spread over the civilised world, cholera and yellow fever were added to the list of quarantinable diseases. Inland "pest houses" were subsequently established on the lines of the lazarette system for diseases such as smallpox and typhus fever.

The measures controlling these pest houses were so vigorous that any person escaping from them was liable to suffer death as a felon without benefit of clergy.

But few of the infectious diseases as we now know them ever reached the pest house, and it was not till the beginning of the present century that any attempt was made to isolate these diseases in special wards or hospitals.

Many of the first hospitals used for this purpose were either erected during the panic of an epidemic, or were constructed and maintained by private subscription for the promotion of recovery of the poor. In this way they were designated and known as "houses of recovery."

The great and grand institution known as the Metropolitan Asylums Board (London), created in 1867, was originally charged in pursuance of the powers conferred upon it "to make provision for the reception and relief of poor persons infected with or suffering from fever, or the disease of smallpox, or who may be insane." It was soon found, however, that a large proportion of the patients admitted were not of the pauper class, and representations were made to have the hospitals made available for all classes of the community. In 1889 an Act was passed giving the authorities power to admit any person into their hospitals who is not a pauper and "is reasonably believed to be suffering from fever, smallpox, or diphtheria."

This Board, instead of looking after the poor only, has become the controlling power of all the infectious diseases of the mighty metropolis of London with its population of 4,500,000 of people. The history of the Metropolitan Asylums Board hospitals is similar to that of many others. The pest houses, or houses of recovery, have gradually been replaced by the isolation hospitals of modern times. It is a matter of regret, I think, that legislation does not compel all local authorities to make provision for the proper isolation and accommodation of the infectious sick of their districts. Officers of health, and more especially those who are in a position to advise the various Governments, should lose no opportunity to suggest the advisability of amending the Public Health Acts so as to make it obligatory instead of permissive for local authorities to provide the requisite isolation accommodation. The adoption of the Notification Diseases Act was at first optional on the part of local authorities, but more recent enactments, such as we have in the Colonial Public Health Acts and the Public Health (Scotland) Act, make the notification of infectious diseases compulsory, and rightly so; for if notification is to be effectual it must be compulsory. What applies to notification of infectious disease is equally applicable to the providing of hospital accommodation on the part of the local authorities. All the length that either our Home or Colonial Health Acts go is "that local authorities may provide hospitals as temporary places for the reception of the sick, and two or more local authorities may combine in providing a common hospital; where such hospital is provided, they may order the removal of any person who is suffering from an infectious disease and is without 'proper lodging or accommodation.'" Although permissive, it is gratifying to find in England there are about 500 hospitals supported by municipal rates, and under the amended County Councils Act we may expect many more. In Australia little or no attempt has been made by local authorities to provide isolation hospitals. In Sydney the Little Bay Coast Hospital is a Government institution; and in Melbourne a combined effort by the various local authorities of the city and suburbs with the aid of a Government subsidy will result in the erection of the Queen's Memorial Hospital for infectious disease.

In Tasmania I have advised that the various local authorities of the southern part of the island should combine for the purpose of providing one common hospital, but up to the present nothing has been done owing to the disputed responsibility by the local authorities and the Government. The local authorities say that the Government maintains and supports the general hospital, and, if so, why not an isolation hospital for the infectious sick? This seems a sound argument enough so far as the class of cases known as Government cases is concerned, but the central authorities argue that powers are given to the local authorities to erect and maintain their own hospital from the funds of the rates, and between the two the matter is still in abeyance.

One marvels indeed at the apathy alike of the people and the Government in this important matter, for surely the most important duty either a central or local authority is called upon to do is to use every means of defence in their power to prevent sickness amongst its people. How can this be done if no means of isolating the infectious sick are provided? With regard to the action of Governments in these colonies, the health of the people is about the last thing considered. In many of the colonies a far greater attention is paid to the inspection or quarantining of imported cattle than to matters relating to the health and well-being of the people.

The necessity of providing means for the isolation of the infectious sick is obvious to you all, and the suppression of epidemic disease largely depends upon such provision. While private individuals and philanthropic effort have done much for the establishment of general hospitals, the construction and maintenance of infectious disease hospitals are rightly considered to be the duty of the local health authorities. It is to be hoped that ere long that local authorities will be public-spirited and progressive enough to recognise that isolation of infectious diseases is as much their duty as the removal of insanitary dwellings, &c.; for it is patent that, unless isolation accommodation is available, much of the good expected from compulsory notification will not be realised.

Admitting, then, that isolation of infectious diseases is imperative if we wish to control epidemics, what diseases should be isolated by local authorities, and how? An ideal system of isolation would mean that every dangerous and infectious disease should be isolated in a hospital, and that one kind of infectious disease must be isolated from another, rather than as is usually the case—the isolation together of two or three of the more important diseases.

But to carry out strictly an ideal system would mean the expenditure of an enormous sum of money, an amount beyond the capabilities of local authorities. In practice, therefore, we must be satisfied with only a small percentage of the infectious diseases being treated in hospital. The list of infectious diseases has steadily increased, and is now a pretty formidable one, including cholera, smallpox, plague, yellow fever, leprosy, typhus, typhoid and scarlet fever, diphtheria, measles, whooping cough, chicken-pox, mumps, erysipelas, puerperal fever, tuberculosis, rabies, &c.; and while it may be desirable to have all these diseases notifiable, it is impracticable for local authorities to provide hospital accommodation for them, and he would be an enthusiastic sanitarian who would advise that they should. We must, therefore, be moderate in our demands; otherwise we will probably fail to get anything. Speaking from experience, we can deal with these infectious diseases as consisting of four groups in these colonies—

Group I.—Mumps, chicken-pox, whooping cough, rotheln.

Group II.—Cholera, leprosy, smallpox, yellow fever, and plague.

Group III.—Measles, erysipelas, puerperal fever, and tuberculosis.

Group IV.—Scarlet fever, diphtheria, typhoid and typhus fever.

GROUP I. may be called the home group, for no one could seriously advocate the isolation of these minor diseases, however much we feel convinced that hospital treatment will be in the interest of the sufferers. No doubt a goodly number of victims are claimed year by year, especially amongst the infant population, by these diseases, and careful nursing such as would be available in a hospital would tend largely to reduce the mortality.

GROUP II. may be designated in these colonies as a Government group; with the exception of leprosy, the diseases of this group are always imported, and in this way come more directly under port health officers. The separation of persons suffering from any of the diseases of this group from the healthy must be prompt and complete. As far as I know, all the colonial Governments have considered it their duty to make provision for this group of diseases; and when a Federal Public Health Department is established they will no doubt continue to be controlled by the central or federal authority.

It is fortunate for local authorities that this group is controlled and maintained by the central authority, inasmuch as each of the diseases composing it is best treated in a separate building, and the necessary expenditure required would be beyond the means of any local authority.

Cholera, though never likely to become epidemic in the Australian continent, may be introduced any day by one of the Eastern trading vessels, and in the event of an outbreak special temporary hospital provision would require to be made. Such a hospital of necessity must be erected at the seat of the outbreak for two very good reasons—namely, the rapid course of the disease, and the risk of removing the patient to any distance.

Leprosy has always been dealt with in a very special way, and the lazarette system and the cruel treatment of the lepers of old are known to you all.

Smallpox should only be accommodated in buildings which are removed from populous centres and at some distance from human habitation. The necessity for this has been well shown by the valuable investigations of Mr. W. H. Power on the influence of the Fulham Smallpox Hospital on the neighbourhood surrounding it. It was clearly shown and proved from Mr. Power's inquiry that there had been an excessive incidence of smallpox in the neighbourhood of the hospital as compared with other parts, and the percentage of houses invaded became smaller as the distance from the hospital increased. We need not pause to consider how the infection travelled: sufficient for us to know that its influence "by some means or other" was felt for a distance not exceeding two miles. For these reasons it is impossible to have a cholera and a smallpox hospital in close proximity. The other diseases of this group are so rare in the colonies that we need not consider them here.

GROUP III.—In this group we have a number of diseases about which much discussion might take place as to whether or not a local authority would be justified in providing hospital accommodation at the expense of the general ratepayer.

Measles indeed has given rise to much controversy as to whether it should be one of the compulsory notifiable diseases, and whether sanitary measures can to any extent control it. Whatever may be the decision arrived at as regards hospital isolation, I believe it is well for the authorities to be aware of the existence of measles in order that some precautions may be taken against the spread of it by the infected. Opportunity is thus given to the authorities to issue slips of instructions which, if they serve no other purpose, have an educating influence over the mass of the people. Those who oppose the hospital isolation of measles do so because they say the infective stage is over before the patient can be removed to hospital owing to difficulty of diagnosis, &c. But while this acute stage of infectivity precedes the full development of the disease, it must not be forgotten that measles, like scarlet fever, is also infective in the later stages.

Measles is a disease of short duration, and the rapidity of its spread over a very large area would, I fear, prevent the necessary hospital accommodation being provided. The contagium of measles is feeble, and its spread through clothing not so active or potent as that of scarlet fever; and hospital treatment is, therefore, not of great value as a preventive measure, but is of the utmost importance from a humane point of view, for undoubtedly measles mortality is great if care is not exercised in the nursing of the sick.

Erysipelas.—In some municipal hospitals cases of this disease are admitted, and of late years it has been brought under the powers of local authorities as an infectious disease. It is indeed questionable how much good is to be derived from the isolation of patients suffering from erysipelas, beyond that which might be attained at home. To the individual sufferer hospital treatment may be eminently beneficial, but how far it affects the well-being of the community is quite another matter. The late health officer of Glasgow expresses the opinion, "At all times we are without the satisfaction of belief in the preventive necessity or utility of the work. Antiseptic surgery and the refined conception of cleanliness which has extended therefrom in all forms of surgical procedure and into hospital management probably has more to do with the prevention of fatal erysipelas than anything else."

Dr. Russell is such an eminent authority that his words are worthy of serious consideration.

Puerperal fever is one of the latest diseases brought under the Notification Diseases Act, but it is arguable how far notification or isolation can either secure benefit to the patient or safeguard the public. The only precaution to be taken is in regard to the doctor or midwife attending. Until we get some system of registration of midwives, I fear much effort expended will bring little result. Medical men in attendance upon patients in the puerperal state should be their own officers of health. There could be no objection, if the patient so desired it, to removal to some isolated wards where good nursing would be available, but to insist upon hospital treatment would, in all probability, have a most disastrous effect upon the favourable progress of many of the cases. For these reasons I think local authorities should not be expected to provide hospital accommodation for puerperal cases.

Tuberculosis.—In connection with this disease South Australia is the first colony to compel the notification of it to local authorities. This step has been taken, no doubt, with the view of assisting the local authorities to stamp out the deadly enemy of the civilised world. Since Koch discovered the bacillus of tuberculosis and the disease became known as a preventable one, health authorities have been inspired with the hope that it may yet be eradicated. In spite, however, of all that has been done to reduce the mortality by removal of insanitary dwellings, preventing overcrowding, securing better drainage, with more careful inspection of milk and food supplies, this disease contributes about one-seventh of the total deaths. The discussion on the treatment of this disease by the open-air method in sanatoria specially constructed will, it is hoped, result in the adoption of means which will lead to a diminution of the mortality. That this disease should be treated in special hospitals or sanatoria I am fully convinced, but I am also of opinion that these sanatoria will require to be maintained by private effort or at the most subsidised by the Government; local authorities will not be able to make provision for these cases, owing to the great expense of the prolonged treatment required.

Group IV.—This group might be called the municipal group, for I think there can be little doubt as to the necessity of isolation, and of the responsibility of the local authority in providing the requisite accommodation. To my mind a local authority not making provision for isolation of the infectious sick of scarlet fever, diphtheria, typhoid, and typhus fevers is not carrying out the spirit of the Act to which it owes its existence. As these diseases, with the exception of scarlet fever, are the results of disregard of sanitary laws, it is reasonable to expect the local authority to provide for the sickness it neglects to prevent.

Scarlet Fever.—With reference to scarlet fever it may be said that the isolation of infectious diseases in many municipal hospitals resolves itself into the isolation of this disease alone. It is the one disease that a local authority ought to make hospital accommodation for—not only because of the diminished fatality amongst hospital treated cases, but because the isolation of a scarlet fever patient removes a focus of infection and a menace to the public health. It is satisfactory to know, therefore, that while we are thus acting in the interest of the sufferer we are safeguarding the public weal. During the three decades ending 1890, the mortality of scarlet fever in England and Wales fell from 1,133 to 600, and then to 350; while in 1894 it was only 167 per 1,000,000, living. In truth the decrease of mortality is shown by statistics to be universal; nevertheless, the great reduction in England, at any rate, is largely due to hospital treatment. If it were possible to ascertain the mortality of home and hospital-nursed scarlet fever, the case fatality of the home-nursed would doubtless be found to exceed by a considerable amount that of hospital-treated. The M.O.H., Nottingham, states that the average percentage of deaths for 1892, 1893, and 1894 was—in hospital cases, 3·2; home-treated, 9·1.

In 1895 it was 3·4 and 11·4 respectively. In Glasgow, in 1874, the death rate of this disease was 17 per cent.; and in 1894 was 4·9 per cent., under hospital treatment. In the same city the total number of deaths from the disease was, during the period 1880 to 1884, 1,862—equal to a rate of 725 per 1,000,000; 1890 to 1894, the total number was 1,108, equal to 347 per 1,000,000. For the first period, 508 deaths, or 27 per cent.; for the second period, 729 deaths, or 66 per cent. of total deaths took place in hospital. That hospital isolation of this disease is generally approved of is evidenced by the large increase in the number of cases admitted into the Metropolitan Asylums Board hospitals. The number rose from 1,949 in 1880 to 6,539 in 1890, or 43 per cent. of all cases; in 1894, 63 per cent.; and in 1898, 74 per cent. of all cases notified were admitted. In 1891 Brighton was isolating 70; Glasgow in 1894, 89; and in Nottingham in 1895, 93 per cent. of all cases notified.

The need of isolation is indeed very apparent when we consider how the infection is disseminated. The aggregation of children in schools, workrooms, or factories is largely responsible for the spread of the disease. It is also conveyed by milk directly or indirectly.

The specific contagium is a micro-organism the life history of which we are as yet not very familiar with.

Crookshank states the virus is contained in the secretions of the mouth and throat, and is given off by the skin in desquamation and possibly by urine. Klein has been able to isolate an organism from the secretions of the mouth and throat which he calls the *Streptococcus scarlatinæ*. The distinguishing characteristics of this organism from the *Streptococcus pyogenes* are: That it coagulates milk in one to three days; secondly, that it turns litmus (blue) milk red, showing that it is an acid producer. He is confident that this germ is as diagnostic of scarlet fever as Löffler's bacillus is of diphtheria.

Up to the present Klein has not, however, been able to find it in the desquamation nor in the urine or ear discharges, but he adds, "It was found to persist and even to occur in abundance in the throat long after these persons had to all appearances completely recovered from their illness." The rule that no child should be considered free of infection until the peeling stage has passed is one that ought to be strictly adhered to, for even if the virus is not given off in the skin the infection persists in the secretions until or even after the desquamation has ceased. Further investigation on this subject by such an authority will be watched by officers of health with interest. It is just possible that the organism is capable of living outside the human body, and in this way epidemics or pandemics may be explainable.

Diphtheria is not so highly infectious as scarlet fever, but it is one of the diseases requiring hospital care, because of the great benefit of early treatment to the sufferer, and the extreme importance of protecting the other inmates of the household. It is generally found in insanitary dwellings where comfort and good nursing cannot be had, and the removal of patients thus suffering to well-ventilated wards, with every essential of treament, must largely increase the chances of recovery. The antitoxine treatment so highly prized has now become almost routine practice in hospital. To be effectual it must be promptly carried out, and this is not always convenient or possible in the rounds of the general practitioner.

In 1894 there were admitted into the Metropolitan Asylums Board hospitals 3,042 cases, with 902 deaths, or 29·6 per cent. of cases treated. In 1896 there were 4,175 cases, with 871 deaths, or 20·8 per cent. of cases treated. Antitoxine, then, is credited with saving 365 lives in the metropolis, assuming the death-rate of 1894 to have been maintained in 1896.

Antitoxine treatment is most effectually carried out under hospital condition of asepsis, &c. The evil effects of aggregation of individuals is evidenced more in this disease than almost any other, but it is impossible to prevent these in small private houses where large families are crowded together. The atmosphere of a room in which a diphtheria patient is lodged is almost certain to become infected by the germ being coughed up, and carried about in the form of dust. Of course, this could be prevented by a proper disinfection or destruction of the excretions, &c., but this is the ideal which is never realised. The provision of hospital treatment of diphtheria means the saving of lives, and no question of mere cost should deter local authorities from doing their duty where the health and lives of their constituents are concerned.

Enteric or Typhoid Fever is one of the scourges of the Australian colonies, and local authorities should use every effort to eradicate this terrible disease. It is endemic in every capital, and frequently becomes epidemic. The medical profession recognise that hospital treatment has such an important effect in reducing the mortality of this disease that, at a great pecuniary loss, they insist upon their patients going into a hospital; it is so in Tasmania, at any rate. Hitherto the general hospitals have been admitting all cases of typhoid, but when you consider that the prevalence of this disease is the premium paid for want of or bad sanitation you must admit that local authorities are not without responsibility if they fail to provide the requisite hospital accommodation. Many people resent the idea of going into a general hospital, which savours more or less of a charitable institution, but with a municipal hospital, supported and kept by the rates, there would be no such feeling. The highest as well as the lowest would not feel that there was any imposition on charity, but rather that he was acting with the very best of motives—namely, self-preservation and protection to others.

By municipal hospitals taking control of typhoid patients, charitable institutions would be greatly relieved—leaving more space available for the care and treatment of general diseases of the poor.

Nurses could also be selected, who, by age or previous attack of typhoid, were more or less immune for special typhoid wards. Year by year we hear of the younger nurses in general hospitals, where typhoids are admitted, being struck down with this disease.

Typhus fever, should it ever visit Australia, would demand prompt and effectual treatment by the local authorities. I think, however, little account may be taken of it in designing an isolation hospital in these colonies. Believing, then, as I do that local authorities should provide isolation accommodation for such diseases as classed in Group IV., I will briefly deal with the objections raised, as I know them, by local authorities, and consider how far the objections are valid ones, or how we may overcome them. Four objections are generally raised—

(1) Expense is too great.

(2) Hospitals do not provide for or cannot cope with an epidemic.

(3) Difficulty in securing a site in a suitable place owing to the detrimental effect hospitals have on the surrounding neighbourhood.

(4) Want of compulsory removal—which means that all cases are not treated alike.

The expense involved in the erection and maintenance of an isolation hospital is the chief reason for local authorities refusing to make this necessary provision; but when the advantages of such an institution are set against the expense of maintaining it, then, I believe, the balance will be in favour of the hospital. The question should not altogether be considered as one of finance, but rather as a means of saving lives, preventing sickness, &c. Experience in public health work tells us that in 80 per cent. of the cases of preventable diseases it is either impossible or impracticable to isolate patients in their own homes. Indeed, it is futile to attempt it; but beyond this there is a class of people in every community that has to be provided for in times of sickness. I refer to the inmates of hotels, boarding-houses, the homeless, and indigent poor. If no hospital accommodation is provided, where are these people, who may suffer from infectious disease, to be treated? The presence of fever in a hotel, boarding-house, milk shop, or dairy practically means ruin to the owner if the Public Health Act is rigidly carried out. Here, then, is a direct loss which may never be recovered. Consider the position even amongst the well-to-do or the industrial class. If one of these diseases visits the home, children have to be kept from school for weeks, though they may not be infected. Business people suffer loss through restrictive measures, and because the public are afraid to go near the place. Workmen, in addition to the great expense imposed on them on account of the sickness, frequently lose their employment because, forsooth, the employer is afraid of the infection. Here, again, we have a direct loss of business and stoppage of work, and an indirect loss through the reduction in spending-powers of the householder.

With an isolation hospital in readiness, all this loss is avoided; the moment a case is notified, the patient is removed with the infected clothing. The house is disinfected and cleansed. Other members of the family are comparatively safe, the business is not interfered with, and the children are again permitted to go to school. Visitors are not driven out of hotels—or it may be the city—only to regret that they ever visited it. Surely, then, these advantages, together with the knowledge that these hospitals prevent disease and reduce mortality, are sufficient to recompense any local authority for the expense involved in founding and maintaining such an institution. Much opposition to spending money is owing to the fact that the advocates insist upon having such palatial buildings for an infectious sick. Now, I am one of those who believe, in order that a hospital for isolation purposes may have the desired effect, it should be a permanent, well-constructed, and attractive building. There must not be a semblance of pauperism or charity about it, otherwise only the poorest and those under the strongest compulsion will be induced to use it. It is no easy matter to reduce the cost of these hospitals and keep them up to the standard which we desire to see. Dr. Wynter Blythe, however, thinks that a reduction in the cubic space from the orthodox 2,000 to 1,560 might be adopted without any injurious effect. He considers, especially with scarlet fever, where 72 per cent. of the cases are children under ten years of age, this could be done.

The question of maintaining these isolation hospitals might be considered here. Now, though most Public Health Acts in the colonies permit any local authority to provide places for the reception of the infectious sick of its own district or jointly with another local authority, and to charge the cost of the same to the municipal fund, I am of opinion that the State should subsidise to the extent of at least £1 for £1. This, I understand, is provided for in the New South Wales and Western Australian Public Health Acts. Where it is inexpedient or impracticable for one local authority to establish an isolation hospital, the central authority should have power to compel adjoining local authorities to combine for this purpose, each paying per capita of the population of their district.

With a subsidy from the Government, local authorities would be able to do away with the much-vexed question of payment of fees by patients. It is essential that an isolation hospital should be absolutely free to the citizens who support it. With the knowledge that fees are charged, objections are raised to removal, and the very cases we desire to isolate cannot be induced to go. If an infectious case is removed by the influence of the health authorities for the public good, the public should see that no injustice is done to the individual by demanding a fee which may be a great hardship to pay. For five years the authorities in Glasgow charged fees, kept ledgers, rendered accounts, &c., until the whole absurdity of distinguishing between infection in paupers and in other people became so palpable that on 23rd April, 1881, a resolution was passed—"That all classes of the citizens suffering from infectious disease should be treated in hospital without any charge being made therefor." If anyone desired extra accommodation, there could be no objection to him having it, provided he paid for it.

I feel confident a hospital run on the lines suggested would, to a very large extent, remove the objection of expense, and would secure the sympathy and support of every taxpayer, and when the public appreciate the sentiment so well expressed by Dr. Russell—"that prevention is the aim and the *raison d'être* of the hospitals of sanitary organisation, and the evidence of the success of prevention, in so far as isolation is concerned, of a diminishing total quantity of disease existing"— we may expect to hear very little of the objection of expense.

2. Hospitals do not provide for epidemics. It should be impressed upon local authorities that these hospitals are not intended to provide for but rather to prevent or control epidemics. The success or failure in preventing the spread of infectious disease depends very much upon having isolation accommodation in actual readiness.

In Dr. Thorne's excellent report it is stated: "If a hospital is hurriedly built under panic, it is often not ready for occupation until the immediate cause of its erection has passed by; it provides accommodation of a very indifferent sort; it fails almost without exception to meet the permanent requirements of the districts, even when in amount it turns out to be more than the district needs, and thus the object of the hospital as a part of the sanitary defences of the district is often attained in a very imperfect manner and at a needlessly larger cost. On the other hand, the hospitals which have been erected during the epidemic periods, and with a view of preventing epidemics by having in actual readiness means for the isolation of first cases of infectious disease, afford as a rule excellent examples of the kind of isolation provision which all sanitary authorities should possess, and this report will give plentiful examples of their success in their intended object."

It is not, perhaps, an easy matter to convince local authorities that these hospitals do prevent epidemics, but after the most elaborate inquiry the report so much quoted from goes on to say, "I could occupy you for hours in telling you instances in which epidemics have evidently been prevented by the isolation of first cases of infectious disease."

Indeed, unless we have early notification and prompt isolation of first cases, scarlet fever is almost certain to spread, owing to the number of mild cases that are allowed to go about quite unknown to the health authorities. The utility of an isolation hospital must not be judged by the number of cases treated, say, during an epidemic, but rather, if it were possible, by the number of cases the occurrence of which it prevented by affording isolation of sporadic cases. While hospitals do not, therefore, provide for epidemics, it is well to have sufficient ground attached to the permanent building to enable additional temporary buildings to be erected, if at any time necessary.

3. Difficulty in securing a suitable site is often experienced by local authorities. This is owing to the popular idea that isolation hospitals contaminate the surrounding neighbourhood and depreciate the value of property. This is the fault of the public believing what has not been established in experience. With the exception of smallpox, there is no danger to the neighbourhood from the aggregation of infectious cases in hospitals well constructed and carefully managed. Dr. Thorne "was unable to ascertain, as a result of every inquiry, that any spread of infection had resulted in the vicinity of any of the hospitals visited, which, apart from an attack resulting from a visit to patients under treatment, could in any way be attributed to hospital influence."

The infection of scarlet fever, diphtheria, and typhoid fever has not, so far as known, been spread through these hospitals, and it has been clearly shown that, with the exception of smallpox wards, isolation hospitals may be built with safety in close proximity to dwellings. It is suggested, however, that a space of 40 feet should be left between the boundary of these buildings and the public thoroughfare. It is generally desired that these hospitals should be removed to some isolated spot at a distance from the centre of population intended to be served, but there are two objections to the adoption of this course—(1) the difficulty of getting parents to allow their children to be removed beyond a distance at which they might make inquiries personally; and (2) the extra expense in providing vehicular traffic as well as the difficulty of procuring medical assistance in urgent cases. I would have no hesitation in recommending the building of an isolation hospital in the centre of the district for the above reasons, provided always that means are available to shut off all ingress and egress except by the recognised gateway. The question of isolating infectious cases in general hospitals has been much discussed by my own Board of Health; this is advocated purely on economical grounds. This method of isolation, though still adopted in some of the Continental hospitals, is practically given up amongst English-speaking communities. Where fever cases are admitted into general hospitals it is only into special pavilions separated from the general building. It is much better, however, to have a separate building and administered by a separate staff. Disease has been spread, and lives sacrificed, by the admission of infectious cases into the same building as general cases of sickness. This is illustrated by the experiences of Pendlebury Hospital. Dr. Thorne showed, though this is a well-constructed hospital with isolation wards in the same buildings, that in the year 1878 fourteen patients in the ordinary wards contracted scarlet fever and three measles; eight patients being treated for measles developed scarlet fever. In 1879, twelve contracted scarlet fever and three measles; three measles patients contracted scarlet fever. In the same year, out of thirty nurses four, who were not occupied in the fever wards, contracted scarlet fever. In 1880 twelve in ordinary wards contracted scarlet fever, and three measles patients contracted scarlet fever.

Figures such as these are convincing proof that infectious cases should not be treated in the same buildings as general diseases. It is concluded, therefore, that the experience of this hospital should show the necessity for a complete severance between those portions of a general hospital into which a number of infectious cases are admitted and the other hospital buildings. But with infectious disease hospitals it is necessary to have large convalescing grounds which are never available at general hospitals owing to their central position in big cities and sparsity of ground. Besides, fever hospitals must have their own laundry, disinfecting chambers, ambulance, &c.; and all these require such room as is scarcely available in the grounds of a general hospital. Besides, we should not think of present needs: future requirements might demand increased accommodation, and sufficient ground should be available for any reasonable extension that might be required.

4. The last objection raised is that we have no means of compelling isolation. Some authorities hold that without compulsory removal isolation is useless. Now, I think I can prove to you the contrary. The law allows the sanitary authority to remove such cases as are "without proper lodging and accommodation," and this clause has recently been interpreted in the most liberal way by Mr. Justice Day and Mr. Justice Laurence in the case of Graham *v.* Warwick (*B.M.J.*, 11th June, 1899).

In this case the Workington Corporation applied to the magistrates for an order to remove a boy suffering from scarlet fever. The patient was one of a family of seven, and resided in a four-roomed house, and was well nursed, &c. The magistrates refused to grant the order because the words "without proper lodging and accommodation" must be construed with regard to the wants and welfare of the infected person. On appeal the court held that the words applied not only to the welfare of the infected person but also to the protection of the public, and that the magistrates ought to have granted the order. With this decision to guide us little more is required in the way of compulsion. I am inclined to think, however, it would be better if instead of the magistrates the local board of health had the power to order removal of infected cases.

Compulsory removal is more or less of a bogey, and indeed it is seldom ever resorted to. The large percentage of cases treated in hospitals without compulsion is evidence that the community is alive to the advantages of hospital treatment. Where removal is likely to be enforced is in connection with some business, factory, milk shops, &c., and if removal is opposed the business may be closed if any danger exists of the disease being spread.

In Glasgow, during 1895, 89 per cent. of all existing cases of scarlet fever were removed to hospital without having to ask for a warrant once a year. "The secret is to have a well-managed hospital with a good reputation for the kindly treatment of the children, to manifest human sympathy with parents in their desire to cling to their children, but to be firm, and let it be evident that the firmness is the firmness of reason and not that of mere despotism." We are not likely to get any further powers in this direction, and we would do well to abstain from asking them. Briefly, then, I submit that isolation of the infectious diseases is desirable in all cases, and absolutely necessary in some.

Group I.—including smallpox, cholera, yellow fever, and plague—should be dealt with by the federated Australia under a federal system of public health. Leprosy will be provided for by the State.

Group II.—consisting of whooping cough, chicken-pox, mumps, and rotheln—must be dealt with in private homes, and the people should be educated by printed instructions how to prevent the spread of them.

Group III.—composed of measles, erysipelas, puerperal fever, and tuberculosis—should not at the present time, in my opinion, be isolated in a hospital provided and maintained by the funds of the municipal rates.

Group IV.—The diseases, scarlet fever, diphtheria, typhoid and typhus fevers, included in the group, should be treated in a hospital provided and maintained by the local authority, with a Government subsidy on the lines already suggested.

Objections.

1. The expense is more than compensated, because of the direct gain.
2. Site. Excepting smallpox, none of the infectious diseases have been spread by hospitals.
3. Isolation hospital only intended to prevent and control not to provide for epidemics, and if necessary temporary extension could be made.
4. Compulsory removal is a bogey, and seldom ever required. Obstinate cases can be dealt with in so many ways through business, employment, &c., that opposition to removal is useless.

Having dealt with the necessity of isolation of the infectious diseases by hospital accommodation, I should be glad to hear the views of others with regard to the planning of the various hospital blocks, administrative offices, mortuary buildings, disinfectant chambers, &c., and the class of material to be used in their construction ; the advisability of having a bacteriological laboratory ; the constitution and mode of election of the board of management ; the staff required, including ambulance service ; and the probable cost of construction and maintainence per bed of such an isolation hospital.

DISCUSSION.

Dr. Wilton Love (Brisbane) thanked Dr. Sprott for his paper. He referred to the local epidemic of scarlet fever, which he was glad to say was passing away. He said that the first function of a local authority was the conservation of public health. As a rule, however, these authorities thought more of the making of roads and of keeping them in order as being the proper outlay for the rates. He advocated compulsory notification, isolation, and hospital treatment. The cases of scarlet fever admitted into the Victoria Park hospital numbered 1,000, and the total death rate from the epidemic was well over 100. Lately the local authorities had begun to feel the pinch of the contributions they had to make to this hospital, and the powers that be had given effect to a recommendation that patients should be discharged before peeling had ceased. Instead of remedying matters, this had been the means of keeping it going.

Dr. Tilley (Warwick) strongly recommended isolation of cases of scarlet fever, and typhoid especially.

Dr. Bright (Hobart) agreed to the necessity for the isolation of cases of scarlet fever, which should be treated in special hospitals. An attempt was being made in Tasmania to have cases of typhoid admitted in the general hospital, but he and others were strongly opposing the movement. (Applause.) There was great danger of discharging patients from hospitals until the period of desquamation had ceased. He moved—

> "That isolation hospitals should be opened as separate establishments, apart from and not adjacent to general hospitals, because of the extreme danger of infectious diseases being communicated to the other patients."

Dr. Davenport (Melbourne) seconded the motion. With regard to notification, he said that in Melbourne this was done very grudgingly. They got a shilling per case for reporting. He thought the health officer should be a man supplied by the Government at a liberal compensation, but that he should not be obliged to support himself by indulging in private practice.

Dr. Espie Dods (Brisbane) supported the motion. He said that 200 cases had been lately turned out of the hospital in Brisbane in the peeling state after being under treatment there for about three weeks. He considered at least six weeks, or perhaps eight weeks, should be the period of isolated treatment. He believed that the Board of Health should have taken up the matter of tuberculosis, instead of a separate society. That board should have larger powers, and should be backed up by the people.

Dr. Brockway (Brisbane) raised his voice against the treatment of scarlet fever in Brisbane. Patients should not be discharged until desquamation had ceased. He advocated the imposition of a special rate by local authorities for public health purposes. He supported the motion.

Dr. Doyle (Newcastle) agreed with the motion, but thought it was, or ought to be, absolutely unnecessary if boards of health were properly constituted.

After slight discussion the motion was carried.

It was also resolved, at the instance of Drs. Sprott and W. Corry—

> "That in the opinion of this Section the compulsory notification and isolation of infectious diseases by proper hospital accommodation is of such importance to the health of the community, as well as to the best interests of trade and commerce, that this Section respectfully submits to the attention of the colonial Governments the desirability of providing such accommodation in the various centres of population."

THE DIPHTHERIA MORTALITY OF THE THREE PRINCIPAL AUSTRALIAN COLONIES FOR THE PAST FIFTEEN YEARS, WITH SPECIAL REFERENCE TO THE INFLUENCE OF ANTITOXIN ON THE DEATH RATE.

By A. Jefferis Turner, M.D. Lond.,
Brisbane.

The antitoxin treatment of diphtheria was introduced into Australia at about the beginning of 1895, and almost immediately came into general use. In the city of Brisbane, for instance, there have been since then very few practitioners—the principal exceptions being those engaged in certain forms of club practice—who would omit to administer an injection of serum to a case of diphtheria. A few cases may have been so treated during the closing months of 1894, but I believe not sufficiently many to exert any appreciable influence on the mortality.

Judging from the great reduction in the case-mortality of the disease in the numerous hospital reports which have appeared since the introduction of the new treatment, it becomes interesting to inquire whether the total yearly mortality as recorded by the Registrars-General of the various colonies shows any signs of reduction. It is obvious that, although the more exact details of cases treated in hospital are best fitted to elucidate the problem as to the efficacy of the treatment, its actual value in saving life, under present conditions of use, can only be determined by such an inquiry.

The principal difficulty in this investigation is produced by the varying epidemic prevalence of diphtheria, or, to speak more accurately, of fatal cases of diphtheria, in different years, quite apart from the question of the possible influence of treatment. If we had had notification of the number of cases of the disease occurring in each year, this difficulty would have been, for the most part, removed. In the absence of this we can only deal with it by taking the average mortality of a sufficiently long series of years, so as to eliminate annual variations. We commence our inquiry at the year 1884, and from this to 1894 inclusive, we have what may be termed a pre-antitoxin period of eleven years, which again we have divided into two periods of six years and five years respectively. The antitoxin period consists of the four years, 1895 to 1898, inclusive. It would be convenient to have a longer antitoxin period, but this is all that is available to us at present. For the colonies of Queensland and New South Wales this period appears fairly satisfactory, as the annual variations in the mortality per 100,000 of mean population are not excessive. Contrary to what might be expected, the colony of Victoria, with a population nearly three times that of Queensland, shows much greater variations in mortality. Probably the explanation of this is a climatic one. Queensland and New South Wales, both on the eastern coast of the continent and facing the Pacific, enjoy a very similar climate. Victoria, lying on the south coast and facing the Southern Ocean has a different and much more variable climate. Whether, however, this is a true explanation of the more variable diphtheria mortality of the lastnamed colony is immaterial to our present purpose.

Another difficulty lies in the considerable number of cases registered as deaths from "croup." The great majority of these may be taken as cases of membranous croup, for death from true catarrhal croup is rare. Without entering into pathological discussions it may, I think, be taken as universally admitted that, if not all, at least the great majority of fatal cases of membranous croup are really cases of diphtheria of the air-passages. Accordingly I have included deaths from croup in my statistics, and would point out that if in so doing I have included some deaths not actually due to diphtheria these would tend to obscure, certainly not to exaggerate, any fall in the mortality due to the use of antitoxin, for it is not contended that non-diphtheritic cases are influenced by this remedy.

The increasing proportion of children in the population since the proportional decrease of adult immigration has, perhaps, had some effect on the annual death rate, diphtheria being with few exceptions a disease of childhood; but, although some slight apparent increase in the mortality of the later years of the series might have been expected from this cause, such is not apparent in the statistics, and evidently the comparative influence of this factor has been inappreciable. The varying meteorological conditions of different years has had no doubt a direct influence on the death rate; but, as there still remains considerable obscurity as to the exact weather conditions that are of importance, I have thought it wiser to eliminate this varying factor by the method of averaging, rather than to attempt to consider it in detail.

Treating the mortality records of the colony of Queensland on these lines, we obtain the following table*:—

QUEENSLAND.—DEATHS FROM CROUP AND DIPHTHERIA.

Year.	No. of Deaths.	Mean Population.	Deaths per 100,000.	
1884	135	298,694	45·2	Mean annual death rate per 100,000 living for six years, 1884-1889, 49·2.
1885	208	318,415	65·3	
1886	200	332,510	60·2	
1887	159	354,777	44·8	
1888	156	377,201	41·4	
1889	165	397,061	41·6	
1890	232	414,716	55·9	Mean annual death rate per 100,000 living for five years, 1890-1894, 44·1.
1891	205	404,772	50·7	
1892	176	415,813	42·3	
1893	156	426,798	36·6	
1894	157	438,727	35·8	
1895	96	452,852	21·2	Mean annual death rate per 100,000 living for four years, 1895-1898, 16·4.
1896	60	466,364	12·9	
1897	67	478,440	14·0	
1898	96	492,602	19·5	

Comparing the two divisions of the pre-antitoxin period, we observe a fall in the average mortality from 49·2 per 100,000 mean population in the first six years to 44·1 in the next five. This fall will be seen to be due to the unusually high mortality in the years 1885 and 1886 in the one, and the low mortality in 1893 and 1894 in the other; eliminating these years, the comparison would be reversed, and glancing at the whole of this part of the table it may be said that there is no evidence of any steady fall. From the year 1890, however, when the death rate was high, there is a gradual fall to 1893 and 1894, and these two last years have the lowest mortality of the whole period.

In the antitoxin period the mortality is 16·4 per 100,000 mean population, but little more than one-third of that in either of the two preceding periods. Furthermore it will be noted that a sudden considerable fall occurred in 1895, a second fall in 1896, and that, although there has been a slight rise since, the figures of 1895 have not been reached. It is noticeable that the highest mortality of any antitoxin year is considerably lower than the lowest mortality of any pre-antitoxin year.

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*I must here express my obligations to Mr. Thornhill Weedon, Compiler of General Statistics to the Queensland Government, for his liberal assistance in furnishing me the material on which the following tables are based, and also for checking the calculations.

We now turn to the returns in New South Wales:—

NEW SOUTH WALES.—DEATHS FROM CROUP AND DIPHTHERIA.

Year.		No. of Deaths.	Mean Population.	Deaths per 100,000.	
1884	...	325	883,145	36.8	Mean annual death rate per 100,000 living for six years, 1884-1889, 43·2.
1885	...	582	927,275	62·8	
1886	...	269	969,455	27·8	
1887	...	423	1,004,835	42·1	
1888	...	450	1,035,705	43·5	
1889	...	494	1,066,450	46·3	
1890	...	649	1,101,840	58·9	Mean annual death rate per 100,000 living for five years, 1890-1894, 47·7.
1891	...	653	1,143,580	57·1	
1892	...	458	1,181,475	38·8	
1893	...	578	1,210,510	47·8	
1894	...	464	1,237,410	37·5	
1895	...	283	1,264,660	22·4	Mean annual death rate per 100,000 living for four years, 1895-1898, 18·4.
1896	...	247	1,287,755	19·2	
1897	...	173	1,310,550	13·2	
1898	...	255	1,334,850	19·1	

Here, in contrast to Queensland, we find a higher mortality in the second of the two divisions of the pre-antitoxin period, these being 43·2 and 47·7 per 100,000 respectively. The difference is attributable to the unusually low mortality in the year 1886, which is the lowest of the whole eleven years. From the year 1890, which was one of the high mortality, there is a fall, which, however, is interrupted by a considerable rise in 1893. There is no evidence of any continuous fall during the whole pre-antitoxin period.

In 1895 there occurred a sudden large fall in mortality, succeeded by further slight falls in 1896 and 1897, and a slight rise in 1898. The average mortality of the four antitoxin years is 18·4 per 100,000, being, as in Queensland, considerably less than half that in either of the two divisions of the pre-antitoxin period.

Furthermore, here also the highest mortality of any antitoxin year is lower than the lowest mortality of any pre-antitoxin year.

In Victoria the returns bear a somewhat different aspect:—

VICTORIA.—DEATHS FROM CROUP AND DIPHTHERIA.

Year.		No. of Deaths.	Mean Population.	Deaths per 100,000.	
1884	...	343	932,630	36·8	Mean annual death rate per 100,000 living for six years, 1884-1889, 45·4.
1885	...	332	956,880	34·7	
1886	...	281	984,860	28·5	
1887	...	390	1,016,750	38·4	
1888	...	583	1,054,980	55·3	
1889	...	809	1,090,350	74·2	
1890	...	1,031	1,118,500	92·2	Mean annual death rate per 100,000 living for five years, 1890-1894, 39·8.
1891	...	496	1,146,930	43·3	
1892	...	363	1,162,710	31·2	
1893	...	182	1,170,330	15·6	
1894	...	225	1,174,730	19·2	
1895	...	186	1,180,040	15·8	Mean annual death rate per 100,000 living for four years, 1895-1898, 19·7
1896	...	174	1,177,435	14·8	
1897	...	319	1,172,790	27·2	
1898	...	247	1,172,952	21·1	

If the average of the two divisions of the pre-antitoxin period and of the single antitoxin period be taken, we obtain results closely similar to those observed in the two former colonies. But, on examining the returns of the separate years, we are struck by the extreme variations of the annual mortality in the pre-antitoxin period. This rose to an excessive height in 1889, and reached a climax in 1890, when it far exceeded that attained in any year in the two colonies previously considered. Thence there occurred a rapid fall, reaching a remarkably low level in 1893, from which occurred but a slight rise in 1894. These variations point to great differences of epidemic prevalence in the years considered. This is even more noticeable when we examine the records for the city of Melbourne, which contains more than one-third of the population of the colony. In the year 1890 there occurred 470 deaths from diphtheria in this city; in the year 1893 only 35. The full records since 1890 are given here—

Melbourne and Suburbs.

Deaths from diphtheria (including diphtheritic croup).

1890 ...	470	1893 ...	35	1896 ...	93
1891 ...	172	1894 ...	51	1897 ...	151
1892 ...	100	1895 ...	57	1898 ...	128

The excessive fall shown in the Melbourne mortality from this disease may be partly due to improved sanitary conditions. That it is not wholly so due may be inferred from the rise that has occurred since 1893, which is considerable, though not reaching nearly the height attained in 1890. The variations must be, in part at least, due to meteorological conditions, assisted to a small extent by the fact that the excessive prevalence of the disease in 1890 must have eliminated many of the most susceptible by death, and left a still larger number comparatively immune by recovery from an attack.

Viewing the mortality returns for the whole colony of Victoria in an impartial manner, we may fairly claim that they do not contradict the very positive and striking figures of the other two colonies; but that taking them by themselves a longer series of years in the antitoxin period is required to establish positive conclusions. There have been disturbing factors at work which obscure the results which in Queensland and New South Wales are sufficiently evident.

The following table shows the annual mortality of the three colonies taken together, and also the mortality of England and Wales for the same years as compiled from the reports of the Registrar-General:—

Number of Deaths from Croup and Diphtheria per 100,000 of Mean Population in each Year in the Three Principal Australian Colonies, and in England and Wales.

Pre-antitoxin Period.

Three Australian Colonies.			England and Wales.		
Year.	Deaths per 100,000 living.		Year.	Deaths per 100,000 living.	
1884 ...	38·0	Mean for eleven years, 44·4.	1884 ...	36·3	Mean for eleven years, 31·5.
1885 ...	50·9		1885 ...	32·0	
1886 ...	32·8		1886 ...	28·3	
1887 ...	40·9		1887 ...	30·3	
1888 ...	48·2		1888 ...	30·0	
1889 ...	57·5		1889 ...	30·3	
1890 ...	72·6		1890 ...	28·8	
1891 ...	50·2		1891 ...	26·4	
1892 ...	36·1		1892 ...	29·9	
1893 ...	32·6		1893 ...	38·9	
1894 ..	29·7		1894 ...	35·0	
Antitoxin Period.					
1895 ...	19·5	Mean for four years, 18·7.	1895 ...	31·4	Mean for three years, 31·3.
1896 ...	16·4		1896 ...	34·4	
1897 ...	18·9		1897 ...	28·2	
1898 ...	19·9				

The average mortality during the eleven years of the pre-antitoxin period in these colonies is 44·4 per 100,000 mean population; during the four antitoxin years, 18·7 per 100,000, being considerably less than one-half the preceding. On a population of nearly 3,000,000, this represents a saving during the four years of about 3,000 lives, which, even though liberal allowance be made for possible statistical fallacies, represents a very substantial debt owed by Australia to Professor Behring, of Marburg University.

Contemplating the individual years, we observe a continuous fall in the mortality during the years 1890 to 1894, a fall chiefly due to the returns from the colony of Victoria. The lowest point reached is 29·7 per 100,000. It is observable that in 1886 the mortality fell to a point almost as low, and that this was followed by a continuous rise, culminating in the excessive mortality of 1890. Whether, therefore, a further fall, instead of a rise, might have been expected in 1895 is very doubtful. In any case, the fall which did actually occur was sharp and sudden, and brought the mortality to a much lower point than it had ever before reached, and, what is still more important, to a point which has not been appreciably exceeded since. The highest mortality of any antitoxin year is much below the lowest mortality of any pre-antitoxin year. That some new factor began to exert a considerable effect in lowering the mortality in the year 1895, and has continued to exert this effect since then, appears to be a fair—indeed a hardly resistible—conclusion. If this factor were not the introduction of antitoxin treatment, no other explanation that I know of suggests itself as adequate to explaining the phenomena. If antitoxin be accepted as the true cause, we may hope by still further extending its use to obtain a still lower mortality.

The returns from England and Wales are in striking contrast to those from Australia. There is here, as might be expected from the much larger population, a smaller extent of variation in the pre-antitoxin period. The average for the eleven years is 31·5 deaths per 100,000, a mortality considerably less than that of Australia. In the antitoxin period I have returns only for the three years 1895 to 1897, and these show an average mortality almost exactly the same as in the pre-antitoxin period, and much greater than that in the three colonies for the same period. The position as compared to Australia has become conspicuously reversed since the introduction of the serum treatment.

This fact is emphasised in the following table, in which I have compared the mortality for the four antitoxin years with that of the previous four years, selecting these because in Australia they were years of unusually low mortality, and, therefore, compare more favourably with the antitoxin years than do the longer period given in the last table:—

TABLE SHOWING MEAN ANNUAL DEATH RATE PER 100,000 LIVING DURING THE FOUR YEARS PRECEDING AND FOLLOWING THE INTRODUCTION OF ANTITOXIN, TOGETHER WITH THE PROPORTIONATE REDUCTION IN MORTALITY.

—	Mean Annual Death Rate per 100,000 for four years, 1891 to 1894.	Mean Annual Death Rate per 100,000 for four years, 1895 to 1898.	Reduction in Death Rate per 100,000 living.	Proportional Reduction of Death Rate (Death Rate for 1891 to 1894 = 100).
Queensland	41·2	16·4	24·8	60·2 per cent.
New South Wales	45·1	18·4	26·7	59·2 ,,
Victoria	27·2	19·7	7·5	27·9 ,,
The three colonies taken together...	37·0	18·7	18·4	49·5 ,,
England and Wales	32·9	Three years only, 1895-1897. 31·3	1·6	4·9 ,,

We see that the mortality in Queensland and New South Wales has fallen 60 per cent., in Victoria not far from 30 per cent., in the three colonies together about 50 per cent., in England and Wales not quite 5 per cent.

It is possible that the true reduction in the English mortality may have been greater than that shown by the figures. The two years preceding 1895 were in this country years of unusually high mortality, that for 1893 being the highest on record during the period treated of. This rise may have had a tendency to continue, and so may have partly masked the decreased mortality caused by treatment. But, after making allowance for this possibility, the small extent of the fall in the mortality in the mother country, as compared with the greatly reduced mortality in the colonies, remains conspicuous, and demands some explanation.

This explanation seems to be offered by a paper by Dr. Goodall,* as far at least as regards the diphtheria mortality of London.

In the following table, based on a table given in this paper, we have the mortality in each year of cases notified as diphtheria. Being a case mortality, the disturbing factor of epidemic prevalence is eliminated; and as the returns are based on a population of four or five millions, they may be regarded as free from minor disturbing influences :—

CASE MORTALITY OF DIPHTHERIA IN LONDON.

Year.	Mortality Per Cent. of Notified Cases Admitted to Asylums Board Hospitals.	Mortality Per Cent. of Notified Cases Not Admitted	Mortality Per Cent. of all Notified Cases.
1892	24·8	21·5	23·8
1893	27·1	23·7	24·5
1894	25·0	24·5	24·7
Mean for three years ...	25·6	23·2	24·3
1895	18·3	23·3	21·2
1896	17·7	21·3	19·9
1897	14·9	20·1	17·4
Mean for three years ...	17·0	21·6	19·5

The proportion of notified cases treated in the hospitals of the Metropolitan Asylums Board in each year varies from 25 to 50 per cent. of the whole number.

It will be observed that a very considerable fall in the case mortality of diphtheria has occurred in the hospitals of the Metropolitan Asylums Board since 1895, but that among all cases not admitted into these hospitals the fall has been very small, hardly beyond the limits of accidental variation. This is so in spite of the fact that hospital cases always contain a large proportion admitted in an advanced stage of the disease. Dr. Goodall remarks :—"All the evidence I have brought forward has been designed to show, and I think does show, that outside the hospitals, general or special, the antitoxin treatment of diphtheria is little practised." On referring to the table showing the diphtheria mortality for the three Colonies, and for England and Wales for the last fifteen years, there will be found, I think, evidence that this statement does not hold good of the metropolis alone. When we consider that a fall of 10 per 100,000 in the annual mortality of diphtheria—a much smaller fall than might be expected from the experience of the Australian Colonies—would, in a population of 30,000,000, mean the annual saving of 3,000 children from a cruel and painful death, I think we are justified in pressing the results obtained here on the attention of our professional brethren in the mother country.

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* *British Medical Journal*, 1899, Vol. I., page 197.

Dr. Brockway (Brisbane) said that they were accustomed to hear Dr. Turner's able papers, and to leave them at that. They were, in fact, so exhaustive and complete as to leave little room for discussion. As far as he was concerned there was nothing to be said, and he considered the lack of argument following upon Dr. Turner's able paper to be a compliment to him. He hoped someone would suitably congratulate Dr. Turner.

Dr. Flashman (Sydney) said he must congratulate Dr. Turner. His paper had shown that there had been a marked decrease in mortality from diphtheria throughout Australia during the time within which antitoxin had been in general use. This was a most gratifying fact in connection with the colonies concerned in such decrease. The difference in the rate of mortality between England and Australia during the antitoxin period was a most interesting point in connection with Dr. Turner's paper. What was the cause of this difference? He was deeply interested in the answer to this question. As for the statistics of Dr. Turner, which were so ably elaborated, they did not lend themselves to criticism. They spoke for themselves. He begged to offer Dr. Turner his congratulations.

Dr. Richards (Mount Morgan) said he would like to ask Dr. Turner whether the difference referred to might not be due to a more complete knowledge of diphtheria by the medical men who passed the cases on to the various hospitals. It frequently happened that when the patient belonged to a lodge the lodge doctor, not having the requisite antitoxin, immediately handed the cases on to the hospital; and he believed that many other medical men, who, before the discovery of antitoxin, would have treated the cases in an ordinary manner, now did the same thing. Diphtheria, too. was far better known than was once the case, and would thus be treated as such at an earlier stage than in former years, and this perhaps would account for some portion of the decrease in mortality from diphtheria in Australia. England showed no such falling off, and, in fact, only dropped two decimal points in that period of time. He found in his experience that the dried serum, which he had used eight or nine times was generally very difficult to dissolve; formed a very sticky solution; and frothed considerably on attempting to dissolve it. Further, it had proved very disappointing in its effects, as compared with the liquid form. This was most regrettable, especially in the case of Central and Northern Queensland.

Dr. Carruthers (Balmain) said that the introduction of antitoxin had caused greater care in the treatment of diphtheria, and owing to a quicker diagnosis the cases would now receive an immediate treatment. He considered that the more accurate diagnosis of diphtheria which obtained at the present day would give an apparent increase to the mortality due to that disease. He congratulated Dr. Turner on his lucid exposition.

Dr. Howard (Melbourne) said that Dr. Turner's paper was a most elaborate one, and was one extremely difficult to criticise. He must congratulate Brisbane on the possession of Dr. Turner. He had a considerable experience of diphtheria in Melbourne, and to his mind the effect of the antitoxin treatment was unmistakable. In 1889 and 1890 there was an appalling mortality in Melbourne from that disease, as there was also from typhoid. In 1893 he met a gravedigger who, on being questioned on the subject, said that business with him then was very bad, but in the boom they couldn't plant them fast enough, whilst now dismissals were taking place. Since antitoxin had been systematically used, the mortality from diphtheria had considerably decreased, and he considered this improved result to be a direct consequence of the antitoxin treatment. It frequently happened that medical men at a distance could not always obtain the antitoxin, and he considered "the dry form" of serum for what they might call "bush work" should be used.

Dr. Richie (Victoria) said that he had treated a large number of cases of diphtheria with antitoxin. He had frequently used the dried serum, and had come to the conclusion that it was just as good as liquid serum, and, whilst it cost 1s. for each dose, 7s. 6d. was charged for a dose of liquid serum. The fact was worth bearing in mind. On behalf of Victoria he congratulated Dr. Turner.

Dr. Eames (Newcastle) said that, looking at the tables which Dr. Turner had placed before them, he could not help thinking that climatic conditions had no little to do with the mortality from diphtheria, and he considered the warmer the climate the lower the mortality from that disease.

Dr. Tilley (Warwick) said he thought that the decreased mortality found in Australia during the antitoxin period was perhaps due to the promptness in treating the cases in their earlier stages. He had, however, formerly treated a large number of bad cases without antitoxin, and in those cases where tracheotomy had been performed only two recoveries had been made, whilst since the introduction of antitoxin he had treated three tracheotomy cases with the serum, and all these patients had recovered. In one case he had used dry serum, but could not dissolve it properly, and he considered that in this case the patient had only received one-third of the dose.

Dr. Turner said that antitoxin was as easy to use in private practice as in hospitals, and should be used in private practice in all cases. If it were generally so used, there would be no necessity to send mild cases to the hospital, and he considered that many cases which were sent to the hospitals in a bad condition would have recovered in two or three days at home under the antitoxin treatment. He held that his proposition was proved, and that the efficacy of antitoxin was no longer a matter of doubt, and that the serum should now be in universal use. The object of his paper was to point out that, owing to the conservatism of the medical profession in Great Britain in matters of treatment, lives were being sacrificed by the thousand which otherwise might have been saved.

THE USE AND ABUSE OF HOSPITALS IN QUEENSLAND.

By A. C. F. Halford, M.D.,
Clermont.

In bringing the subject of my paper before you I make no apology. I do, however, crave your indulgence for the manner in which it is treated.

It is to-day an important subject with us all, and from whatever aspect we approach it we are impressed with much that is inefficient and a great deal that is unsatisfactory.

My first task is to prove to you that the present position of the public charities is one which, for the sake of the community at large, requires re-organisation and reform.

Secondly, I will undertake to say in what way this much-needed reform may be accomplished.

In support of my main contention I have been at great pains to collect evidence from all parts of the colony in which hospitals have been established. In the answers to the circulars addressed to medical officers in charge of general hospitals I have received many valuable communications. To those gentlemen who took so much trouble in preparing their answers I acknowledge a deep sense of gratitude.

I also addressed a letter to the Home Secretary, asking for particulars with regard to the endowment of charitable institutions. I received a courteous reply, the substance of which was that "the provisions of the Hospitals Act have to be extended to the institution, and a memorial of the name of the treasurer thereof must be forwarded to the Registrar-General." These are the only conditions precedent to granting subsidy. The letter also stated that it is the intention of the Government to review at an early date the entire question of the relations between the public hospitals, the medical profession, and the State.

The general opinion vouchsafed to me by my colleagues is that our hospitals are mostly identical with institutions of a mutual benefit character. With that opinion I cordially agree.

Each one is run on lines dictated by its little governing body, quite irrespective of rights and privileges. The pay-patient is the one individual catered for. On him the destitute patient must wait; for him the hard-driven medico must work, and by him lose a fair prospect of a decent livelihood in private practice.

In the face of this the Government distributes large sums annually in endowments amongst these institutions. It is time for us to ask, "Why should these hospitals, which are run on mutual benefit lines, receive help from the Treasury? Would not the ordinary medical lodges have as good grounds to demand a right to participate in this distribution of public money?"

The most unjust feature in the present endowment system is that, in quite a large number of hospitals, endowment is claimed on a subscription (usually 10s. or 20s.) which entitles the donor to all the benefits of the institution for the subsequent twelve months. In many cases no further payment is demanded for treatment received. The hospital authorities explain that no contributions will be forthcoming unless this *quid pro quô* is granted to the subscriber. They do not forget to claim endowment on such "charitable" donations.

I have only to quote from the rules of almost any of the hospitals from whose surgeons I have received answers to my circular, to clearly show that my contention is not in the least overdrawn.

I have selected a few cases sufficient to justify my accusations. For instance, a hospital provides in Rule 7 that—

> No ticket or order shall be necessary for entrance to the institution, but applicants for treatment shall be admitted by the surgeon at any hour. . . . All paying patients shall be charged the sum of 4s. per day . . . payable in advance.
>
> Any subscriber of £1 per annum shall be entitled to be treated free of charge in the institution during that year. . . . Such subscribers shall not be required to sign the declaration as to their inability to pay the hospital charges referred to in this rule. [The reference is to the "Patients' Admission Book."]

The resident medical officer's comments on this rule are worth reproducing: "Any patient, no matter what his circumstances financially, can receive medical attention twice daily—oftener, if necessary—medicine, nursing, board, and lodging for the sum of 28s. per week. For the sum of £1 yearly, if paid in advance, a person, no matter what the circumstances financially, can regard himself the member of a 'benefit society,' to give him the above attentions during that year up to 31st December, for any and every disease which is serious enough to cause detention in hospital. With regard to the out-patient department, any person, whoever he or she may be, is permitted to seek out-door medical attention, and is supplied with medicine, both free of charge. In this department there are many who seek the benefits, though they can well afford to pay an ordinary medical practitioner. As there is neither 'ticket nor order required' (*vide* Rule 7), any person can casually drop in and request treatment." A truly dreadful state of affairs.

From Dr. ———, of ——— : "I hear that the only difficulty is found in the out-patient department. There seems to be a belief widely prevalent amongst contributors of the labouring classes (shearers, rouseabouts, &c.) that, if they happen to make a donation to a hospital, they have a perfect right to receive treatment when they require it, apparently as if they had paid into a medical club or benefit society. Consequently, if any one of these men is known to be earning fair money and is refused out-patient treatment, he promptly starts an outcry all over the district with the intent to interfere with the financial support to the hospital, and not always without success." This, gentlemen, is a serious charge.

Dr. ——— has grasped the situation at ——— in a comprehensive manner, having introduced many improvements.

When he first took charge, the lodges of the district had bought the right to use the out-patient department as a consulting institute, in cases where they were not satisfied with their own surgeon, for the sum of £6. The committee stopped this. The usual complaint, that the contributors to the hospital require a *quid pro quô*, is made. Stress is also laid on the fact that many well-to-do people seek admittance to the hospital and are only required to pay a weekly fee of 35s.

In other respects the hospital is not abused. Destitute patients get the preference when the hospital is crowded. No family connected with a friendly society is treated unless by the expressed wish of the lodge surgeon. Other undeserving out-patients are marked by the visiting committee.

The main grievance is with regard to pay-patients. Dr. ——— sums up the matter in these terms :—

"It is a real grievance that an individual in good circumstances should consider that he or she has discharged all their liabilities by the payment of the sum of 35s. per week to the funds of the hospital—few pay so much. There is a disposition to run into hospital, even with the better classes, as pay-patients. They submit that they have not homes of their own (if single), or (if married) that they cannot get proper nursing outside hospital. Granting the truth of this, I do not admit that it is a sufficient reason that they should pauperise themselves by obtaining free medical attendance and nursing at a charitable institution. (The full amount charged scarcely covers the cost of drugs and rations.) Since patients of this class cannot be well excluded from hospitals, I would suggest the following remedy."

The remedy suggested is to increase the fees payable to the hospital, or to pay the surgeon himself ; or, as an alternative, to remove the pay system altogether, and make hospitals purely charitable institutions.

Reports from two other hospitals may be alluded to.

In the one the only complaint is the too free distribution of out-door tickets. There is a rule, however, in which a subscriber who donates not less than £1 is allowed free admission to the hospital during the ensuing year. This may be only for destitute persons, but the surgeon does not say so.

In the other institution the surgeon reports the following state of affairs :—

"All annual subscribers among stations and station employees are admitted free, otherwise they would refuse to contribute."

No additional charges are made, any further payment being voluntary.

The remedy suggested by my correspondent is that the State should take entire control ; that admission be made on the recommendation of a State censor ; that the system of honorary consultants be abolished.

"By this means," writes my informant, "the subscribing public would not be defrauded; the State would not be robbed of money improperly expended; but far above all others comes the benefit to the rank and file of our long-suffering profession who are neither senior consultants nor on the paid honorary staff. Their practices must increase at least 10 per cent."

Does it not strike one that, in the face of these facts our so-called public hospitals deserve to be regarded as commercial institutions, offering "insurance against disablement from sickness or accident"? It is a fraudulent misappropriation of the Government endowment to maintain such institutions in our midst, posing as they do as objects of charity.

THE REMEDY.

We cannot hope for any good results until we have clearly demonstrated to the Government the illegality of the claim (in part) of many hospitals to the endowment. In the first place we can show that hospitals claim endowment on the amount paid by a subscriber for a ticket which will entitle him to all the benefits of the institution issuing it. Surely a ticket issued under such circumstances (*i.e.*, carrying endowment) should be available for charitable purposes alone. Surely, also, the State should not be asked to subsidise a subscription the giver of which is innocent of any charitable motive.

If a certain section of the public wish to have a hospital imbued with a mutual benefit spirit, let it erect and maintain such at its own expense.

At any rate, the Government should be approached with a view of obtaining a due recognition in the appropriation fund of all hospitals run on a purely charitable basis, and an assurance that institutions of mutual benefit character will receive no such recognition.

The hospitals at present are dependent on the Government subsidy.

Surely the circumstances of our case are sufficient to enlist the sympathies of the State.

Let us therefore urge the State to dictate reforms in the required direction.

SUGGESTED REFORMS.

The first step should be to place all hospitals on a uniform footing and to adopt for them a common code of rules. This is essential in order that the organisation may be more perfect. It will ensure also that any patient going from one hospital to another will invariably be subject to the same conditions. At present patients are allowed certain privileges at some hospitals which are denied them at others; they obtain certain benefits as subscribers at one place that they do not enjoy elsewhere.

This condition of things makes it all the more difficult to deal with applicants for relief. In Queensland there is a large moving population of shearers, miners, and ordinary labouring men who patronise the hospitals largely in their wanderings. Hence the necessity for the common code of rules.

Secondly, as a result of the adoption of the common code of rules, let the aim and object of the institutions be to succour those who are sick and unable to bear the cost of treatment elsewhere. Urgent cases should be eligible under the usual conditions. Thus, abolish all payments except those of a purely voluntary nature, and maintain the institutions out of funds provided by some system of direct or indirect local taxation; or, rather, let us leave the financial question in the hands of students and masters in political economy.

Thirdly, it is desirable to appoint an inspector-general of public charities.

Fourthly, to establish a public charities medical service, from which the various institutions of the colony could be officered.

These reforms can only be accomplished by bringing the public charities directly under State control, which is desideratum No. 5.

Should these reforms find favour in Queensland, there is no reason why they should not be extended to all the Australian Colonies, thereby federating the colonial charitable institutions.

In conclusion, gentlemen, let us take active measures to supply a remedy; the expectant plan has had its day. Then, perhaps, reforms in some shape or other will follow. I trust that my small effort will set wiser heads thinking, so that ultimately the sympathies of the State will be enlisted for charity's true cause alone, and that a very substantial improvement may be in store for the medical profession in its relations to modern charitable institutions. Above all, let us unanimously plead for the cause of the destitute sick.

DR. RICHARDS (Mount Morgan) said that he wished to thank Dr. Halford for his paper, and for having brought forward the subject of hospital abuse. He lived in Central Queensland, where the practice obtained to a greater degree than in any other portion of the colony, and where, for a small fee, medical attendance was supplied. He was once under the impression that a subscriber helped the hospital as in the South, but he found that in Mount Morgan this was far from being the case. There were 2,000 men employed at Mount Morgan. A portion of their wages was retained by their employers, and was handed to the hospital. These men were then entered in the books of the institution as subscribers, with the right of becoming either out- or in- patients. These subscribers not only included the miners but also officers of the company with incomes of from perhaps £500 to £700 yearly. It was scandalous for men in receipt of such incomes to receive treatment on these terms. It must, however, be said that these officers did not usually avail themselves of this privilege, though the ordinary business people of the town frequently went to the hospital for treatment. When the Government was paying £2 for each £1 subscribed by the public, the Government should regulate the hospital by nominating the committee in whole or in part. The committee in these hospitals were not game to effect the radical alterations which were desirable. If, however, the Government nominated a committee including the local P.M., this difficulty could be obviated. It was a shame that the public should be called upon to provide free board, lodging, and medical treatment for people who could easily afford to pay for them for themselves.

DR. SAMUELSON (Armidale) said that he had been for ten years in New South Wales, and had experienced a similar difficulty to that which was being experienced in Queensland. It was true the appointment of Government representatives on the committee might have some effect, but he was afraid the effect would be merely one of disunion. In his own personal experience he had found the committee were often too ready to spend their money on "frills," whilst many comforts were denied to patients, owing to the money going in this manner. On one occasion the committee of a hospital had been asked to furnish a proper water supply for the use of the doctors, but the request had not been complied with, though it was highly necessary. In New South Wales the treasurer of the hospitals there was a nominee of the Government, but he was only too frequently in entire sympathy with the committee. He considered that there was an unevenness in the distribution of the hospital subsidies, and that the subsidies should be distributed to the hospitals in strict accordance with their requirements. He would like to see a thorough inspection of hospital accounts inaugurated, and an inquiry should be held as to the management of the country hospitals with a view to controlling their internal expenses. There should also be inquiries made into the subject of paying-patients. It seemed to him that there was a great deal of neglect in many of the hospitals of the indigent sick in favour of those who were regarded as paying-patients. A man who paid the sum of £1 a week for maintenance was looked upon as a paying-patient, although it cost more than £1 per week to maintain him and to supply him with the necessary medical attendance. In these hospitals the paying-patient was looked upon as a *persona grata.* He always referred to these patients, much to the annoyance of the committee, as partially paying patients. A distinct fund and a service of inspection should be established for the benefit of the indigent sick who became the children of the State.

Dr. Carruthers (Balmain) said that at the present time a Royal Commission was inquiring into the condition of the hospitals of New South Wales, and he thought some abuse of the hospitals might be removed by making the patients who sought hospital treatment subscribe to a declaration that they were unable to pay for the same, and he thought a declaration should also be signed by the person giving the ticket of admission to the hospital to the intending patient.

Dr. Comyn (Brisbane) said that he was a doctor in the suburbs of Brisbane, and he considered that the ambulance brigade were only too ready to bring people to the hospital. Small operations in the suburbs had almost entirely disappeared. Although there were several private hospitals in Brisbane, the senior member for Brisbane North, who had met with an accident on the railway, had been taken to the General Hospital, He believed that the member for North Brisbane did not for one moment know that he was enjoying the charity of the medical men or the charity of the people of Queensland.

Dr. Thomson (the President of the Congress) warmly deprecated the introduction of the personal element into the discussion. He knew of this instance at the time of giving his inaugural address, but if all the circumstances were taken into consideration it would be seen there was great excuse for the taking of Mr. Forrest to the General Hospital. The accident happened late at night, and at a considerable distance from town. Mr. Forrest was lying in great agony under a bridge. A train was stopped, and he was taken on board. He was in such a condition that he did not know where he was going. The ambulance brigade was summoned, and he was taken to the hospital. He did not believe that Mr. Forrest was ever consulted on the matter. He considered that although that case appeared to be a glaring one, and one easily referred to, that there was a great deal to be said in favour of Mr. Forrest being removed to the hospital. He thought that anything like a suggestion that he was making use of public charity should be heartily condemned. His gratitude and his munificence—and he said it advisedly—removed anything like that stigma from him.

Dr. Pinnock (Ballarat) said that he considered the paying-patient evil might be modified by the use of a declaration so framed that if it were falsely made the patient could be prosecuted for perjury.

Dr. Pain (Sydney) said that in his own experience he might sum up the whole question by saying that the paying-patient evil depended upon the medical men, the governing body of the hospitals, and the medical superintendent. In this connection he, as medical superintendent of a hospital, had had considerable trouble, chiefly through general practitioners. He frequently received letters from doctors recommending patients for admission to the hospital. He did not admit a patient without a note from the doctor who last attended such patient. One doctor had sent a patient to his hospital, which patient he felt assured was well able to pay for medical treatment. He had rung up the doctor upon the telephone to tell him so, but was met with a strong assurance from that doctor to the contrary. He admitted the patient, but afterwards had the satisfaction of being able to prove to the doctor who had sent the patient that the patient at the time of admittance was worth thousands of pounds. Many general practitioners would not send a patient to a specialist. There was a certain amount of jealousy in the matter, and they sent the patients to hospitals instead. The percentage of cases admitted by him that were not "doctors' cases" was small. In fact, he might safely say that fully 80 per cent. of the patients were admitted on the recommendation of medical men. If there existed abuses in hospitals, it was largely on the part of medical men. He found himself in this connection encumbered with great difficulties, and he suggested that suitable people should become members of medical clubs.

Dr. Halford said that he considered that the Government should cause an inquiry to be held into the whole hospital system, and moved—

> "That this Section recommends the Congress to forward memorials to the Governments of the respective colonies requesting them to undertake actively the reorganisation and reform of public charities."

Dr. Thompson (the President of the Section) said that the Government intended to do something in the matter, and a change in the hospital systems might shortly be expected, and he thought that if that motion were carried it would strengthen their hands.

ARMY MEDICAL SERVICES.

By W. L'Estrange Eames, M.B. Dub.,
Newcastle.

The importance of this subject is my only apology for bringing it before this Congress, for whom, at first sight it apparently has no concern; but when one considers for a moment the great disabilities which the profession in the service at home suffered for so many years, and when the inefficiency of the service in a certain degree reflects credit or otherwise on all medical men, whether in the service or not, I think it is a subject which we at this Congress are not only justified in considering, but I am certain that that consideration will have an effect for good not only for the profession in all the colonies, but that it will also be in the public interest.

Wherever an army medical service has been built up in any Colony, it has been by the efforts of individual men who may have been appointed to the medical services of the local forces. Now, history teaches us that the last thing to be considered in any service in times of peace is the efficiency of the medical department as may be required for a war footing, and, so long as the medical arrangements will just meet peace requirements, so much money and no more can be dragged out of those who have charge of the purse, and then when war suddenly arises it is endeavoured to hastily supply the needs of the department, both as regards organisation, personnel, equipment, and transport, and the inevitable and invariable result is a miserable breakdown, which reflects discredit on the department and all concerned. We have the latest example—viz., the Americo-Spanish war; and I quote the official report of the Special Commission appointed by the United States Government to investigate the conduct of the War Department, especially as applied to the medical services. I also quote the extract from "Sanitary Lessons of the Americo-Spanish War" in the *Sydney Morning Herald* of 5th August, 1899:—

THE MEDICAL DEPARTMENT OF THE UNITED STATES IN THE LATE WAR.

The official report of the Special Commission appointed by the United States Government to investigate the conduct of the War Department in the recent war with Spain has now been issued. In regard to the general question, the commission has come to the conclusion that "there was lacking in the general administration of the War Department, during the continuance of the war with Spain, that complete grasp of the situation which was essential to the highest efficiency and discipline of the army." With regard to the medical department in particular the findings were as follow:—

1. That at the outbreak of the war the medical department was, in men and materials, altogether unprepared to meet the necessities of the army called out.

2. That, as a result of the action through a generation of contracted and contracting methods of administration, it was impossible for the department to operate largely, freely, and without undue regard to cost.

3. That in the absence of a special corps of instructors, and the apparent infrequency of the inspections by chief surgeons, and of official reports of the state of things in camps and hospitals, there was not such investigations of sanitary conditions of the army as is the first duty imposed upon the department by the regulations.

4. That the nursing force during the months of May, June, and July was neither ample nor efficient, reasons for which may be found in the lack of proper volunteer hospital corps, due to the failure of Congress to authorise its establishment, and to the non-recognition in the beginning of the value of women nurses, and the extent to which their services could be secured.

5. That the demand upon the resources of the department in the care of the sick and wounded was very much greater than had been anticipated, and consequently, in like proportion, these demands were imperfectly met.

6. That powerless as the department was to have supplies transferred from point to point, except through the intermediation of the quartermasters' department, it was seriously crippled in its efforts to fulfil the regulation duty of "furnishing all hospital and medical supplies."

7. That the shortcomings in administration and operation may justly be attributed in large measure to the hurry and confusion incident to the assembling of untrained officers and men ten times larger than before, for which no preparation in advance had been or could be made because of the existing rules and regulations.

8. That notwithstanding all the manifest errors—of omission rather than commission—a vast deal of good work was done by medical officers, high and low, regular and volunteer, and there were unusually few deaths among the sick and wounded.

The Commission makes definite recommendations for the future conduct of the medical department, urging the necessity of a larger force of commissioned medical officers; authority to establish in time of war a proper volunteer hospital corps; the establishment of a reserve corps of selected trained women nurses; extra supplies of all sorts to be held constantly on hand; improvement in transportation, less red tape, and the authorisation of surgeons to purchase such articles of diet as may be necessary for the proper treatment of soldiers too sick to use the army ration.

SANITARY LESSONS OF THE AMERICAN-SPANISH WAR.

At one of the sessions of the American Medical Association, Surgeon-General Sternberg, of the United States Army, delivered an address on "Sanitary Lessons of the War." He said (reports the *New York Tribune*), in part, that, compared with the Civil War and other great wars in the present century, the mortality from wounds and disease among their troops in the war with Spain had been low. Their wounded had, to a large extent, the advantage of prompt treatment with antiseptic dressings, and a considerable proportion of those who were not killed outright had recovered without any mutilating operation or septic complication.

The total number of deaths reported, including regulars and volunteers, from 1st May, 1898, to 30th April, 1899, was 6,406. Of these 5,438 died of disease, and 968 were killed in battle or died of wounds, injuries, or accident. In the Civil War the number of deaths from disease was 186,216. The number who were killed in battle or died of wounds was 93,969, or about one-half of the deaths from disease. The total deaths from disease in the Union armies from the beginning of the war to 31st December, 1862, was 34,326; and in the Confederate armies in the same period, 31,238.

General Sternberg presented a table giving the monthly death rates from disease in the American armies from 1st May, 1898, to 30th April, 1899, and, for comparison, the rates for the same period during the first twelve months of the Civil War, showing that the percentage to 1,000 for the last year was 25, and in the first year of the Civil War it was 45. After comments upon these figures he continued :—

"But the comparatively small number of medical officers of the regular army available for duty in the large camps occupied by our volunteer troops at the outset of the war proved to be entirely inadequate to control the sanitary situation in these camps, and as a result of the conditions existing the mortality from typhoid fever in our armies during the year ending 30th April, 1899, has been more than twenty-two times as great as the annual mortality in our regular army during the decade immediately preceding the war period. As compared with the first year of the Civil War, however, there is a decided improvement, the typhoid mortality for the first year of the Civil War having been 1·971 per 100,000 of mean strength, and for the Spanish-American war 1·237 per 100,000. Moreover, the vigorous sanitary measures enforced enabled our troops to quickly free themselves from the ravages of this infectious disease; and while the line of typhoid mortality continued to ascend during the first year of the Civil War and subsequently, it rapidly fell after the middle of September last, and for the last six months of the period under consideration has been remarkably low. Indeed, in the history of large armies the record has never heretofore been equalled.

"The principal lessons to be derived from our recent experience may be stated as follows :—

"A trained medical corps hardly adequate for an army of 25,000 men cannot control the sanitary situation when this army is quickly expanded to 250,000. Physicians and surgeons from civil life, however well qualified professionally, as a rule are not prepared to assume the responsibilities of medical officers charged with administrative duties and the sanitary supervision of camps. The proper performance of such duties cannot be expected from a physician without military training or experience, no matter how distinguished a position he may have held in civil life.

"Courage and patriotism on the part of line officers and enlisted men cannot take the place of knowledge and experience. New levies of troops are, as a rule, ignorant of the first principles of camp sanitation, and reckless as to the consequences of their neglect of prescribed sanitary regulations. Therefore training and discipline are essential factors in the preservation of the health of soldiers in garrison or in the field.

"The value of the aphorism 'in time of peace prepare for war' has received additional support. This preparation should include a corps of trained medical officers larger than is absolutely necessary for the army on a peace basis, and systematic instruction in military medicine and hygiene for the medical officers of the National Guard, as well as for those of the regular army; also, instruction of line officers in the elements of hygiene, and especially in camp sanitation. It should also include the establishment of camping grounds in various parts of the country, having an ample supply of pure water, a proper system of sewers, &c. If our volunteers could have been assembled in such camps in the late war a saving in lives and money would have resulted which would, without doubt, have demonstrated the economy of such preparation for war in time of peace."

I would suggest that in consideration of these facts, and in order to try and avoid falling into the same mistake in these Colonies, that it is advisable for this Congress to appoint a committee to bring under the notice of the Governments of the various Australian Colonies the urgent necessity for some combined line of action requisite to be taken to meet the requirements in question, and in view of federation in the near future it is impossible to attach too much importance to a matter which is purely national.

This, coming from Congress, will carry great weight with those in power, and will materially assist the various principal medical officers to place their respective services on a footing equal to all requirements in peace, and capable of an efficient and rapid extension to meet the emergencies of war.

THE FERTILITY OF THE UNFIT.

By W. A. Chapple, M.D. (N.Z.), D.P.H., M.R.C.S. Eng.,
Wellington.

Births, deaths, and migration are the factors which make up the population question.

The problem has burned in the minds of all great students of human life and its conditions.

Aristotle says (Politics II., 7-5): "The legislator who fixes the amount of property should also fix the number of children, for if the children are too many for the property the law must be broken." And he proceeds to advise (*ib.* VII., 16-15): "As to the exposure and rearing of children, let there be a law that no deformed child shall live, but where there are too many (for in our State population has a limit) when couples have children in excess, and the state of feeling is adverse to the exposure of offspring, let abortion be procured."

The difficulty of over-population was conspicuous in the minds of Aristotle and Plato, and these philosophers both held that the State had a right and a duty to control it.

The question in the mind of the Greek philosophers was this: Over-population is a cause of poverty; under-population is a cause of weakness; defectives are an additional burden to the State: how shall population be so regulated as to establish an equilibrium between the stability of the State and the highest well-being of the citizens?

The combined philosophy of the Greeks taught: Delay marriage, encourage the best citizens to procreate their kind, and practise exposure of infants, and abortion.

A century of debate has raged around the name of Malthus, the great modern analyst of the population problem. He published his first essay on population in 1798—a modest pamphlet, which fed so voraciously upon the criticism supplied to it that it developed into a mighty contribution to a great social problem, second only in time and in honour to the work of his great predecessor in economic studies, Adam Smith.

Malthus' first essay defined and described the laws of multiplication as they apply only to the lower animals and savage man. It was only in his revised work, published five years later, that he described moral restraint as a third check to population.

No one who has studied the economic and vital statistics of the last half-century can fail to be impressed with the change that has come over the relative ratios of increase in population and food.

Bonar says ("Malthus and his Work, p. 165)": "The industrial progress of the country (France) has been very great. Fifty years ago the production of wheat was only the half of what it is to-day; of meat, less than the half. In almost every crop and every kind of food France is richer now than then, in the proportion of two to one. In all the conveniences of life (if food be the necessaries) the increased supply is as four to one; while foreign trade has become as six to one."

In a remarkable table prepared by Mr. F. W. Galton, and quoted by Mr. Sydney Webb in "Industrial Democracy," it is clearly shown that while the birth rate and food rate (defined as the amount of wheat in Imperial quarters, purchasable with a full week's wages) gradually increased along parallel lines between 1846 and 1877, the former suddenly decreased from 36·5 per 1,000 in 1887 to 30 per 1,000 in 1895, the latter increasing from 0·6 to 1·7 for the same period.

The remarkable thing about the facts that this table so clearly discloses is that with a gradual increase of the means of subsistence from 1846 to 1877 there is also a gradual increase in the proportion of births to population. But at the year 1877 there is a very sudden and striking increase in food products, and the purchasing power of the people coincides exactly with a very sudden and striking decrease in the birth rate of the people. The greater the decrease in the birth rate, the greater the increase in the people's purchasing power. Now, what has brought about this change in the ratios of increase in population and in food respectively.

Some serious factor, inoperative during the thirty years prior to 1877, must have been introduced into the social system to work such a marvellous revolution during the last twenty years.

Some economic writers find it easy here to discover a law, and declare that the birth rate is in inverse ratio to the abundance of food. (Doubleday quoted by Nitti, "Population and the Social System," p. 55.)

Other economic writers of recent date attribute this great change in ratio of increase to economic causes. Only a few find the explanation in biological laws.

Herbert Spencer is the champion of the biological explanation of a decreasing birth rate.

With the intellectual progress of the race there is a decadence of sexual instinct.

But to what extent does this affect fertility? Sexual power and fertility are not synonymous terms. The vast profusion of seed in plant and animal life would allow of an enormous reduction in the amount produced without in the least affecting fertility. Even admitting the application of Spencer's law to sexual vitality, and allowing him to claim that, with the progress of "individuation" there is a decline in sexual instinct, would the fertility of the race be affected thereby?

To have any effect at all on the birth rate, the instinct would have either to be killed or to be so reduced in intensity as to stop marriage or to delay it till late in life. When once marriage was contracted, sexual union at an appropriate time—once every two years—would, under strictly normal conditions, result in a very large family.

For, according to Mr. Spencer's theory, it is the instinct that is weakened, not the power of the spermatozoa to fertilise. Evidence is wanting, however, to show that there is a decrease in the sexual power of any nation.

The fact is that the sexual instinct is so immeasurably in excess of the maximum power of procreation in the female, that an enormous reduction in sexual power would require to take place before it would have any effect on the number of children born.

The number of children born is controlled by the capacity of the human female to bear children, and about one birth in every two years during the child-bearing period is about the maximum capacity.

A moderate diminution in the force of the sexual instinct might lead to a decrease in the marriage rate, but it would require a very serious diminution, bordering on total extinction of the instinct altogether, to exert any serious effect on the fecundity of marriage.

All that can be claimed for this theory of population is that, reasoning from known physiological analogies, we might expect a weakening of the desire for marriage coincident with the general development of intellect in the race.

There are as yet no facts to prove that such weakening has taken or is taking place, nor are there facts to prove that population has in any way suffered from this cause.

If such a law obtained, and resulted in a diminished birth rate, the future of the race would be the gloomiest possible. An inexorable law would determine that there could be no mental evolution, for the best of the race would cease to propagate their kind. All who would arrive at this standard of mental growth would become barren. And against this there could be no remedy.

One of the main contentions of this paper is that the best have to some extent ceased to propagate their kind, but it is not maintained that this is the result of a biological law over which there is no control. It can be safely claimed that to Malthus' three checks to population—vice, misery, and moral restraint—the demographic phenomena of a century have added no other.

The third check, however—moral restraint—must be held to include all restraint voluntarily placed by men and women on the free and natural exercise of their powers of procreation.

Malthus never anticipated the changes and developments of recent years. He advised moral restraint as a preventive measure in the hope that vice and misery, as checks, would be superseded, and no more would be born into the world than there was ample food to supply. He believed that moral restraint was the check of civilised man, and as civilisation proceeded this check would replace the others, and prevent absolutely the population pressing upon the limits of subsistence.

But he did not know, or at least there is no evidence that he knew, that an intermittent restraint, whether without or within the marriage bond, would fulfil the same purpose.

He saw in moral restraint only self-denial, constant continence, and entertained no suspicion that the generative instinct could be cheated of its natural fruit. The passion for marriage is so strong (thought Malthus) that there is no fear for the race; it cannot be over-controlled. The gratification of the sexual instinct and procreation were the same thing in the mind of Malthus.

The statistics of all civilised nations show a gradual and progressive decline in the birth rate, much more marked of recent years. In Germany, between the years 1875 and 1890, it has diminished from 40 to 35 per 1,000 of the population. In England and Wales it has dropped from 35 to 30 during the same time; in Ireland from 26 to 22; in France from 26 to 21. In the United States of America, between the years 1880 and 1890, the decline has been from 36 to 30; while in New Zealand it has declined from 40·8 in 1880 to 25·7 in 1898.*

During this period, 1875 to 1890, the rapid strides made in industry and in production have been unparalleled in the history of the world. Wealth has accumulated on all sides, and production and distribution have far outrun the needs and demands of population. To-day food is more abundant, cheaper, and therefore more accessible to all classes of the people than it was fifty years ago; and coincident with this rapid and abundant increase in those things which go to supply the necessities, the comforts, and even the luxuries of life, there has been a constant and uniform decline in the birth rate, and this decrease is even more conspicuous in those nations in which the rate of production has been most pronounced. It would even be true to say that the birth rate during recent years is in inverse proportion to the rate of production.

An increasingly large number of people have come to the conclusion that the burden and responsibility of family obligations limit their enjoyments in life, their ambition, and even their scope for usefulness, and have discovered, through the spread of physiological information, means by which marriage may be entered upon without necessarily incurring these responsibilities and limitations.

It is the knowledge of these physiological laws, and the practice of rules arising out of that knowledge, that account for the declining birth rate of civilised nations.

No branch of physiology is more interesting and attractive than that relating to sex; and the discovery amongst women that there is a period of susceptibility and a period of insusceptibility to conception in each monthly cycle has proved more contagious than physiological truths usually do. Continence during this susceptible period exerts a powerful influence on the birth rate of those who practise it. But such practice involves a self-control and self-denial not uniformly possessed by all. Many artificial checks are in common use, but all necessitate a certain amount of restraint, and to some extent arrest the sexual impulse.

If it be true that the birth rate is controlled by a voluntary effort on the part of married people to limit their families, and that that effort implies self-restraint and self-denial, it would not be too much to claim that those best capable of exercising self-control, and with the strongest motives for such exercise, are those most responsible for the declining birth rate, and that those with least self-control and the fewest motives for exercising the control they have, are most likely to have the normal number of children.

It has already been suggested that the desire to limit families is due to a consciousness of responsibility on the part of prospective parents. They realise the stress of competition in the struggle for existence: they are anxious for their own pecuniary and social stability, and even more anxious that the children for whose birth they are responsible should be provided with the necessities and comforts which health and development require. They are eager, too, that their children should be equipped with a good education, and thus be given a fair advantage in the race of life.

To the great mass of people this is only possible when the numbers of the family are limited. As the numbers of the family increase, the difficulties of clothing, feeding, and educating them increase; and each member is the poorer for every birth, and in this sense an increasing birth rate is a cause of poverty.

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*Taking Australia as a whole, Coghlan says there are 47,000 births less than would have occurred under the rates prevailing ten years ago.

It will be readily conceded that those actuated by the motives just considered—those with the keenest sense of responsibility in life, those capable of exercising the self-restraint which family limitation requires—constitute the best types of citizens in any community. From such the State has good reason to expect its best stock.

It is one purpose of this paper to show that this class, which can and should produce the best in the largest numbers, are being overwhelmed with the burden of supporting an ever-increasing number of incapables, and, largely in consequence of this increasing burden and responsibility, are unwilling to produce because they are unable adequately to support their own kind.

There is a class in every large community whose sense of responsibility in life is at zero, whose self-control is substituted by the law and its sanctions, and whose modes and habits of life are little better than those of the lower animals. Their appetites are stronger; their desires, though fewer, are more intense; and their self-control less easily and less frequently exerted than those in the highest planes of life.

In the first place, then, they have less desire to limit their families, and less power to exercise the self-restraint that is necessary to do so. Less sense of responsibility is attached to the rearing of a family, whilst the education of their children gives them little or no concern, and they entertain no ambition that members of their family should compete in the struggle for social status. Their instincts and impulses are their guide in all things. They marry early, and procreation is unrestrained except by the hardships of life.

This constitutes a numerous class in every large community, and includes the criminal, the drunkard, and the pauper, and many defectives such as epileptics and imbeciles. Now all these propagate their kind. The checks to the increase of this class are the checks which are common to savage man, and are elaborated in his first essay by Malthus. They are vice and misery, but the State in its humanitarian zeal is controlling these checks.

If it were not for moral restraint, these two checks—vice and misery—would absolutely control the population of the world.

The mind of man has added to these a new check which applies to, and can only be exercised by, himself. And the problem is, How far will misery and vice as checks to the population be eliminated, and moral restraint take their place; and, if this restraint must control and determine the population of the future, how far will its exercise affect the moral and mental evolution of the race?

If moral restraint and the consequent limitations of families are the peculiar characteristics of the best people in the State, and the absence of these characteristics is peculiar to the worst people of the State, the future of the race may be divined, by reference to the history of the great nations of antiquity.

An accumulating amount of evidence shows that society is face to face with this grave aspect of the population question. The birth rate of the unfit is steadily maintained. Improved conditions of life increase the number that arrive at maturity and enter the procreative period, so that not only are defectives born into the world at a constant rate, but sanitary laws and a growing impatience with the sufferings of the poor tend to so improve their conditions of life as to increase their birth rate and their chances of arriving at adult life.

Shortly stated, then, the problem that society has to solve is this: The birth rate is rapidly declining amongst the most fit to produce the best offspring, while it is steadily maintained amongst the least fit, so that the proportion of the unfit born into the world is annually increasing.

Notwithstanding all the advantages of our boasted civilisation, with its improved conditions of life, the blessings of sanitary science and an advancing preventive medicine, crime is increasing all over the civilised world, insanity is increasing, and physical and mental impotence from epilepsy, imbecility, and pauperism is increasing.

Mr. Douglas Morrison, in "Juvenile Offenders," says: "I have had occasion to point out publicly from time to time that one of the formidable problems confronting civilised communities at the close of the present century is the problem of habitual crime. It is perfectly well known to every serious student of criminal questions, both at home and abroad, that the proportion of habitual criminals in the criminal population is steadily on the increase, and was never so high as it is now. . . . Whether we look at home or abroad, whether we consult the criminal returns in the old world or the new, we invariably find juvenile criminality exhibiting a distinct tendency to increase. It is a problem which is not confined to any single community; it is confronting the whole family of nations; it is arising out of conditions which are common to civilisation."

In "Prisoners and Paupers," Henry M. Boies says, in reference to the eleventh census of the United States: "Some of the disclosures made are shocking, if not appalling, in the highest degree, to our confidence in the future. One of these is the abnormal and disproportionate increase in the criminal class in society. . . . In the last decade, with an increase of 24·5 per cent. in population, the number of the inmates in our penitentiaries, jails, and reformatories has increased 45·2 per cent., or nearly twice faster than the general population. This, too, in spite of the enormous multiplication of churches, Christian efforts of all kinds, schools, educational progress, and various philanthropic agencies of an elevating and conservative nature, attended with a lavish expenditure of money and labour."

Most of the nations of history have recognised the gravity of the population question, but they were mostly concerned with the tendency of the numbers in the State to increase beyond the means of subsistence.

Aristotle declared (Politics, Book II, p. 40) that "neglect of this subject is a never-failing cause of poverty, and poverty is the parent of revolution and crime," and he advocated habitual abortion as one remedy against over-population. The combined wisdom of the Greeks found no better method of keeping population well within the limits of the State's power to support its members than abortion and the exposure of infants.

Since Aristotle's time, abortion has been largely practised by civilised nations. Mutilation and infibulation of females have been practised by savages with the same end in view, while vasectomy, orchotomy, and ovariotomy have had their avowed advocates in our own time.

The purpose of all these measures was to limit population with little or no distinction as to fitness to survive. The Spartans, in ancient times, and many social reformers of to-day have discussed and advocated the artificial limitation of the unfit. The exposure of defective infants is a Spartan method of preserving the physical and mental stature of the race.

The surgical operations on both sexes advocated by some social writers of recent date have not been received with much favour, and, as a social reform, have not been practised. As operations they are grave and serious, profound in their effect upon the individual, and a violation of public sentiment. Anæsthetics and antiseptics have, however, made them possible; and if a surgical operation could be devised, simple and safe in performance, inert in every way but one, and against which there would be no individual or public sentiment, its application as a social reform would go far to solve the grave and serious problem of the fertility of the unfit.

Modern developments in surgical science have made possible a method for the sterilisation of females with these characteristics. As women are the prime factor in procreation, their sterilisation would as effectually stop the birth rate as if men, too, were sterilised. No operation can be performed on men without producing the gravest organic mental and moral changes, and public and individual sentiment can hardly be expected to reconcile itself to the grave consequences of surgical interference, except in the worst classes of criminals.

The unfit are subject to no moral law in the matter of procreation. They can be taught nothing, and they will practise nothing. Like the lower animals they obey their instinct, and gratify their desires as they arise.

It has been seriously suggested that the poor should be systematically taught Néo-Malthusianism methods for the limitation of their offspring.

The best amongst the poor might practise them; the worst certainly would not; and this limitation amongst the best would only increase the ratio of the fertility of the worst.

Of surgical methods castration of males, oöphorectomy, and vasectomy have all been suggested.

Annual castration of a certain number of the children of the popular classes was, not long ago, seriously proposed by Weinhold.

Boies, in his "Prisoners and Paupers," declares that surgical interference is the only method of dealing with the criminal, and preventing him from reproducing his kind. He says: "These organs have no function in the human organism except the creation and gratification of desire and the reproduction of the species. Their loss has no effect upon the health, longevity, or abilities of the individual of adult years. The removal of them, therefore, by destroying desire would actually diminish the wants of nature and increase the enjoyments of life for paupers."

Anæsthetics have ensured that these operations may be performed without the slightest suspicion of pain, and with careful sympathetic surgery pain may be absent throughout the whole of convalescence. Antiseptics have made it possible to perform these operations with practically no risk to life.

Though castration and oöphorectomy can be performed with safety and without pain, they are absolutely unjustifiable operations, if done to procure sterility. Every incision and every stitch in surgery, beyond the necessities of the case, are brutal; and to remove an organ, when the section of its duct is sufficient, is the worst possible surgery.

But vasectomy, though surgically simple and a lesser violation of sentiment than castration, cannot be justified except in exceptional cases.

Neither of these operations makes the subjects of them altogether or at once impotent, certainly not for years. It sterilises and partially unsexes them, and in the end does so completely.

But so grave are the physical and mental changes that follow the operation in the young adolescent, and so violent is the outrage upon man's nature and sentiment, that no objections hurled against the degrading, brutalising influence of the lash would adequately measure the pernicious influence of this operation upon the subjects of it.

TUBO-LIGATURE.

Society must be protected, not so much against the criminal as against the fertility of the criminal; and no rational, practicable, acceptable method has as yet been devised.

The operations on men to induce sterility must be dismissed as unsatisfactory.

But analogous operations may be performed on women. And if women can be sterilised by surgical interference, whence comes the necessity for sterilising both?

Oöphorectomy is analogous to castration. It is an equally safe though a slightly more severe and complicated operation. It can be safely and painlessly performed, the mortality in uncomplicated cases being practically *nil.* The changes, physical and mental, are not so grave as in the analogous operation on the opposite sex, and they vary considerably at different ages and in different cases. The later in life the operation is performed, the less the effect produced: at or after the menopause, no change is noticeable. In many and especially in the younger women, however, grave mental and physical changes are induced: the menstrual function is destroyed, the appearance becomes masculine, the face becomes coarse and heavy, and hair may appear on the lips and chin, lethargy and increase of weight are often noticed, and not a few—especially in congenitally neurotic cases—have an attack of insanity precipitated. On the same principle on which the radical operation on men was condemned, oöphorectomy stands condemned. It is a serious operation, often attended with grave mental and physical disturbances, not the least of which is the partial unsexing of those subjected to it. It is absolutely unnecessary, in order to effect complete permanent sterility.

As the lesser operation, vasectomy, is effectual in men, so is a lesser operation, tubo-ligature, effectual in women; and it has this paramount advantage that whereas vasectomy, being occlusion of a secretory duct, leads to complete atrophy and destruction of the testes, ligature of the Fallopian tube, which is only a uterine appendage and not the secretory duct of the ovary, has absolutely no effect whatever on that organ. A simple silk ligature of each Fallopian tube would effectually and permanently sterilise, without in any other way whatever altering or changing the organs concerned, or the emotions, habits, disposition, or life of the person operated on. The Fallopian tubes have been ligatured by Kossman, Ruhl, and Neuman for the sterilisation of women with pelvic deformities; but all testify to the danger of subsequent abnormal or ectopic pregnancy, and several instances are given. Mr. Bland Sutton relates a case in an article on Conservative Hysterectomy in the *British Medical Journal* for this year.

After numerous experiments on healthy tubes, I have found that simple ligature with even a moderate amount of force in tying will cut the tube through in almost any part of its length. The mucous lining is so thrown into folds that its thickness in relation to the peritoneal layer is considerable. Because of this, the tube when tied alone is brittle, and a ligature applied to it will very easily cut through, and either allow of reunion of the severed ends or leave a patent stump. In a recorded case in which pregnancy occurred after each tube was ligatured in two places, and then divided with a knife, a patent stump was no doubt left.

In order to obviate this danger the peritoneal layer must be opened, and the mucous membrane, which is quite brittle and easily removed, must be torn away for about one-quarter of an inch. A simple catgut or silk ligature lightly tied would then be sufficient to insure complete and permanent occlusion.

CONCLUSION.

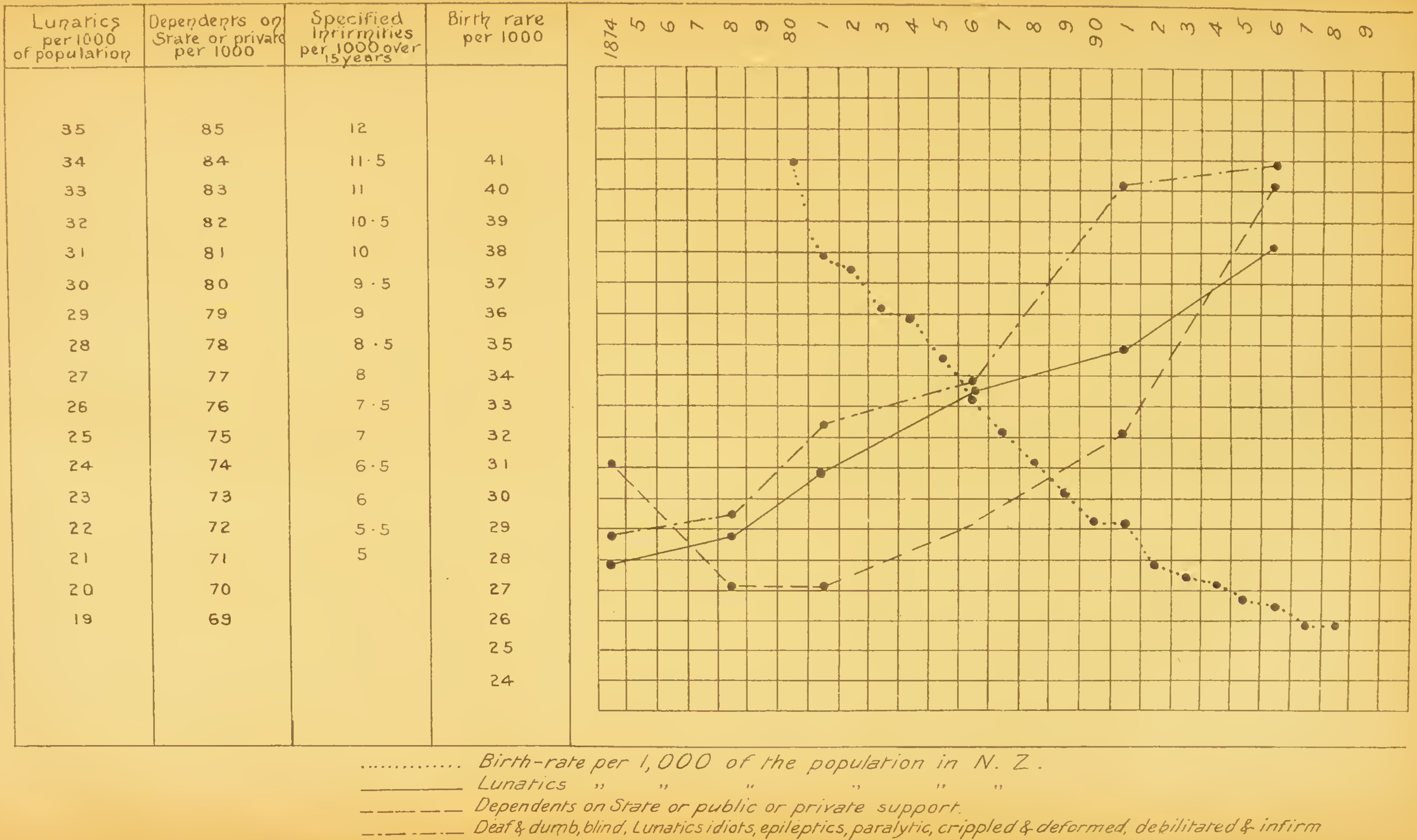
In conclusion, let me briefly review the whole position taken up in this imperfect study of a great question:—

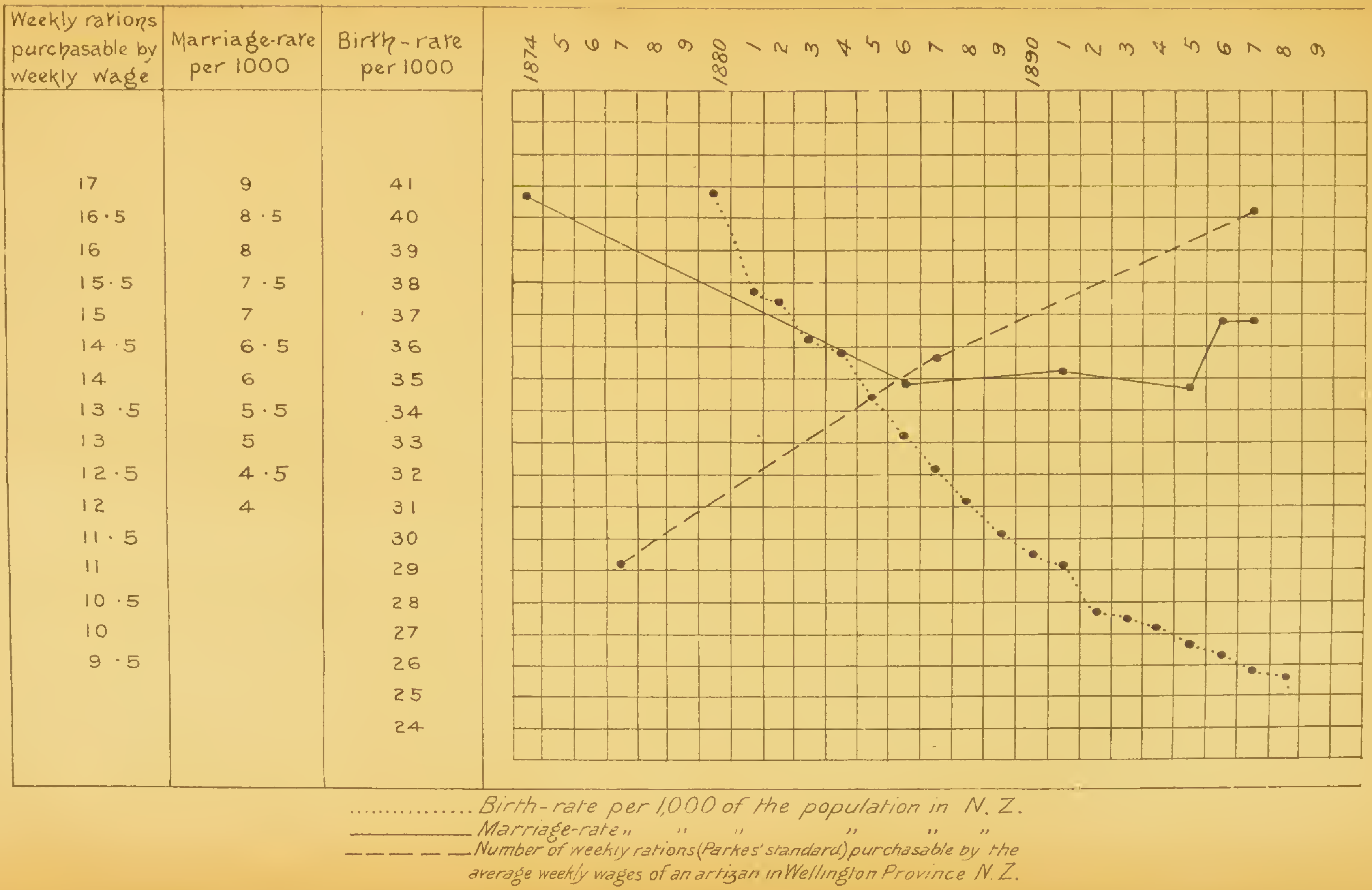
1. The birth rate is rapidly and persistently declining.
2. The food rate is persistently increasing.
3. The declining fertility is not uniform through all classes.
4. The fertility of the best is rapidly declining.

5. The fertility of the worst is undisturbed.
6. The policy of the State is inimical to the fertility of its best, and fosters the fertility of its worst citizens.
7. The infertility of the best stock is due to voluntary curtailment of the family through sexual self-restraint.
8. No such factor does or can obtain as a check to the fertility of the unfit.
9. The proportion of the unfit to the fit is in consequence annually increasing.
10. The future of society demands that compulsory sterilisation of the unfit should be adopted.
11. No method ever tried or suggested offers the advantage of simplicity, safety, effectiveness, and popularity promised by tubo-ligature.
12. The State must protect itself against the collateral danger of artificial sterilisation of its best stock.

HEARTS OF OAK FRIENDLY SOCIETY.

Year.	Number of Members at the Beginning of of each Year.	Number of Cases of Lying-in Benefit Paid during Year.	Percentage of Cases paid to Total Membership at beginning of Year.	England and Wales—Births per 1,000 of the Total Population.
1866	10,571	2,300	21·76	35·2
1867	12,051	2,853	23·68	35·4
1868	13,568	3,075	22·66	35·8
1869	15,903	3,509	22·07	34·8
1870	18,369	4,173	22·72	35·2
1871	21,484	4,685	21·81	35·0
1872	26,510	6,156	23·22	35·6
1873	32 837	7,386	22·49	35·4
1874	40,740	9,603	23·57	36·0
1875	51,144	12,103	23·66	35·4
1876	64,421	15,473	24·02	36·3
1877	76,369	18,423	24·11	36·0
1878	84,471	20,409	24·16	35·6
1879	90,603	22,057	24·34	34·7
1880	91,986	22,740	24·72	34·2
1881	93,615	21,950	23·45	33·9
1882	96,006	21,860	22·77	33·8
1883	98 873	21,577	21·82	33·5
1884	104.339	21,375	20·51	33·6
1885	105,622	21,277	20·14	32·9
1886	109,074	21,856	20·04	32·8
1887	111,937	20,590	18·39	31·9
1888	115,803	20,244	17·48	31·2
1889	123.223	20,503	16·64	31·1
1890	131,057	20,402	15·57	30·2
1891	141,269	22,500	15·93	31·4
1892	153,595	23,471	15·28	30·5
1893	169,344	25,430	15·02	30·8
1894	184,629	27,000	14·08	29·6
1895	201,075	29,263	14·55	30·4
1896	206,673	30,313	14·67	


Lunatics per 1000 of population
35
34
33
32
31
30
29
28
27
26
25
24
23
22
21
20
19
Dependents on State or private per 1000
85
84
83
82
81
80
79
78
77
76
75
74
73
72
71
70
69
Specified Infirmities per 1000 over 15 years
12
11·5
11
10·5
10
9·5
9
8·5
8
7·5
7
6·5
6
5·5
5
Birth rate per 1000
41
40
39
38
37
36
35
34
33
32
31
30
29
28
27
26
25
24
1874 5 6 7 8 9 80 1 2 3 4 5 6 7 8 9 90 1 2 3 4 5 6 7 8 9
.... Birth-rate per 1,000 of the population in N. Z.
———— Lunatics " " " " " "
— — — — Dependents on State or public or private support.
—·—·— Deaf & dumb, blind, Lunatics idiots, epileptics, paralytic, crippled & deformed, debilitated & infirm


Weekly rations purchasable by weekly wage
17
16·5
16
15·5
15
14·5
14
13·5
13
12·5
12
11·5
11
10·5
10
9·5
Marriage-rate per 1000
9
8·5
8
7·5
7
6·5
6
5·5
5
4·5
4
Birth-rate per 1000
41
40
39
38
37
36
35
34
33
32
31
30
29
28
27
26
25
24
1874
5
6
7
8
9
1880
1
2
3
4
5
6
7
8
9
1890
1
2
3
4
5
6
7
8
9
……………Birth-rate per 1,000 of the population in N. Z.
————Marriage-rate " " " " " "
— — — —Number of weekly rations (Parkes' standard) purchasable by the average weekly wages of an artizan in Wellington Province N. Z.

FRACTURED RIBS IN THE INSANE.

By J. F. Flashman, M.D.,
Callan Park Asylum, Sydney.

Some little time ago an unfortunate case occurred at the Hospital for Insane, Callan Park, Sydney. It formed the subject of a coronial inquiry and subsequently was freely commented upon by the daily Press. Since cases of a similar nature, though rare, may at any moment occur in any hospital for the insane, I thought it might be interesting to bring it before you at this Congress, together with the records of many similar cases which I have been able to find. It may be that some of you have come personally into contact with such cases.

The case I refer to was that of a man—a Norwegian, aged thirty-eight years, who worked as a wharf labourer.

He was admitted to the hospital on 24th October, 1898 The case was diagnosed as one of general paralysis of the insane, with marked locomotor symptoms Immediately prior to or just after admission he was considerably depressed, having an idea that he was suffering from an infectious disease which he had communicated to his wife and children. He was so impressed with this idea that he was unwilling that his children should go out into the public streets, lest they should communicate the disease to others.

Physically, on admission, he was found to be fairly well nourished, but he was very weak, especially in his lower extremities.

His speech was slurred, his tongue and facial muscles were tremulous, and his pupils were unequal. Knee jerks were absent, and his walk was characteristic of locomotor ataxy.

He was troubled at times with retention of urine, and later on with constipation.

Those who see many cases of general paralysis of the insane will, I think, admit that the condition of melancholia in which the patient was, on admission, in no way disproved the diagnosis of general paralysis. Of course, the classical case does not pass through a period of marked depression; but then the majority of cases, are not classical, and at Callan Park we find that dozens of cases, admitted as cases of melancholia or as cases of mania, afterwards develop unmistakable signs of general paralysis of the insane. It was so in this case. After a time his depression passed entirely away, and he became happy and contented, in spite of the fact that he was a physical wreck.

This feeling of *bien être* was for a time the only idea of the patient's which could in any way be described as grandiose. However, towards the end of March of this year he became very talkative, and developed ideas of fabulous wealth He gradually became more excited and more loquacious. At times he would attempt to turn somersaults to show his wonderful agility. He soon became destructive both by day and night. Owing to his extreme restlessness and destructiveness, it was found necessary to confine him in a single room. He was given a mattress on the floor, the remainder of the floor space being covered with straw. His bedclothes at first consisted of the ordinary sheets and blankets, but later it was found necessary to substitute rugs. Even these he at times destroyed, tying the fragments round his limbs in grotesque adornment Besides his bedding, the only other article in his room was a pot of stiff rubber about 8 inches across and about 6 inches deep.

About the morning of the 2nd May I saw the patient stoop down, rest the palms of his hands on the floor, and throw his hind limbs off the floor to show how well he felt.

On the morning of the 4th the patient was much quieter. He complained of nothing, but as it was noticed that his tongue was dry he was ordered stimulants. The next day he was apparently in the same condition. On the morning of the 6th he seemed to be weaker, and was carried into the hospital-room.

In the afternoon I was sent for, and found the patient much worse. His temperature had risen to 103° Fahr., and his breathing was rapid. I immediately surmised that the patient had had a fit the previous night, and that his symptoms were due to that.

Thinking, however, that he might have developed pneumonia, I made an examination of the chest. My examination in front showed the apices to be resonant on percussion. On auscultation I heard nothing suspicious.

I then had the patient rolled gently to one side, so that I might examine the bases of the lungs. Both were slightly dull, the right one more so than the left. On auscultation, soft râles were heard on both sides with some indistinct friction. As the patient was talking incoherently the whole time, it was quite impossible to make anything like a minute examination. Being turned on his side, I noticed, seemed to distress him considerably.

I concluded that his condition was due primarily to one of those epileptiform convulsions which are so commonly the immediate forerunners of death in cases of general paralysis of the insane. The condition of his lungs I considered to be of quite subsidiary importance. I sent for his friends and informed them of his condition, and what I thought was the cause. He died in their presence during the night.

As is usual, a *post-mortem* was asked for. There was no objection on the part of the friends, and the examination was carried out next morning.

There were no marks of bruising on the body, the only mark visible being a small abrasion on one heel.

On making the first incision I was surprised to see a little pus ooze out of the tissues just over the centre of the sternum. On reflecting the skin over the chest I was amazed to find five of the ribs on the right side and four on the left fractured. The sternum was also fractured across the centre. On further examination it was seen that there was a little purulent collection round the ends of the broken bones. The fractures were situated immediately outside the junction of the ribs with their cartilages. There was a certain amount of thick lymph deposited on the pleura immediately below the fractures. This lymph was peeled off, and I passed my fingers over the pleura beneath the fractured ends. I was unable to detect any rupture of that membrane. I further found that the ribs were in an exceedingly brittle condition—so much so that I was able to take a rib between my finger and thumb and snap it off into lengths of about an inch.

The case was reported to the coroner, who decided to hold an inquest.

The point to be cleared up was, of course, How had the man received the injuries? There were two theories chiefly favoured at the time. The first was that the man had been subjected to undue violence at the hands of the attendants. The second was that the man had fallen forward whilst in his room, striking his chest upon the rubber pot, and had so produced the fractures.

Personally, I am now of the opinion that both theories were wrong, and that all the injuries described were in all probability produced by the falls and bumps incurred by the patient when amusing himself with his apparently harmless acrobatic performances.

The question of personal violence at the hands of the attendants was carefully gone into by the coroner and the jury; they failed to find the slightest evidence against anyone. It also came out in evidence that the patient was a general favourite with those around him. I was so assured of the trustworthiness of the attendants in whose care he was, that I set to work to try and throw some light upon the matter.

One of the first cases of a similar kind which I came across was the report of a general paralytic who died from the effects of "frightful injuries received in Hanwell Asylum" The injuries were: "The breast bone was broken; the third, fourth, sixth, and seventh right ribs, and the fourth, fifth, sixth, and seventh left ribs, were also broken; the chest was bruised." A medical critic added—"It is sufficiently plain that, however the event may have happened, the poor fellow was effectually crushed to death" "We are driven," he further adds, "by a study of the evidence to one of two equally painful conclusions: either that the supervision and care of patients in Hanwell Asylum are so grossly defective that injuries such as these may be inflicted without anyone knowing anything about the manner of their infliction, or that there has been a conspiracy of silence."

I am glad to be able to say that before I conclude the reading of this paper I will produce ample evidence to prove that in all probability the harsh phrases used in the report from which I have just quoted were entirely undeserved, and that the conclusion come to by the medical critic was equally wrong.

Another case was that of a male, æt. fifty-two, "of large size," suffering from general paralysis, his case being characterised by "great restlessness and violence." During life he "neither admitted that he suffered any pain, nor gave any indication of so doing." Death arose apparently from "cerebral compression." At the *post-mortem* examination attention was, therefore, directed mainly to the brain. "After I left the dead room," says the medical man who reports the case, "believing I had seen all that the case afforded, my assistants proceeded to examine the rest of the body. They were surprised to find pus diffused beneath the muscles of the left side (of the thorax), and fractures of five ribs running in a vertical straight line a short distance from the junctions with the cartilaginous portions. No reunion has taken place. There was no reason to doubt that the fractures of the ribs had taken place before the patient's arrival at the asylum. The rectilinear course of the fractures appeared to indicate that they had resulted from a fall forward on some hard, narrow surface, such as the edge of a board or plank. The account given as to his violence and restlessness corroborated the supposition. This patient appeared not only free from pain or muscular impairment up to the period when symptoms of cerebral or cerebro-spinal compression showed themselves, but he preached and shouted perpetually.

The next case was that of a male, aged thirty-three, "furious and dangerous, restless, noisy, and destructive" on admission, but who "subsequently became quiet and harmless. During life he complained of no pain, and had no cough. The immediate cause of death was, nevertheless, hydrothorax. *Post-mortem* examination revealed the fracture of seven ribs, the appearances proving that the fractures here also had occurred prior to admission. In neither of these cases was any lesion of the ribs either diagnosed or suspected during life. Neither of the two would have been known without *post-mortem* examination."

Dr. Lander Lindsay, of Perth, Scotland, points out as a difficulty in the diagnosis of these cases the absolute insensibility of many of the victims—no pain.

He mentions several cases—one of fractured ribs and sternum, and one of fracture of the arm—all in acute mania. The injuries were accidental, or produced by the patients themselves. In no cases did the patients complain of pain or injury.

Dr. Lindsay concludes :—

1. Surgical injuries sometimes occur among the insane without external marks of violence ; and that—
2. Serious organic lesions frequently exist without relative symptoms during life.

He says that none but those habitually engaged in the management of lunatics can be aware of the extent to which accidental or self-inflicted injuries occur, or of the exceptional character of these injuries.

They are exceptional in so far as it is (1) frequently difficult to understand how they could have been inflicted (I refer to cases in which all ill-usage by attendants has been impossible), and (2) in so far as serious structural lesions may be developed without the usual accompanying or proportionate physical indication, or without vital symptoms of any kind. Thus, I have known almost all the ribs of a young man's side broken without a single outward indication, or the exhibition of any kind of symptom. No complaint ever emanated from the patient ; there was no bruise, mark, no lung symptom, no indication of the slightest suffering from first to last. Nor was it ever discovered how the injury was inflicted. The fractures were detected accidentally by manual palpation. The patient was confined to bed for some days, his thorax tightly swathed in flannel merely as a precautionary measure ; but no chest or other symptoms were ever developed, and the patient never could comprehend why he was confined to bed and swathed in flannel.

In a paper on "Muscular Action, a Cause of Fracture among Paretics," by Dr. C. B. Burr, Superintendent of the Eastern Michigan Asylum, Pontiac, he says :—

"I refrain from taking up your time in discussing the question of the fragility of bones in paresis, and would merely point out that there is among surgeons a perfect unanimity of belief that the existence of ataxic diseases renders the bones peculiarly liable to fracture." After quoting authorities in support of the general position that the effect of nervous diseases is to render the bones liable to serious injury from apparently trifling causes, he mentions his cases occurring in the asylum under his care.

The first case was that of a female patient who for some months previous to the accident had been suffering from general paresis. Whilst obeying a call to the dining-room in a wrong direction, she was recalled by an attendant, and, in suddenly hurrying to retain her footsteps, she fell to the ground, sustaining a fracture of the right femur in the lower third, the soft part of the limbs betraying no evidence of any bruise or contusion.

The second case was that of a male patient who was out walking on an even board walk, and in attempting to turn fell to the ground, the attendants perceiving an audible snap. On examination it was found that he had fractured the tibia and fibula in the lower third.

In both cases the opinion of Dr. Burr is that the fracture occurred owing to the inequality of the muscular action upon bones rendered fragile by disease, and that they were the causes and not the result of the fall.

The following is the report of an accident to a patient of St. Andrew's Hospital, Northampton : -

It was a case of a male patient suffering from general paralysis. His thigh bone was fractured while he was being dressed by his attendant. His leg was stiff and bent at the knee, as is often the case with bed-ridden persons ; and when the attendant attempted to lift the patient up in a sitting position to dress him, the thigh bone snapped from the weight of the body. The patient recovered from the injury, but he died some time after from general paralysis. The committee of management were satisfied that no violence was used.

Another case is reported by Dr. Benham, who, in speaking of the patient, remarks :—

"I examined him when in the bathroom, and no fracture or displacement of the ribs was discoverable. Owing to his becoming restless and excited when his body was touched, I was unable to apply the stethoscope to his chest.

"Patient immediately before admission had become very violent."

Extracts from the casebook are as follow :—

"16th December.—In a state of maniacal excitement; took nothing. At night he was reported to have been very noisy and excited all day, and to have taken no food; had torn two shirts to pieces, and his pulse was very weak. He was carried on to a couch outside his room, and was fed by me with the aid of a stomach pump. To the feeding mixture was added 2 oz. of whisky, ½-oz. of tincture of hyoscyamus, and a dose of house medicine. He was restless whilst being fed, but no violence was used, and, particularly, no pressure was applied over the chest. After this I saw him bathed and put to bed. This was the only time he left his room.

"17th December.—Stronger to-day; pulse better; took food well, and was quieter.

"18th December.—Refused his breakfast; later, took from me 2 oz. of port wine and a quarter of a pint of beef tea. His hands were confined in gauntlets, as he tore everything to pieces. At 1 p.m. he took 2 oz. of port wine and a little milk. His pulse was still very weak. At 2·30 p.m. he took nearly a pint of beef tea; at 4·45 p.m. he took 2 oz. of port wine and a pint of milk; and at 5·30 p.m. he took 2 oz. of port wine and ½-pint of milk. From this time he became very excited, and refused all nourishment. He gradually sank, and died at 7·35 a.m. on 19th December.

The extract from the *post-mortem* notes which follow were taken by Mr. H. Rimmington Mead :—

"Autopsy, twenty-nine hours after death :—Rigor mortis universally present; left lung, weight 12 oz.; right lung, weight 15 oz. Both collapsed. Heart, weight 11 oz. Pericardium full of fluid; right ventricle full of clots. Brain much wasted generally; weight 36 oz. Arachnoid membrane dull and opalescent. About 6 oz. of fluid escaped on the removal of the skull-cap. Both lateral ventricles were full of fluid, and the brain substance was generally congested. On section of the right kidney a small quantity of cystic pus escaped: the seventh to third ribs on the left side were found to be fractured at from 3 inches to 4 inches from the sternum. There were no external marks of injury, and no displacement existed. There was a small quantity of dark grumous-looking fluid effused opposite the fracture of the seventh rib. The ribs were in a very advanced state of degeneration, and almost broke under the finger. Nothing else called for particular notice.

"Immediately after the *post-mortem* I communicated with the coroner, who held an inquest at the asylum on 24th December. After a lengthened inquiry, the jury returned a verdict to the effect that the patient died of 'Exhaustion after acute mania.'

"*Remarks.*—This case is interesting in view of the fact that when a patient in an asylum is found *post-mortem* with broken ribs a suspicion that undue violence has been used by someone connected with the establishment is almost sure to be raised. The above case tends to demonstrate how erroneous such a view may often be. This man, owing to the terror he had displayed on being touched on his body, had, though nothing could be detected from external examination, suggested to me the idea that some injury might possibly have happened to him previous to admission. He was placed under special supervision, and visited at all hours. Mattresses were placed over the floor of his room, so that he should, so far as possible, be safe from injury when restless. On the only occasion, as before-mentioned, upon which he left his room, I was present, and therefore am enabled to express a confident opinion that his injuries had not been received here. Of course it is perfectly well known through the 'specialty' that fractured ribs with no displacement may occasionally be found in advanced cases of brain diseases which are never detected during life, but it is by no means always easy to convince a coroner's jury of this. Owing to the precautions adopted from this patient's admission it was not difficult to show how improbable, to say the least, it was that the injuries could have occurred here, nor could any blame be attached to the workhouse officials. My own view is that the fractures were not of very recent origin. His wife's statement to me, after the patient's death—that he, a blind man, had got out of bed when alone in his bedroom and broken the furniture, and used such violence as to break a large hole in the roof—indicates to my mind a ready way in which the injuries might have been received.

"The fractures were all situated from 3 inches to 4 inches from the sternum, and the bones were in such a state of degeneration that they would break up on the slightest pressure. There was no displacement of the broken bones whatever, and no injuries to the walls of the chest or respiratory system could be detected, so that it is evident that the fractured ribs could not have in any way contributed to the cause of death, which was, in my opinion, due to the state of the brain. This view was also taken by the jury, who had the advantage of a medical coroner to direct their investigations."

I find that Dr. Campbell, Pathologist to the County Asylum, Rainhill, Lancashire, made some investigations in the year 1893 on this very subject. With an instrument devised by Dr. Mercier, he made a series of investigations on the "breaking strain of the ribs in the insane." The following are some of his conclusions:—

1. The breaking strain of the ribs of general paralytics is considerably below the normal standard. Taking the breaking strain of the eighth rib of a healthy adult male as equivalent to 62 lb. against the convexity and 65 lb. against the concavity, I find that the ribs of the thirteen male general paralytics I have examined show an average breaking strain of 44·8 lb. against the convexity and 4·4 lb. against the concavity.

2. In cases of senile insanity the breaking strain is exceedingly low. This is particularly evidenced in the case of females, for, taking the breaking strain of the eighth rib of a healthy adult female as 29 lb. against the convexity and 30 lb. against the concavity, my six female cases of senile dementia yielded the extremely low average of 11·8 lb. against the convexity and 11·3 lb. against the concavity.

3. In most of the varieties of insanity the breaking strain is diminished. The average breaking strain of the fifty-eight cases tested is, in the case of the thirty-five males, 41·04 lb. against the convexity and 42·14 lb. against the concavity; and in the case of the twenty-three females, 20·68 lb. against the convexity and 20·90 lb. against the concavity.

4. Sex is an important factor in reducing the breaking strain. The female rib would seem to be as nearly as possible half as strong as the male.

5. The breaking strain of the rib varies proportionately with the age of the individual. It increases steadily from youth upwards till about the age of thirty-five, when it reaches its maximum, and it progressively diminishes with senescence.

6. In these experiments the difference between the breaking strain against the convexity and that against the concavity was not so great as I expected. In the majority of instances the breaking strain against the concavity was the greater, but the excess was not a large one.

Microscopical Examination.—Turning in the next place to the microscopic examination of the ribs tested, and the results obtained therefrom, I would at the outset state that in every instance in which the breaking strain was ascertained to be a low one, I was able to demonstrate some deficiency in the structures which go to constitute the architectural strength of the bone, and thereby I, in a measure, checked the accuracy of Dr. Mercier's instrument.

Although from the above quotations it would seem on the average the breaking strain of the ribs of general paralytics is reduced by about 17 lb. against the convexity and 20 lb. against the concavity, yet we find that in extreme cases it may be lowered as much as 46 lb. against the convexity and 47 lb. against the concavity.

It further appears from his report that it is not every case of general paralysis which shows the brittleness of the bones. For instance, he quotes one case of a general paralytic in the last stage, the breaking strain of whose ribs was 60 lb. and 45 lb. respectively. One of his cases of senile dementia had a breaking strain of only 8 lb. and 7 lb. respectively. One of his cases of melancholia had a breaking strain of only 17 lb. and 15 lb.; one of his cases of epilepsy, of 24 lb. and 27 lb.; one case of organic dementia, of 14 lb. and 12 lb.; and one case of acute mania, of 17 lb. and 15 lb.

Dr. Campbell also points out that owing to the small amount of violence used in fracturing the rib there is but little displacement, and that on account of the softness of the bone there is no crepitus.

My opinion coincides most completely with Dr. Campbell's view, and I know, from my personal experience of the Callan Park case, how easy it is for a medical man to pass over such serious injuries as fractured ribs unless he has some suspicion beforehand that such a condition exists.

Before concluding, I would like to quote some remarks which were made by Dr. Mercier when commenting upon the paper by Dr. Campbell, from which I have so freely quoted.

Dr. Mercier said this very interesting subject was first brought to his mind very forcibly by an inquest. In his early days he was medical officer to a large workhouse, and had occasion to send a patient to one of the Lancashire asylums. The superintendents of the Lancashire asylums were at that time very arbitrary gentlemen, and when the patient arrived the superintendent would not take him in. An interval of two days ensued before room could be found in another asylum to which he was sent. Before going to the first asylum he was very carefully examined, but before going to the second the examination was not repeated owing to the fact that he (Dr. Mercier) was crippled in both hands by an accident. When he got there it was discovered that he had about twelve ribs broken. Death ensued, and there was an inquest and a *post-mortem*. He attended them; and before the doctor was able to say that the ribs were extremely brittle, the coroner's comment was, "Oh! yes; I dare say." As a matter of fact, he could take the ribs up in one hand and break them off again and again in pieces of half-an-inch or an inch with his thumb. They were extremely brittle, but he was very much impressed by the fact that it was impossible to make the coroner or jury believe that they were abnormally so. They seemed to associate together the brittleness of the rib with the toughness of the story, and to consider that they were inversely proportional to one another. It occurred to him that it would be a very good thing if one could devise an instrument by which the actual breaking strain could be measured. Many investigations had been conducted with regard to this quality of ribs; and ribs had been decalcified, weighed before decalcification and afterwards, and the breaking strain endeavoured to be deducted in that way. It had always occurred to him that, instead of adopting this method and guessing from the results whether the rib was fragile or not, it would be better to break the rib and to determine how much force was required, and that with a full series of experiments made in that way they would be able to go before a coroner with a good face and to say, "These ribs are brittle, and to this extent, that they break with a force of something like one-twelfth of that required to break a normal rib."

With these remarks of Dr. Mercier I will conclude, only adding that I beg to thank you sincerely, gentlemen for your courtesy in giving me such an attentive hearing.

REFORM IN QUARANTINE.

By W. L'E. Eames, M.B. Dub.
Newcastle.

Federation is now all but an accomplished fact, and quarantine is one of the services which the Federal Parliament will at once take over. It behoves us, therefore, to consider the subject very carefully, in consequence of its bearing on commerce. Prior to the year 1892, quarantine was practised only on infected vessels—that is, with infectious sickness on board; but since that time we have been making our quarantine regulations more stringent, whilst the necessity for doing so is daily becoming less. Every vessel from an infected port is now not only strictly quarantined until the crew have been examined, but, whether there is sickness or not, she is fumigated and forced to pump out every drop of her drinking water; and *until she has emptied* every drop of water from her tanks, no one is allowed to leave the ship.

But the matter does not end here. If sand or soil ballast is carried, this has to be lightered to sea, which, in my opinion and that of the Newcastle medical profession, is an unnecessary measure. Prior to the year 1892, as soon as a port was proclaimed free from sickness, the name was taken off the proclaimed list; but now, once a name goes on that list there is no hope of getting it off again; and when I tell you that pretty well the whole of the East is on that list you can well understand what an effect a quarantine of this sort, which is ever increasing in stringency, is bound to have on our commerce, and how it affects our trade with the East. I will read you a letter from Mr. Earp, President of the Newcastle Chamber of Commerce, which bears out this point:—

> I can positively state that the various restrictions which have been placed upon the shipping, together with other various drawbacks, have impaired the reputation of the port and diverted tonnage, which would have come to Newcastle, to other destinations for loading.
>
> The matter of ballast reached a very acute stage during the recent months, and the new regulations issued about six months ago ordering stone ballast to sea brought about a climax.
>
> Fortunately, both for the shipping trade and for the sake of the harbour works in progress, the order regarding stone ballast was withdrawn, but no definite instructions appear to have been issued regarding other ballast, notwithstanding that when a deputation from the Newcastle Chamber of Commerce waited on Dr. Ashburton Thompson, early in July last, a promise was made that the list of proclaimed ports would be revised. As the stone ballast forms about one-third and the other descriptions two-thirds of the total, the relief afforded in regard to stone does not meet the case.
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> Shipowners with vessels in Eastern and Australian waters have many options in regard to the ports they shall send their vessels to for loading, and, in deciding this matter, of course take into consideration, in addition to the freight ruling, the expenses of the ports and the despatch obtainable; and it would even pay them better to send their vessels across the Pacific in ballast than suffer the losses ranging at the port of Newcastle owing to detentions getting rid of ballast at this port.

> There is a good demand for Newcastle coal at the present time for San Francisco, Hawaiian Isles, Eastern and other ports, but very little tonnage available to execute same. A San Francisco house writing under 12th July says:—"There is but very little doing in Australian coals in the market just now. This condition is brought about by the scarcity of tonnage available at Newcastle. Orders for other ports have remained for many months unexecuted owing to vessels being unprocurable." It is thus seen how Newcastle can ill afford to drive away shipping from its harbour by hyper-stringent rules and regulations entailing heavy losses upon the owners.
>
> I append a list of vessels which came from the Eastern ports during the months May, June, and July, and which were subjected to great delay in getting rid of their ballast. This bears upon clause 3 hereof, and would seem to show that the result to Newcastle shipping has been short, sharp, and effective.
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> List of vessels which have arrived at Newcastle in ballast during the months April, May, and June, 1899, during which months great delay occurred in sending the ballast to sea:—
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> Stenbeck, from Kobe; Andromeda, from Shanghai; Tenasserim, from Manila; Jacques, from Hungary; Argus, from Manila; Haddon Hall, from Yokohama; Cabul, from Samarang; Albyn, from Yokohama; Belmont, from Shanghai; Falkirk, from Samarang; Alton, from Sourabaya; Langford, from Java; Lindley, from Sourabaya; Verajean, from Nagasaki; Scottish Moors, from Sourabaya; William Tillie, from Hiojo.
>
> As showing that the shipping from Eastern ports in ballast has almost ceased there is only one vessel now listed to arrive—namely, Ancona from Manila.
>
> A large colliery company complained very severely during the months mentioned of the delay to their vessels (included in this list). They had received notification from owners almost amounting to threat of withdrawal of the vessels, thus showing the unfavourable impression being produced in the minds of shipowners at home—an impression bound to have its logical results—namely, in the avoidance of the port when possible to load ships elsewhere.

Quarantine is instituted as a measure principally for preventing the introduction of plague, cholera, yellow fever, and smallpox. Now, in order to study its effects, I will read you the editorial of the *British Medical Journal* of 12th September, 1891, says:—

> Those which cling to it most tenaciously are the most neglected countries from a sanitary point of view, those in which sanitary progress was almost unknown, and those which suffered most from cholera. Nations which subject themselves to the disabilities and drawbacks of quarantine are not inclined at the same time to spend money on sanitary measures. The fact, indeed, of being promised protection against disease by means of quarantine seemed a reasonable objection to incurring expense on work of sanitation; and the more ignorant the population the more they preferred the risks and drawbacks of quarantine to the certainty of having to pay for improved water supply, drainage, and removal of town refuse.

And so on. No one would think Australia, with her sparse population, her boards of health, free education, waterworks, and Public Health Acts, was referred to in this article; but I can assure you it is in the following terms:—

> *The experience of Australia in relation to smallpox.*
>
> So long as the inhabitants are taught that, on suspicion, whole shiploads of people, healthy or not, will, irrespective of consequences to them, be detained in quarantine, with a view of protecting the colonists against smallpox, why should these latter subject themselves to the inconvenience, trivial as it is, of vaccination? Sanitary administration in relation to cholera and other diseases stand in precisely the same position to cholera and other diseases as vaccination does to smallpox; and quarantine measures applied to one or other disease constitute a distinct hindrance to the adoption of the only true measures of prevention.

How true this is every medical man in the country can testify, and what a broken reed to trust to.

Here, in New South Wales, hardly one person in ten is vaccinated; and as for the effect on trade, there is hardly a ship in Newcastle, which at this time of the year is unprecedented. For a few moments I am going to deal with the regulation insisting on earth or sand being sent to sea, and its relation to smallpox, yellow fever, plague, and malaria.

About smallpox the soil has never been questioned.

Yellow Fever.—Until Sanarelli discribed the micro-organism which causes yellow fever, in 1897, this disease was looked on by many as essentially a mud or soil contained poison, but his experiments go to prove that the essential factor for the development of colonies of icteroid microbes is the presence of mould. *British Medical Journal*, 3rd July, 1897, says :—

> The common moulds of the atmosphere are the great protectors of the bacillus icteroides. It is indeed highly probable that, especially in the holds of badly ventilated vessels, the constant heat and humidity, considered in their physico-chemical effects, are the conditions which maintain so long in life the microbes of yellow fever accidentally arrived there. We must then consider moulds as the natural protectors of the specific agent of yellow fever, because it is solely through their intervention that the latter can find the force to live and multiply, whereas the unsuitableness of the nutritive means or the unfavourable action of the developing temperature would render its existence altogether impossible. It is then probable that the factor of humidity, whether on board ships, or on the coasts, or in the interior of countries, represents the principal coefficient of a biological phenomenon rather than that common meteorological influence, the action of which is always identical in the ætiology of almost all the epidemic diseases. On the other hand the notable resistance presented by the bacillus icteroides towards the principal factor of natural disinfection—that is to say drying, and its great longevity in sea-water, sufficiently explain the ready acclimatisation of ictriod typhus and its obstinate persistence, especially in the maritime localities afflicted by the immigration of its specific agent.

Plague.—Kitasato and Yersin have demonstrated that the specific cause of plague is a cocco-bacillus which occurs in great profusion in the characteristic buboes, also in the spleen, intestines, lungs, kidneys, liver, &c. The most potent circumstances which predispose to the epidemic outbreak of plague are *extreme filth and overcrowding*, an atmosphere saturated with the emanations of the sick; a lowered tone of the general health; abundant saturation of the soil and surrounding media with animal refuse, fitting them as a nidus for what might be termed natural culture of the germ; abundance of body vermin of all kinds; abundance of other vermin such as rats and mice, which serve as multiplices of the virus; carelessness about personal cleanliness, about wounds of the hands and feet, about clothing, about food-dishes and water. Plague bacilli have been detected in the dust and earth from the floors of plague-stricken houses.

Cholera.—(Notter and Firth): "It is now generally accepted that the particulate contagium of cholera is the specific micro-organism called the comma bacillus. Whether it is by this particular organism alone, or whether it is only when in conjunction with some other, as yet unknown, microbe, that the symptoms of cholera are generated, the general belief prevails that cholera is mainly spread by means of the drinking water, though dissemination may occur in other ways—more particularly from an 'excrement-sodden earth,' which fouls not only water, but air. Koch, who discovered the comma bacillus, made the very interesting observation that the comma bacilli die very rapidly when dried. Hence it is inferred that no living comma bacillus can exist in dust, and that the transport of living comma bacilli through the air is impossible" (Poore).

Dempster's experiments indicate that in dry soils, evaporation not prevented, comma bacilli were alive on the third but dead on the fourth day. Now, the average length of a voyage to New South Wales is sixty days—never less than twenty-seven days; and anyone watching the discharge of ballast will be struck by its dry nature. So that even if comma bacilli were placed in the ballast they would be perfectly harmless by the time they arrived here.

Malaria.—I need only refer you to Patrick Manson's book on tropical diseases, and an article which appeared in the *British Medical Journal*, 1st July, 1899, by Major Ronald Ross, for all information, which seems to definitely prove that the cause of malaria does not exist in the soil, but is due to a certain parasite developed in a mosquito and injected into man ; furthermore, that it is not every kind of mosquito that is capable of becoming malaria-bearing. Since the introduction of malaria seems to depend on a certain kind of mosquito which does not exist here, it, therefore, appears there is no occasion to shut out these soils coming from malarious countries, but to rather turn our attention to the life history of the mosquito, and prevent the conditions arising here which would prove favourable to its development.

Now, a visit to Carrington, a municipality adjoining Newcastle, will convince even the most sceptical that the work in progress there is calculated to lessen the opportunities for the development of the mosquito—by filling up marshy ground which, at the present time, simply hums with our native variety; and the natural inference is to fill up all pools and puddles as quickly as possible, which might become breeding grounds for the malarial variety should it be accidentally introduced.

Having considered the various diseases which have been looked on to a certain extent as soil diseases from the latest available data, I think we are justified in considering that, to become harmful in the soil, even if the germs are placed in it, certain conditions are necessary. Do these conditions apply to earth or soil ballast as brought to Newcastle ? This is in almost every instance taken from the side of a hill or from the sea-shore; but wherever it comes from, it has to be shovelled into a basket or truck, which thoroughly aërates it, then taken to the ship and emptied into the hold, which aërates it again. The hold of the vessel is lined with boards, beneath which are the bilges. If there is any moisture remaining in the ballast, this will drain into the bilges, and be pumped out. Vessels coming to Newcastle average some sixty days.

Now, if there was anything deleterious in the ballast surely it would show some evidence on the crew who worked it into the ship; inhale its exhalations ; frequently swallow some of the dirt in their food, which anyone who knows anything of a forecastle will readily admit ; sleep with it in their working clothes hung right alongside of them ; soil their wounds—such being of common occurrence in their employment—and live over it for an average of sixty days, in all weathers, hot and cold, fine or wet. Surely if there is anything to be feared from such soil, it is bound to show itself, and I think we may safely conclude that, in the event of a vessel arriving free from sickness, there can be no possible danger to those who simply view it for years from a distance. Again, even if infected earth were implanted, unless conditions favourable for the germination of the special micro-organism implanted exist, no danger will result. Now, anyone visiting Carrington, and observing the discharge of ballast, must say that, far from such conditions being produced or existing, a great national work is being done, which will benefit the State by millions of pounds, and at the same time convert what might prove a possible danger into very desirable land. There is another point: The treatment which the ballast receives in being worked into a ship is the very best possible for the disinfecting and purifying of infected earth, especially when the modern ship is in question, dry, well ventilated, and with no danger of excreta draining into the hold, as was frequently the case in the older type of vessel.

So much for the ballast. With regard to the pumping out of water, whatever reasons there may be for vessels with sickness on board doing so, none exist when they have a clean record ; and in any case to keep a ship and all on board in strict quarantine while the water is being pumped out is surely a very arbitrary and unnecessary measure.

Quarantine for Smallpox.—The futility of quarantine for smallpox in the days of fast steaming has been so frequently pointed out, and is daily becoming more evident, that there is no occasion to more than refer to it here. Why not take a leaf out of Germany's book? Last year (1897) five deaths occurred in the German Empire from smallpox in a population of 53,000,000, and this was due to the simple but efficacious precaution of compulsory vaccination and revaccination. The adoption of this measure will only be brought about when it is made perfectly clear to the people that *quarantine is not to be relied on as a protection against smallpox.* Dr. Thomas Borthwick, M.D. Edin., M.O.H. for Kensington and Norwood, read an excellent paper in 1897 before the South Australian branch of the British Medical Association on this subject, and his conclusions were:—

1. That the quarantine system is defective in many respects.
2. That the leakage is liable to occur under the system from sources which are unavoidable.
3. That leakage has actually occurred.
4. That the reason why the disease has not obtained a stronger hold inland has been owing to the application of harsh methods of local quarantine, and the expenditure of large sums of money.
5. That all this procedure does not guarantee complete protection.
6. That the imposition of effective quarantine would dislocate commerce to such an extent, and cause such general discontent, as to render it absolutely impracticable.

Dr. Collingridge, medical officer of the port of London, in his Milroy Lectures on Quarantine, says:—

1. The system is based upon a theory that is no longer tenable.
2. That it has never been found possible to maintain it absolutely, and that consequently a country has been subjected to a barbarous, unnecessary, and costly system without even the satisfaction of knowing that the experiment was complete.
3. That where carried out as strictly as possible it has not had the effect of arresting the importation of disease.
4. That sanitation and a radical system of medical inspection have proved more efficacious and at the same time less burdensome.

The whole history of quarantine is a series of illustrations of its futility, and proves it to be, as Sir John Simon so graphically described it, "an elaborate system of leakiness."

An effective quarantine is practically impossible in a commercial country.

Dr. Browning, after extended travel and experience of quarantine, says that "the pecuniary loss inflicted by the system may be estimated by millions of pounds sterling in delays, demurrage, loss of interest, deterioration of merchandise, increased expenses, fluctuations of markets, and other calculable elements; but the sacrifice of happiness, the weariness, the wasted time, the annoyance, the sufferings inflicted by quarantine legislation—these admit of no calculation, they exceed all measure."

There is no occasion to say more; let us profit by the experience of other nations; it is cheaper than paying for it ourselves, which we shall assuredly do, and the price in our unvaccinated community will be a terrible one.

We are shortly to become a nation; let us show that we fully appreciate that advantage. From our close proximity to the East, a magnificent field of commerce is offered to us; we have exceptional natural advantages; but if at the commencement of our nationhood we put the disabilities of quarantine in the way of our commerce, the markets which are at our doors will be exploited by other nations. It only remains for us to perfect our sanitary measures, waterworks, and disposal of town refuse, and adopt *compulsory vaccination*; then abolish quarantine and substitute medical inspection, with the application of our Public Health Act. Then, free from all the disabilities which quarantine imposes, we shall stand before the world as an example of an enlightened nation, and ready without restrictions to enter into commercial undertakings, confident, in the strength and perfection of our sanitary systems, pure water, and disposal of town refuse, of being able to do so without risk—the proud offspring of a grand old mother.

A PLEA FOR LEGISLATION TO EMPOWER MEDICAL PRACTITIONERS TO MAKE *POST-MORTEM* EXAMINATIONS IN CASES OF DOUBTFUL DIAGNOSIS.

By R. D. Pinnock, M.D., Ch.M. Glasgow,
Senior Surgeon, Ballarat Hospital.

There is little doubt in my mind that all members of this Congress who habitually peruse the medical journals must have been struck with the large proportion of interesting cases recorded that, notwithstanding the great care and deep thought exercised by one or many well-trained and-experienced professional men, have eventually succumbed, leaving the diagnoses admittedly imperfect. You will also have noticed that in very many of these records the writer will have closed his narrative with an expression of regret that, owing to the objections of the relatives, no *post-mortem* was permitted. I feel sure that you all, in common with myself, must on many occasions have experienced keen disappointment in being thus debarred from that more complete knowledge of our cases of doubtful diagnosis and unusual scientific interest which a *post-mortem* examination can alone afford.

Even in our hospitals we often find the relatives actually on the watch to take away at the earliest available opportunity the body of a deceased person in whom they have shown very little interest during life, and have visited seldom during his or her last illness.

The State, under the plea of benefiting humanity, has never hesitated in enacting laws interfering with the personal opinions of its subjects—*e.g.*, locking up a drunken man; compelling employers to pay certain fixed wages; insisting on citizens keeping their premises in a sanitary condition; compelling busy persons to drive at a walking pace over crossings, &c., &c.—and yet permits one or two individuals to say that no investigation of a cause of death shall be made in the interests of those living and suffering from similar diseases. And here the question may be asked whether the living have any real moral or legal right in the bodies of their deceased relatives. If so, why are they not permitted to keep them preserved in spirits in their own homes instead of being compelled to bury them in a legally authorised burial ground?

The sentimental aspect which is so often advanced of dislike on the part of relatives to mutilation of the dead is absurd, when every day we find these very objectors gladly assenting to infinitely greater mutilation on the living in order to save life or limb. And what mutilation ever caused by a *post-mortem* examination can be so abhorrent to a thinking being as that caused by the slow processes of decay in the grave.

Those who argue from the sentimental standpoint—and they appear to be the very large majority—are, in my humble opinion, guilty of what a great writer describes as either "angelic hair-splitting" or "demoniac inaccuracy."

The objection to the publicity and the ignorant and often offensive gossip contingent on *post-mortem* inquiries at inquests, which the law enforces when the lay officers think fit, can be understood by all of us. But in an investigation for purely scientific reasons and in the interests of future sufferers from disease, carried out privately, and with the safeguards to be presently suggested, there should be no objection on the part of any reasonable person who has any consideration for or interest in humanity at large

Unfortunately, there is too much maudlin sentiment abroad on matters of this kind, and too little sympathy for the human race amongst whom our lot is cast. The safeguards I would suggest, in order to prevent unnecessary or vexatious requisitions by the medical attendant for *post-mortem* investigations, are :—Firstly, that no charge be made by the medical practitioner for the examination ; secondly, that there should be a medical man of acknowledged ability and high personal character, nominated by the State for every district of a suitable size, who should be authorised to decide whether the grounds advanced by the medical attendant for a *post-mortem* were reasonable. Personally, I believe that such safeguards would not be required, except to allay the nervous apprehensions of the ignorant. For it is not at all likely that any practitioner would desire the tedious and unpalatable work involved in a careful *post-mortem* examination without fee or reward, unless he were convinced of its necessity on scientific grounds and for the benefit of humanity at large.

In conclusion, I would suggest that, if the members of this Congress should concur in the views put forth in this paper, they should adopt a resolution recommending that steps be taken to ensure the necessary legislation being enacted.

SEWERAGE AND HEALTH.

By Theo. Mailler Kendall, B.A., L.R.C.P & S.E.,
Medical Adviser to the Board of Water Supply and Sewerage, Sydney.

Man was originally a nomad, and dwelt in such shelters as were provided by Nature. As man increased in numbers, he outgrew the capacity of these shelters, and, always ready to adapt his surroundings to himself, constructed for himself a protection for his body against the various elements. In this way houses were evolved and towns originated. In constructing these protections man also constructed conditions of disease, and as the villages and towns extended themselves, so waste products accumulated in and around dwellings, and it became necessary to devise some means by which these products might be safely and quickly removed. The failure to remove these products was followed by disastrous consequences, for putrefaction rapidly began, and the air, formerly pure, became charged with pathogenic germs, and this air-sewage passing quickly from place to place sowed the seeds of disease. Certain towns through their suitability for trade attracted large populations and became the centres of commerce. The measure of the prosperity of any country is its amount of lucrative commercial intercourse with other countries, and the maintenance of a high standard of public health is intimately connected with the maintenance and increase of trade. It is, therefore, highly necessary that great care should be taken to maintain a high standard of health among those members of the community, who, by their industry, contribute to the maintenance and advancement of commercial enterprise. Sir James Paget considers that national wealth and national health are synonymous terms, and that "the necessary high standard of health can only be maintained through individual effort and by individual appreciation of hygienic law." In the same manner as man only slowly advanced from the depths of barbarism to the heights of civilisation and culture, so he only slowly learned to appreciate the danger which accrued through indifference and neglect, and it was only through the experience, bitterly gained by the appearance and reappearance of diseases, that man at last began to appreciate hygienic law. Exasperated Nature exacted the full penalty for the neglect shown of her laws, and devastating epidemics of disease appeared from time to time, and almost obliterated nations and changed the face of history. Fortunately for man, Nature herself provides her own self-cleansing powers, or we would more often suffer from our negligent uncleanliness; and it may be that, through disastrous epidemics of disease, Nature, our great schoolmistress, has only been teaching us, little by little, the salutary lessons of the jurisprudence of self-preservation. There can be no doubt that "the hygienic arrangements of large cities are the products of expediency," but I join issue with Dr. Vivian Poore when he goes on to state "that they are devoid of principle, and that they are not infrequently carried out in defiance of the teachings of science." May be that sometimes the method of application is wrong, but the principle is there all the same; and as we know that it has been left to the industry and experience of each succeeding age to perfect those arts and sciences which owe their birth to some profound intellect or to some chance circumstance, so it can only be through the experience and industry of years that the method of applying sanitary laws can ever become perfect. Dr. Poore admits that "hygienic measures in cities have been, for the most part, hastily adopted in order to escape from the dangers which are inseparable from an undue concentration of population," and yet he insists that overcrowding has been encouraged by sanitary improvements. Undue concentration of the population is the result of the exigencies due to trade, and sanitation is the outcome of this undue concentration. The question of water supply and the maintenance of its purity is of the greatest importance to any community, and paramount is the value of the maintenance of health, as growing out of those sanitary results, and the inducement to cleanliness of person and habitation, which have arisen through an abundant supply of pure water constantly delivered to the householder. The first essential of a water supply is purity of its source, and to this end it should be drawn, if possible, from a district beyond the haunts of man, and unknown to domestic cattle or animals. In all early settlements water was usually obtained from neighbouring rivers, creeks, or holes; and when the settlements increased in extent, these sources of supply were supplemented by means of wells. All these sources of supply yielded very pure water, but, as man is a very dirty animal and supremely indifferent to the comfort and safety of his fellow-creatures, these sources of water supply gradually became polluted, either directly through organic refuse being cast into the source, or indirectly through the pollution of the soil and the subsequent contamination of the subsoil water. To prevent this pollution it is necessary that all waste products should be got rid of in a safe, sure, and speedy manner. Dr. Farr says that "the sewerage system is a necessary complement of the water supply," and without doubt water is the most effectual and economical agent in the removal of waste matters, house slops, &c., from our habitations. If putrescible matters are allowed to collect in and around dwellings, putrefaction takes place, and although the number of putrescible substances is not great, still they are universally distributed and are derived from every organised being. When any organic substance dies it is quickly invaded by hordes of anærobic microbes which attack it with dangerous activity and cause dangerous smells. Putrescible matters, therefore, become a source of danger to life if putrefaction is allowed to take place, and any method which gets rid of such a danger continually in the most expedient manner cannot be devoid of principle. In country towns it may be quite possible to consign all organic matter to the soil as its proper destiny, but unless great care is exercised there is danger that by the "immediate burial of organic refuse below the surface of the soil" the subsoil water may be contaminated. It is easy to promulgate Utopian ideas, but human indifference and carelessness must not be lost sight of, and it must be remembered that on the purity of the earth depends the purity of the water supply, and that through constant fouling of the land the subsoil water may teem with pathogenic bacteria which are specifically concerned in disease and held to be a part of its ætiology. "To live in filth," says Vivian Poore, "and to inhale the products of putrefaction given off from the pails of fæces, of privies, or sodden ground, results in disease, but there is no evidence that such disease thus generated is able to spread through the soil and to infect a neighbouring house or street." In the face of recent investigations such a theory cannot be upheld, for the disease arising from such fouling will not long remain limited in the area of its operations, and Dr. Poore himself admits that to "bury excrement deeply in the soil is to court failure and to endanger a water supply." Dr. Poore complains that "the mixing of putrescible matters with water is a fundamental error, as it encourages putrefaction and hinders nitrification." It must be admitted that the water-carriage system of sewerage, as at present carried out, does hinder nitrification, but it cannot be admitted that it encourages putrefaction. If by concentrating excrement it is possible to prevent the propagation of pathogenic bacilli, all is well; but if it is impossible to guarantee that there will be no drainage from these buried masses of filth, then the subsoil may be polluted and the excremental drainage may pass into the subsoil water, which may thus become of the proper dilution to encourage the development of pathogenic bacilli. Nature has the capacity to deal with waste matter altogether unaided, but Nature requires that sufficient room shall be allowed for her operations. This question of the pollution of the subsoil water is most important, for the dissolution and distribution of foul matter through the soil, in the form of dilute sewage, would be favourable to the development of disease germs, while the removal of sewage by drainage would cause an impure subsoil water to be replaced by a less impure liquid. Constant fouling of the land is likely to cause fouling of the subsoil water, and the subsoil drainage of farm lands is concerned in maintaining a condition of nitrites and nitrates in a water supply so as to render it prejudicial to health. Agriculture may be the key to sanitation, but epidemics of typhoid fever at Glasgow and Nice were traced to the contamination of the water supply through the washings from some adjacent cultivated land. Dr. Robertson has conducted a very thorough series of experiments, from which he has proved that surface soils "teem with micro-organisms, which have a tendency to upward diffusion," and that, "if such organisms are of a pathogenic nature, they may, after passing into the atmosphere, disseminate disease and death." Through these experiments he has been able to demonstrate how very easily the typhoid bacillus may grow in certain soils, can persist in soils throughout the winter months, and that when the soil is artificially fed, by a leaking drain or by the access of filthy water from the surface, the micro-organisms will then take on fresh growth in the warm season. He has also proved that the *bacillus typhosus* can grow not only in soils, but also on the surface of moist stones. Sir Richard Thorne Thorne has emphasised this important point, and the experiments of Dr. Sidney Martin lead to the conclusion that, "with a due sense of the restrictions imposed by the artificial character and limited number of the experiments thus far reported on, we have reason for inferring that, whilst one soil and set of circumstances are highly favourable to the vitality and propagation of the typhoid bacillus, another soil and set of circumstances are rapidly destructive to it, and we seem to be upon the border of a territory where some vein of actual knowledge is likely to be discovered which will materially assist not only in solving the problem of the different behaviour of enteric fever as regards its persistence and recurrence in one and another dwelling, town or district, but of further emphasising the value of that practice which has been adopted in England for the prevention of typhoid fever." Sir Joseph Fayrer remarks on this subject: "It is not enough that we know the seed, but it is necessary that we should also know the nature of the soil, the meteorological and other conditions which determine whether it is to grow and multiply or to remain inert or harmless. If one can learn how to destroy the seed or sterilise the soil in which it attains its full development, or if we can neutralise the favouring conditions and so prevent or impede its growth, then we shall have solved a great problem, and conferred a lasting benefit on mankind." Dr. Poore admits that the surface soil is very rich in saprophytic bacteria which serve to transform inert organic matter into organic substances, and he even admits the possibility of the presence of pathogenic bacteria which may give origin to disease. Therefore, although Nature, through the soil, can change organic matter, still the processes of such change are not altogether free from danger, and, in the light of Robertson's experiments, the consigning of organic refuse to "its proper destiny" beneath the soil may, through the tendency of the pathogenic bacteria to upward diffusion, become a menace to public health. The soil must be kept pure, must be sterilised as far as possible, and be kept free from fouling by house refuse and domestic filth. The truly scientific system of dealing with disease surpasses all others in efficacy and efficiency, but we must not become too scientific, and although it seems a pity to upset deep-rooted theories and pet notions of sanitation, still it is to be remembered, as Oliver Wendell Holmes has stated, that "science is only an excellent piece of furniture for a man's upper chamber provided he has common sense on the ground floor." The science of public hygiene is not devoid of principle—it is not based upon fads and fancies: but it is founded upon common sense and scientific research, and it is "the logical outcome of the work of many great minds applied to problems under conditions very multitudinous and constantly changing." Measures for hygienic improvement, which in the past were looked on as mere hypotheses, are now, notwithstanding the obstacles to research arising out of ill-considered sentimental opposition, regarded of vital importance, and "they enlist," says Dr. Wilson, "the services of the people themselves in continuous efforts at self-improvement, and the general well-being of the people must mainly depend on their own exertions and self-restraint." The principle of any subject ought not to be confounded with its practice or mode of application, for everything must have a beginning, and it is only experience that perfects all things; for we may rest assured that, as the neglect of sanitary law is such a fault as neither God nor man can well forgive, we will be taught, even though the teaching be bitter, how to apply any principle in a proper manner. "We live," says Sir Joseph Fayrer, "in an age of progress and discovery. Intellectual activity has never been greater, scientific research never more profound or far-reaching. Half-a-century ago the great mass of the population lived and died under conditions which violated all the now well-known principles upon which health depends, and what were once regarded as mere theories of no practical value are now accepted as of cardinal importance, and statesmen have learned to realise that sanitary science comes well within the sphere of practical politics."

THE WATER CLOSET.

Man in his primitive state disposed of the liquid refuse from his dwelling by turning it on to the soil surrounding the house he lived in. Such a proceeding was fraught with danger, as the soil became polluted through large quantities of solid and liquid excreta finding their way into the soil and subsoil water. The conveyance of fæcal matter from latrines is a practice of considerable antiquity, and we read that some latrines, in the time of Augustus, discharged their contents into sewers, and had an ample water service by which their contents were flushed into the sewers. Fosbrooke relates that there existed in the palace of the Cæsars a water closet furnished with a cistern from which water was distributed to several seats, and there is still to be seen a specimen of a water closet removed from the ruins of Pompeii. Unfortunately, Rome, with its Cloaca Maxima, the monument of the water-carriage system of sewerage, fell, and civilisation was put back centuries, so that for

1,000 years few persons understood the paramount value of a bath. The history of the Canterbury Cathedral reveals that many years ago there existed within it privies, or fourth dormitories, opening into a fosse which was flushed with rain water falling from the roof of the building—and Aubrey, in his "Surrey," mentions "a pretty machine for cleansing away filth from a privy." Notwithstanding all these movements in the right direction, it was not so many years ago that each house had its hole in the ground for storing fæcal filth, and cities were cursed through this midden system. In 1778 Joseph Bramah re-invented the water closet, and it gradually crept into use till the year 1813, when it was further perfected, and came into general use 1828-33. The principle of the water closet was a good one; and although, perhaps, at first its method of working was, through want of experience, not only imperfect but even a source of danger, still defective as it was, and often still is, it was superior from a sanitary point of view to the privy. Through the water-closet system of drainage, the direct pollution of the soil by excreta has been considerably lessened, and succeeding years have brought about those improvements which have made the water closet not only a convenience but also a safeguard of health. However pure the air surrounding a house may be, however dry the site, and however wholesome the water, that house cannot be healthy unless a system is provided to convey from its precincts all the liquid refuse before decomposition has set up. The advent or rather the re-appearance of the water closet caused a reaction against the midden system which it gradually supplanted, and it brought about a great improvement in sewerage systems, which before its advent were in a very primitive state. As the decomposition of sewage takes place directly it passes into the drain, it is necessary so to lay drains and sewers that they may have a self-cleansing velocity. This was first pointed out by Chadwick, who suggested "that town refuse ought to be removed by the scour of running water into sewers." The gradual improvement of the water closet is an example that progress is not effected by indifference and want of appreciation, but that science, "if it is really to do for a people all that it is its duty to do, must be a living thing, a vital principle in the hands of men, and must not be represented by a dead routine." As far as it is possible to ascertain through historical records, the water closet of the present century did not originate typhoid fever, for this disease was known in the year 1412, was described in 1612, and was common in Dumfries in 1734. The sweeping condemnation of Dr. Vivian Poore that "the present system of sanitation is devoid of principle, is fraught with danger, and causes that undue concentration of the population, which brings about a deterioration in physical health," is met by the experienced rejoinder of Sir Richard Thorne Thorne, who remarks: "It is manifest that an enormous saving of life has resulted through the establishment of sewers and public water supplies. Before the institution of such public works, typhoid fever was mainly endemic where it is now occasionally epidemic, and, although the epidemics are sometimes certainly startling, the total amount of typhoid fever is trivial compared with what it used to be. Towns with a systematic system of sewerage have, as a rule, a lower death rate than towns not sewered at all, and drainage works beneficially influence public health by preventing the systematic pollution of the air under or in the vicinity of our houses through decaying waste matters." If it be true, then, that by proper prophylaxis certain zymotic diseases may be made to disappear, why are we so slow to adopt the remedies which science and history have pointed out? Are we in doubt of the correctness of our conclusions, or are we indifferent to the sacrifice of human life? If by some well-contrived form of murder a few people disappeared, the outcry would be great if police failed to discover the perpetrator of the deed. Thousands are, however, allowed to perish annually through preventable disease, because the cost of saving life is counted too great; and, although those interested in sanitation are perpetually pointing out the measures necessary, more money, more faith, and more energy are needed to deal with this measure satisfactorily.

It is not because there is any doubt as to the correctness of our conclusions, but because, through apathy and supineness, we fail to appreciate that the cost of saving life would be infinitely less than the wealth lost through its sacrifice. The present system of sanitation, therefore, is not devoid of principle, seeing that it has brought about a saving of life, and it is not the cause of undue concentration of the population. In fact, undue concentration of population has resulted through facilities for trade, and sanitation has followed upon the evils arising, and is the jurisprudence of self-preservation. The principle has always been present, and it is only through experience that the mode of application has become more perfect, and as we have mounted to our present state of perfection, and climbed the steps that others have cut, so on the more perfect cutting of this age others will climb to bring sewerage systems to the highest pinnacle of perfection. In dealing, therefore, with the undue concentration of the population, the scope and aim of sanitary science should not be limited to the consideration of zymotic diseases, "but should extend to the results of abnormal social conditions arising out of the strain and struggle for existence, involving over-competition in the various occupations by which life is supported or wealth or distinction acquired, and under the pressure of which so many lose their health or even succumb."

SAVING OF LIFE THROUGH SANITARY WORKS.

The increase of a population is an indication of increasing prosperity of a country, and the influence exerted by sanitary science on vital statistics is observed in the death rate. It is a well-known fact that the decrease in the mortality of any city has been very great after the establishment of a proper water supply, but the after-installation of a proper system of sewerage has brought about a still greater decrease; and the experience of all cities which have engaged in such works of a high state of civilisation has been that, as these systems have been extended, so the state of public health has been raised to a higher standard. In considering such results due consideration must be given to climate, for a hot climate is likely to bring about putrefaction more quickly than a cold one. Many years ago the city of London was much affected with ague, but since the sewerage has been perfected and extended this disease has almost totally disappeared. During eight years after the extension of the improvements of the sewerage system, in London the saving of life amounted to 15·76 per cent.; in Brighton, 16·57 per cent.; in Portsmouth, 20·62 per cent.; Bristol, 12·5 per cent.; Cardiff, 29·1 per cent.; Birmingham, 14·02 per cent.; Nottingham, 34·19 per cent.; Liverpool, 11·51 per cent.; Manchester, 23·1 per cent.; Oldham, 24.49 per cent.; Bradford, 18·40 per cent.; Leeds, 23·14 per cent.; Newcastle-on-Tyne, 26·84 per cent. During the period extending from the year 1888 to the year 1898, the death rate in the metropolis of Sydney, New South Wales, decreased from 189·2 per 10,000 of the population to 151·8, and, had it not been for the severe epidemic of measles which occurred during the year 1898, the death rate would probably have been as low as 128·9 per 10,000 of the population. This great decrease in the mortality of the metropolis of Sydney has occurred since the installation of and during the extension of the sewerage system; and although other sanitary reforms have been effected during this period of time, still there is no doubt that the sewerage system is the prime factor in furthering this decrease, in that it has assisted the other reforms so that they have been enabled to be of great benefit. In the year 1888 the death rate at one year of age was 66·07 per 10,000 of the population; in the year 1897 it fell to 38·0, and rose again slightly in the year 1898 to 46·1. In the year 1888 the death rate at five years of age was 121·9 per 10,000 of the population, while in 1897 it was 48·2, and in 1898 64·2. Surely such a decrease must be considered a triumph for our sanitary systems, especially when we remember how widespread was that uncontrollable epidemic of measles in the year 1898. With such a success, where can be the lack of principle and the immorality of our sanitary measures? If by immorality Dr. Vivian Poore means that the total excremental matter is not spread upon the soil, then the immorality is justified, since through it life has been saved. Evidence is abundantly forthcoming of the benefits of sanitation in the improved death rate and the material diminution of disease. The usual criteria adopted as evidence of the health of any community are—the general death rate, the zymotic death rate, and the infant mortality. The gross death rate is a fairly simple and in the main an accurate measure of the prevalence of disease. The zymotic death rate is not so safe a standard. The gross death rate in the city of Sydney has during the last eight years materially decreased; the zymotic death rate has also decreased during the progress of sanitary measures, although at times it has assumed alarming proportions. It is to be specially noticed that the diminution of mortality has been especially marked in those districts which have been provided with a proper system of sewerage. Although so much has been achieved, more remains to be done; for the effect of insanitary conditions is seen in the miserable physique of the masses, their dirt and immorality, and the appalling sacrifice of infant life. "Now the aim of house sanitation is to lower the death rate and to promote the health and well-being of the house occupants. Everyone has a direct interest in lowering his own death rate and that of those dependent on him, and in living his life under such conditions as are most conducive to health, happiness, and capacity for work." It is, therefore, to be remembered that when a proper sewerage system is provided it is incumbent upon the owner to set his house in order and to connect it properly with the main sewer. In estimating the value of a sewerage system it is not only necessary to take into consideration the decrease in mortality and the saving of life, but also to take account of the sickness rate. If this rate is calculated according to the latest methods and reduced to a money value it will be found approximately that in the city of Sydney about £20,000 have been saved during the last eight years, and consequently it is fair to assume that if all the houses were fitted with perfect sanitary systems a much greater saving would result. Alas. owing to human indifference and ill-judged economy, full acceptance of the benefit of the sewerage system has not been taken, and property-owners can only gradually be made to appreciate the value of sanitary works in raising the standard of health and in preventing sickness. It is very necessary to distinguish between a house drain and a sewer, and it must be remembered that, although a sewerage system may be perfect, a connecting house drain may through individual neglect become very foul and a source of disease. No sanitary system is automatic, and consequently house drains need attention, so that they may always be clean. In order to prevent the ingress of foul air from drains, &c., into houses, water traps have been invented, but these traps need constant attention, or they may become a source of disease rather than of safety. Neglect of a house drain is a sanitary fault, and will surely beget disease.

SEWAGE DISPOSAL.

The disposal of sewage has always been a matter of difficulty. and the modes of procedure are various. When sewage is carried away from a dwelling it must be carried to some point where it will do the least harm. Now sewage is liable to vary according to locality, season, weather, and hour of day on account of its highly complex nature and the unstable character of its component parts. It contains a large amount of liquid or soluble matters which are liable to rapid chemical change, and give off volatile products—gases or vapours—in abundance, which are highly offensive to the sense of smell. At all times when the sea has been sufficiently near, it has been the practice to carry the sewage thither, so that it will be dispersed and broken up by the action of the waves. As the sea, however, is not always handy, rivers and harbours have been used as the dumping grounds for sewage to the detriment of both, for the purity of the water was fouled, and both became sources of disease. In more circumscribed towns sewage farms have been planned where the sewage is filtered through the earth before the resulting effluent is allowed to flow into the nearest watercourse. When sewage is spread upon the ground, it cakes the ground and prevents the air getting at the roots of the plants, and so retards the ripening of the fruit and grain. In the primitive condition of mankind, the population was scanty and nowhere congested, and so long as the population remained scanty there was little chance of an epidemic. In the present age, however, morbific ferments may be at any time introduced into the body, and there generate poisons of a most intense and deadly action. However porous the soil, a quantity of evil-smelling liquid rises up into the air, carrying with it such disease germs as may be present, for, however rapidly the sewage may sink into the earth, a certain portion must escape by evaporation and rise into the air before it has had an opportunity to be disinfected. The plan of treating sewage on land depends for its success on the fostering of and the increase of micro-organisms. The use of crude sewage upon the land, however, has always been attended with difficulty, and instead of assisting Nature an attempt was made to control her actions by using precipitants to destroy the more solid matters by chemical action. This method of precipitation was accompanied by the formation of the inevitable sludge, which became dangerous to health through its tendency to putrefy, and although the resulting effluent was partially though not completely sterilised, and sufficiently inoffensive to be discharged into the nearest watercourse, still the success was only partial, for, when the effluent reached the larger body of water, dilution caused the antiseptic properties thus conferred to vanish, and then putrefaction commenced and the water again became dangerous to life. New light, however, has now been shed upon this vexed question, and it is becoming more and more clear every day that, now we are upon the right track to fairly and honestly follow those methods displayed to us by Dame Nature, she is rewarding us with an insight into the possibilities of her system far exceeding the most sanguine anticipations of the past. At a future period the sea-shore will run no risk of pollution, and, as the science of biology has revealed that the natural method is the proper one for dealing with sewage, the death-knell of the costly sewage farm has been rung. The satisfactory types of sewage treatment involve two processes: First, the disintegration and disposal of solid matters; second, the conversion of soluble residua into harmless constituents in the effluent. The organic matter of sewage is essentially manurial, as it includes all the nitrogen eliminated from the bodies of man and other animals, as well as phosphoric acid and potash. As the supply of phosphoric acid and combined nitrogen is not unlimited, and as these are the scarcest items of plant food, it becomes a serious matter to countenance wholesale destruction of these essentials of life. Dr. Vivian Poore has complained that the water-carriage system of sewage hinders nitrification and thus cheats agriculture of its dues. No doubt he had some reason for this complaint, as we know that every grain of nitrate that can be rescued from sewage is a clear gain to the community; but unfortunately sewage had to be removed speedily and economically, and it is only now that we learn of methods by which the nitrate can be saved without any danger to the people. Notwithstanding, therefore, the admitted deodorising and disinfecting powers of the earth, still it does not immediately absorb and destroy offensive matters of sewage. The precipitation method of treating sewage having become a failure, in the year 1892 Scott Moncrieff proposed the utilisation of bacteria in the treatment of sewage. In sewage there exist certain germs which are harmful and ought to be deracinated, and certain other germs which are useful to plant life. If, then, some discriminating means can be devised which will destroy the evil germs and leave the useful ones untouched, no delay should be practised before making use, to the full, of such means as are provided in the natural method of treating sewage, and which may yet be so improved as to render sewage potable. In Scott Moncrieff's method no antiseptics were needed, and as the outcome of it Cameron formulated his septic tank. The septic tank is merely a large cesspool from which light and air are rapidly excluded, and in which anaërobic organisms flourish and attack the sewage, disintegrating the solid matters with the evolution of much gas, and forming a fine mineral deposit. From the septic tank the sewage flows into aërating troughs, where, under the influence of light and air, aërobic bacilli begin to work; thence the sewage passes on to the coke filter beds whence it escapes in a highly purified condition. Dibdin and Thudichum ascertained by means of experiments that the septic tank was not needed, and that the whole of the necessary action of bacteria could take place in the coke filter beds. They were of opinion that "the whole purifying action was due to the encouragement given to the genesis and living of the micro-organisms," and they allowed the sewage to flow on to a bacterial tank, composed of stone ballast, and thence to the coke breeze filters. These experiments proved the value of the purifying action of bacteria during the passage of thin films of fluid over surfaces exposed to the presence of air, and they showed that it was only a question of regulating the amount of food supplied to the micro-organisms along with a free supply of oxygen. Mr. Garfield, of Wolverhampton, exchanged coke for coal as the constituent of the filter bed, and he found that "coal appears to have the power specially of removing putrescent organic matter from sewage, and that the effluent produced is as good as or even better than that which may be obtained from land. It is superior to coke or ballast, in that it has at once the chemical action, and its purifying power is marked from the first day it is used. A still better result is obtained after a period of two or three months' use." In my own laboratory I allow the crude sewage to run into a tank filled with stone ballast broken to various sizes, and from this tank the effluent passes down a series of steps to the coal filter. During the passage of the sewage over the steps it becomes thoroughly aërated. and after it passes into the coal filter it is allowed to lie there for two hours. I have found that the effluent coming from my coal filter is devoid of smell, shows little or no reaction to Nessler's test for ammonia, and is rich in the nitrates necessary for nourishing the soil. The difficulty which has always been present in dealing with sewage is the formation of sludge, which separates as a scum in the septic tank, and is, for the most part, left as a black mass on the surface of Dibdin's filters. The experiments of Drs. Kenwood and Butler show that this evil may be got rid of. These observers first filter the sewage through a filter bed, which is so arranged that the filtration is upward instead of downward, and they get rid of the sludge in the substance of this bed. After undergoing this process of upward filtration, the resulting effluent is allowed to flow on to the coke filter, where it is exposed to the action of bacterial agents. Drs. Kenwood and Butler believe that, "while upward filtration offers a better prospect of effecting the separation and solution of the suspended matters of sewage, it at the same time reduces the pollution of the effluent better than any system which aims at their removal by digestion in a hollow chamber, such as the septic tank." As soon, therefore, as these recent improved methods of sewage disposal are adopted, all charge of immorality against our systems of sanitation must be dismissed. Instead of constructing costly works to rid us of a nuisance, formerly considered useless, the natural method of dealing with sewage will allow us to return it to the land rich in all of its nutritive constituents, and deprived of all those which render it offensive and dangerous to life. Rightly does Dr. Sims Woodhead say that "we have received a bright inheritance, and that it rests with us to hand it on to our successors, not only untarnished, but with added lustre." The giant strides made in sanitation during this Victorian era will ever remain memorable in history, but still those of this generation can say, with the hunter in the "African farm": "At the clumsy work they will laugh; when the stones roll they will curse me, but they will mount, and on my stair! They will find her, and through me! And no man liveth to himself, and no man dieth to himself."

Having seen that sewerage has an effect in reducing the mortality and prolonging life, let us consider some of the diseases which are directly affected by a sewerage system.

DIARRHŒA.

The spread of choleraic diarrhœa is influenced very greatly by an insufficient water supply, for the state of health may be thus lowered in any community, and epiphytic affections may arise. When diarrhœa becomes endemic in any community, it is almost always owing to impure air, impure water, or bad food. When the air becomes impure and the water supply also becomes impure, it becomes necessary to seek out the cause of the fouling of these elements. Climate and season have always exercised a great power over different diseases, and often supply favourable conditions of propagation; for when the temperature is high and the atmosphere dry, the germs of disease flourish rapidly, because their vitality is promoted. On the other hand, excessive humidity of the atmosphere, during the persistence of high temperatures, favours putrefaction, and while a high temperature persists it matters little whether the atmosphere is excessively humid or excessively dry, for the danger of disease originating is about the same. According to Ballard, "the essential cause of diarrhœa resides ordinarily in the superficial layers of the soil, where it is intimately associated with the life processes of some micro-organism not yet isolated, detected, or captured." Tompkins, of Leicester, is of opinion also that the cause of diarrhœa is a soil-bred organism, and that organic pollution of the soil renders it distinctly favourable to a high diarrhœa mortality, especially through those made soils where liquid filth is allowed to pollute the earth beneath. When foul products are cast upon the soil or into pits, they putrefy there and give forth poisonous gases to the air, and form a breeding ground for those microscopic germs which we now know to be the causes of so much death and disease. A high atmospheric temperature conduces to a high diarrhœal mortality, but this influence, although very great, is only exerted in an indirect manner. Springthorpe, of Melbourne, finds that diarrhœa becomes epidemic when the mean temperature has reached an elevation of 60° F., but my own experiments place the necessary elevation—at any rate, in Sydney—at 65° Fahr.; and in this I am confirmed by other observers Ballard has found that when the earth temperature, at 4 feet from the surface, reaches 56° Fahr., no matter what the mean atmospheric temperature may be, there is a rise in the prevalence of diarrhœa. From my own observation at Erskineville, I have found that the summer rise of diarrhœa occurs when the temperature of the 4-feet earth thermometer reaches 60° Fahr. Excessive dryness or complete wetness of the soil both appear to be unfavourable to diarrhœa, but there is a degree of habitual dampness, which, while being marked, is not sufficient to preclude the free admission of air between the constituent physical elements of the soil, and is specially favourable to diarrhœa. Such a degree of dampness obtains when in the season of diarrhœa the subsoil water stands sufficiently near to maintain, by capillary attraction, the dampness brought about by previously greater nearness of the water to the surface. Klein has recently discovered that the *Bacillus enteriditis sporogenes* contained in horse-dung is one of the causes of diarrhœa; and, as the inhalation of finely pulverised horse-dung is not a useful application to the respiratory membranes, it is to be hoped that in the future efficient municipal control will cope with this factor of disease, and that the abominable and insanitary practice of sending out dust carts to take away house refuse at all times of the day will be discontinued. If, then, diarrhœa is essentially a disease of the soil, and is to be avoided, measures must be taken to preserve the soil itself free from germs of an evil nature, and also to improve it in other ways. It is in connection with the maintenance of the purity of the soil, and its habitual dampness as a factor of diarrhœa, that the laying of sewers plays so important a part. The laying of deep sewers assists in preserving the purity of the soil, and brings about the drying of the soil which affects the variation of the earth tempearature. Before the laying of sewers in the metropolis of Sydney, the rate of mortality from diarrhœa per 10,000 of the population was very high—

—	Under Five Years.	Five Years and under Ten Years.	All Ages.
Before laying of sewers...	7·5	1·9	10·9
Since ,, ,, ...	2·9	·5	4·9

but since the laying of sewers, as will be seen from the above table, it has greatly decreased. A comparison of the rate of mortality from diarrhœa in the city of Sydney shows that this rate has been gradually decreasing ever since the year 1888:—

1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.	1898.
10·9	7·1	7·3	8·3	7·4	6·6	7·0	6·1	4·8	4·9

In the sewered districts of the metropolis this decrease was very marked indeed; and although diarrhœa is not a disease which is directly controlled by efficient drainage, still there is every evidence that the disease has greatly decreased as the sewerage system has been extended—

RATE OF MORTALITY FROM DIARRHŒA PER 10,000 OF THE POPULATION.
SEWERED DISTRICTS.

—	Before Laying of Sewers.	Since Laying of Sewers.
City of Sydney	10·9	4·9
Darlington	6·6	2·6
Glebe	10·0	3·2
Newtown	5·2	3·0
Paddington	10·5	5·0
Redfern	15·0	10·0
Waterloo	27·0	19·8
Woollahra	9·1	4·9

DIPHTHERIA.

Diphtheria is a toxic disease, inasmuch as there is a general intoxication of the whole system due to poisonous products of the diphtheria bacillus. According to Newsholme, "we have in diphtheria a disease which creeps slowly from place to place, and months or several years may elapse before it takes root and begins actively to propagate itself." The reappearance of this disease at certain intervals in any particular district points to its specific organism having either a saprophytic tendency, or to its retaining its vitality in dust, thus rendering the site of previous cases more or less permanently infective. This disease favours damp soils, and in former years was strictly a rural disease, but owing to certain conditions and changes of environment it has extended its habitat. "The peculiarity of its distribution suggests," says Longstaff, "that the cause is not to be sought for in any of the higher developments of civilisation, such as sewers, but it is to be looked for rather in some condition associated with a primitive form of life." Although the bacillus of diphtheria has never been found in sewer air, the emanations of foul drains can induce an adynamic state of health which might bring about a physical change in the system, so as to increase its susceptibility either through the simple vulnerability of the respiratory mucous membranes by which the infection enters, or by lessening the refractoriness of the whole system to infection. It is very difficult to understand the seasonal fluctuations and the causes of the epidemicity of diphtheria. The extension of education during the past half-century has tended to increase this disease, since school attendance brings together those members of the community who are by reason of age most susceptible to the disease, into the exceptionally close relationship to each other for many hours of the day; and protracted close contact of children in schools, even if the ventilation is perfect, affords greater facilities for infection than would occur under home conditions or during the hours of play in the open air. Removal of faulty sanitary circumstances has not always been followed by a diminution of this disease, but by ventilation and good drainage all possible breeding grounds for the diphtheria bacillus may be removed. Sir Richard Thorne Thorne has pointed out that diphtheria has little connection with any sewage system. He says: "In such outbreaks of diphtheria as I have investigated, some obvious defect, leading to the pollution of respired air by sewer or drain emanations, was regarded as the cause of the disease. I have found in almost every instance that it was not only impossible to eliminate other and better sources of infection, but also that some other alternative sources were generally found to have had an obvious causal relation to the disease." The evidence that diphtheria is due to air-borne germs is very satisfactory; it is really a filth disease, communicated by aërial convection, and may invade the most carefully kept house through the progress of air sewage from an ill-kept dwelling. The relation of filth to disease is well emphasised by Pettenkofer, who likens filth to gunpowder and disease to the spark. It is well known that in a thoroughly soaked sewage-sodden soil, to which little air can have access, nitrification does not take place, and hordes of pathogenic micro-organisms may be produced and become disseminated through the soil by upward diffusion. Although, therefore, it is a popular belief that this disease is intimately connected with sewerage systems, such a connection is remote, although improved sanitation does assist in checking its ravages. It is most certainly connected with school attendance, as is shown by "the holiday drop," and it is intimately connected with filth. The filthy dustbin and the ill-managed municipal garbage tips are standing disgraces to any community, for it is marvellous that in this enlightened nineteenth century these collections of filth should still exist, sending forth currents of air sewage into healthy zones, and encouraging infection to be carried from person to person by aërial convection. Individual carelessness contributes to the spread of this disease, and through uncleanliness and ignorance many innocent persons who may truly appreciate the value of hygienic precautions may suffer infection and death. Diphtheria, therefore, is a disease which is somewhat beyond the scope of benefit through a sewerage system, but sewerage can help by drying the soil, lowering the height of subsoil water, preventing fouling of the soil, and by preventing the origination of those emanations from foul drains, &c., the outcome of a badly managed and badly installed system of sewerage. The mortality from diphtheria in the city of Sydney has been very distinctly decreasing ever since the year 1888, and the fall was marked even before the introduction of the antitoxin treatment. Dr. Herman Biggs says: "Antitoxin can never take the place of improved sanitary conditions in doing away with the breeding places of the diphtheria bacillus outside the human body"—

RATE OF MORTALITY FROM DIPHTHERIA PER 10,000 OF POPULATION.

CITY OF SYDNEY.

1889.	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.	1898.
5·2	4·62	4·62	2·9	3·1	3·1	2·7	2·07	1·3	1·6

TUBERCULOSIS.

Phthisis, or more properly tuberculosis, is now acknowledged to be an infectious disease of a micro-parasitic nature, intimately connected with dampness of soil. This disease is capable of being transmitted, either by aërial convection or by ingestion of food, through a specific virus or microbe which is a facultative saprophyte as well as a parasite, and is capable of growing in the organic matter contained in respired air or in the vapour arising from the soil. It may be, and undoubtedly is, produced by the inhalation of specifically infected dust which contains micro-organisms of a pathogenic nature. These micro-organisms are the spores derived from dried and desiccated sputum which has been recklessly thrown out by the filthy, pernicious, and unnecessary habit of expectorating in public thoroughfares and conveyances. Tuberculous persons are a source of danger to others, chiefly through the sputum; and M. Cornet has shown that a very large number of those places where phthisical persons live and congregate do contain material, in the form of dust, which is capable of communicating tuberculosis. There is little doubt but that Davies is right in stating that "aggregation into communities with the inevitable resulting overcrowding of habitations, and the establishment of more sedentary occupations, gave the necessary encouragement to an organism, able to live and multiply favourably at the body temperature and in the body tissue, to select an appropriate site for its output, and so, in course of time, firmly to establish its continuance as a parasite." Sanitary defects in houses are apt to cause a deterioration of the general health, and to predispose to infection; but infection itself, direct or indirect, is apt to be intensified and its conveyance to be much facilitated by overcrowding. The danger of infection from the sputum is magnified through the fact that the bacilli of tuberculosis retain their vitality, for a long time after they have left the body, in dust and dried sputum. The virulence of the microbe of tuberculosis is diminished by currents of air and sunlight, or, as Mason describes it, "swilling and flushing with fresh air"; but re-breathed air is a source of great danger, for stagnant air, charged with organic material, may maintain the virulence of the microbe for a long time after the bacillus has left the body. Buchanan has, by his experiments, conclusively proved "that phthisis is intimately connected with dampness of the soil"; and Bowditch, in America, remarks: "Residence near or on a damp soil, whether that dampness be inherent in the soil itself or caused by percolation from adjacent ponds, springs, rivers, meadows, or marshes, &c., is one of the principal causes of phthisis. As tuberculous disease in infants and young children is very often a disease with lesions of the cerebrospinal membranes, or a disease affecting the intestines and their related lymphatic glands, there is good ground for belief that the infection in childhood at least finds its entry in a very large proportion of cases by means of the alimentary canal. This extension of infection by ingestion of food points to an extension of bovine tuberculosis, which constitutes a serious economic loss and a new source of danger to man. Brown, of Carlisle, has pointed out that the tuberculous lesions in cattle have the same localisation as in the human subject; and other observers have laid much stress upon the infectivity of phthisis by means of milk. Ransome has proved that during the last thirty years there has been a steady diminution in the mortality from pulmonary phthisis in direct proportion to the improvement in general hygiene; and Sir Richard Thorne Thorne states: "During the last half-century sanitary improvements have reduced the death rate from phthisis—at any rate, from that form of phthisis which is due to aërial convection." Professor Nocard asserts that "it is the duty of every medical practitioner to preach that tuberculosis is a preventable disease arising, in the majority of cases, from a contagion that is easily guarded against"; and he points out "that only about one-sixth of the cases of human tuberculosis could be attributed to heredity." Sir Peter Eade says that "heredity in phthisis consists only in the tissues of some bodies being so constituted as to form an unusually suitable soil for the bacillus, and that, if the germs of the disease can be kept away, these specially structured individuals will have a much greater chance of survival, even if they be somewhat more delicately constituted than their neighbours." On this point also Sims Woodhead remarks: "The children of tuberculous parents can be placed under such conditions that they will never contract the disease. They do not inherit tuberculosis from their parents, though they may inherit those weaknesses of tissue and constitution which render them peculiarly liable to succumb to tho attacks of parasites that in their parents are doing such damage." Sir Richard Thorne Thorne points out that "that form of tuberculosis which is caused through infection by means of the alimentary canal has steadily increased the number of its victims, and this increase has gone hand in hand with the steady increase in the consumption of milk." The subject of milk is beyond the scope of this paper; therefore, suffice it to say that the danger from milk may be lessened if it is pasteurised in small quantities, but that at the present time there is no practicable and certain method for freeing milk on a large scale from germs without, at the same time, seriously prejudicing its flavour and nutritive value. "Phthisis," says Sir William Broadbent, "is no longer to be regarded as a visitation of God, for it is now well known to be the result of man's ignorance and carelessness" By the effects of subsoil drainage, improved hygiene, raising of the general standard of cleanliness, both of person and dwelling, and the greater attention now given to ventilation, the mortality from tuberculosis has been decreased." Buchanan attributes much of the diminution of the phthisis death rate to "that drying of the soil which has followed upon the laying of main sewers." Since tho institution of sanitary works we have witnessed an enormous decrease in the number of persons suffering from phthisis and a decided lengthening of the duration of the disease. Sir Joseph Fayrer says: "Sanitation has done good by helping to improve the ill-ventilated over-crowded dwelling, damp water-logged soils, and impure water. Through these improvements fewer persons die from phthisis and are slower to die when affected." Clearance of over-crowded building sites and the reconstruction of insanitary house property are now most important factors in dealing with this disease which annually claims its holocaust of victims, and through which, as Francis Fox states, "Great Britain loses annually 70,000 souls because people do not understand the value of fresh air." The value of sewers in reducing the death rate from phthisis is shown by the following table:—

MORTALITY FROM PHTHISIS.

—	—	Density of Population per Acre.	Rate per 10,000 of Population.	
			Before laying Sewers.	Since laying Sewers,
England	Salisbury ...	...	44	22
,, ...	Ely	...	31	16
,, ...	Rugby	...	43	10
,, ...	Banbury	...	26	16
,, ...	Macclesfield ..	...	51	35
Sydney, N.S.W.	City	46·1	13·6	9·2
,, ,,	Glebe	37·0	11·5	6 2
,, ,,	Darlington ...	90·9	8·0	2·6
,, ,,	Newtown ...	48·5	6·1	3·9
,, ,,	Redfern	55·9	12·1	5 2
,, ,,	Waterloo ...	11·7	15·9	6·4
,, ,,	Paddington ...	48·3	13·6	7·2
,, ,,				

TYPHOID FEVER.

The subject of typhoid fever is one of universal interest, and, after having engaged for some years the attention of sanitary experts, has resolved itself into one of national importance. This disease, owing to the seeming impotence of medicine to effect a remedy, has become a grave question to all deep-thinking politicians, as it has in the past been accompanied with disaster, and has continually made its re-appearance in the form of devastating epidemics. The cost of a virulent epidemic of typhoid fever is not lightly to be passed over, and, to properly understand the importance of this disease, it is necessary to reduce the results of its ravages to a money value, in order to show how great an inroad it has made into the national wealth. According to the various methods of calculation given by Broadbent and others, the seasons of typhoid fever for nine years before the year 1889 cost the city of Sydney an approximate sum of £500,000. Since the year 1889 to the year 1898, this sum has been reduced by one-half. Since the laying down of sewers and the improvement of the general hygiene, the mortality has decreased from 209 cases in the year 1889 to 73 cases in the year 1898; and the approximate cost to the State has been reduced from £55,980 in the year 1889 to £19,780 in the year 1898, making a clear gain of £36,200. In order to obtain the greatest amount of good, it is necessary that every individual should recognise his true position of responsibility; for it is only through ignorance and indifference that infectious material is scattered broadcast, diminishing the chances of recovery of persons suffering from disease, and imperilling the safety of others. The contagium vivum of typhoid fever has generally been admitted to leave the body by means of the stools; therefore, the prevention of infection is not the purification of sewage, but the destruction of the germs before they have escaped from the sick room. The infection of typhoid fever may be communicated directly from one individual to another, or by means of some vehicle such as food, water, or milk containing the microbe of disease. According to Dr. Haviland, "typhoid fever has no reason for existing at all," but Dr. John Drysdale remarks that "although the area of its operations may be circumscribed, still it can never be hoped that good food, drainage, or ventilation will extinguish it altogether as long as man is a social animal." Transportable miasmata are originally due to internal local conditions of soil and water; and although Mr. Ernest Hart laid down that "every death from typhoid fever is a violent death and a case of water-poisoning," Sir Charles Cameron is of opinion that "typhoid fever is a semi-miasmatic disease, and a disease of the soil which resembles in its propagation malarial disease." It is highly probable that the culpable fouling of the soil practised by our forefathers, and continued by ourselves, has more to do with the propagation of disease than any property in the virgin soil itself, and that one of the chief means of the propagation of typhoid fever is through a special organism becoming endemic in the soil in close proximity to the patient's abode, or through it finding its way into milk or water. Sir Richard Thorne Thorne has observed that "the steady diffusion of typhoid fever, on the appearance of a case in an insanitary area, shows that enteric fever, though mainly distributed in epidemic form by means of water or milk, is by no means always a water-borne disease; it raises anew the question as to how far recurring prevalences of enteric fever in one town or in one spot can be due to the persistence, in more or less active form, in certain soils of the organism of the disease." Dr. Sidney Martin has ascertained through various experiments that "the bacillus of typhoid fever behaves very differently when, in the absence of competing micro-organisms, it is implanted in organically polluted soil and in virgin soil. In soil of the first kind the bacillus grows rapidly, and spreads abroad, whereas in virgin soil, under like conditions of moisture and temperature, it languishes and quickly dies out." Mr. Adams points out the danger of infection through soil, and he classifies every soil through which foul surface deposits pass, into three layers: First, the danger zone, containing many putrefying organisms; second, the risky zone, containing many nitrifying organisms; and third, the safe zone. He also lays stress upon the fact that, after long droughts, the surface soil becomes fissured and cracked as it loses its cohesion, and thus foul surface deposits may be washed, by heavy rain, through into the neighbourhood of the water-zone without having undergone any purification. The soil in a midden town is particularly likely to cause infection, for it is impossible to ensure disinfection of a midden which has been once polluted, and, as these receptacles are seldom impervious, the surrounding soil soon becomes saturated with infective fæcal matter. Professor Delépine has found the bacillus of typhoid fever in the soil surrounding a midden. Professor Corfield is of opinion that defective sewers and drains are sufficient of themselves to cause an epidemic of typhoid fever; and Dr. Wilson also makes the statement that "any epidemic of typhoid fever in a sewered city points to imperfect ventilation, faulty construction of sewers and drains, a contaminated water supply, or to polluted milk." Since the bacillus of typhoid fever may germinate and multiply outside the human body, there is no apparent reason why it should not survive, under favourable conditions, for an indefinite period. Murchison adduced the fact that "single cases of typhoid fever do arise in the same house again and again at intervals of a year or longer"; and Sir Charles Cameron thinks that "this disease may cling to individual houses, and infect succeeding tenants." It is quite possible also that those currents of air sewage which arise from putrefying sources may pass, loaded with morbific matter, into a healthy zone, and so infect healthy persons. Sewage consists of the solid and liquid excreta of man and animals, and is a mixture of a most varying and complex character. Sewage is constantly changing its character and transforming gases, through that process of oxidation which is always taking place in it. If, then, this complex mixture is allowed to flow upon the soil in an indiscriminate manner, it naturally follows that there will be a pollution, and that, through this pollution, disease will arise. Past experience has taught us it is impossible to bury waste products always beneath the soil so that Nature may deal with them unaided. The supreme indifference of individuals renders this mode of procedure fatal, because due care is not exhibited in dealing with sewage in this manner, and thus it becomes necessary to devise some plan for the safe and speedy removal of sewage from the neighbourhood of dwellings. By thus providing a proper sewerage system we assist Nature in providing good local hygienic conditions, so that the resisting powers of the human organism will not be lowered, and that it will be able to oppose invading germs which come from other sources. All infectious diseases must be transmitted from the air, the soil, or from water. Recent investigation has shown that typhoid fever is not always a water-borne disease, and we have seen that infection may lie dormant in the soil, and cling to the houses for lengthy periods, so that the sources of infection are varied. Notwithstanding any assertion to the contrary, the infection of typhoid fever through the soil is not only probable but a known fact, and the contagion may be carried through some distance by the movement of the ground air, which is a well-ascertained fact. The primitive method of getting rid of excrement was to cast it anywhere upon the soil and to leave it there to be dealt with by Nature. This method was succeeded by the midden or pit, which we have seen to be dangerous because it was not always an impervious receptacle, and because of its frequent juxtaposition to the source of water supply, in the shape of wells. The water closet has, to a great extent, displaced the midden, especially in all those places where a sewerage system has been installed; but in those places which have no proper system of sewerage, in order to prevent the existence of the dangerous midden, it became necessary to adopt some plan for the removal of fæces, and thus the earth and pail closets came into existence. A strong objection to earth closets is the fact that bacilli may remain alive in the mould or ashes used for an almost indefinite period. The pan system is a clumsy device, which has, from time to time, retarded the natural order of development. Both these systems, however, notwithstanding their evident defects, are infinitely better than the midden, in that fouling of the soil can never take place to so great an extent. The incidence of typhoid fever in dwellings provided with pail closets and those provided with water closets is very marked. According to the Nottingham records, one in 101 houses with pail closets and one in 360 houses with water closets were attacked with typhoid fever. The Belfast records show that one in 108 houses with pail closets and one in 400 houses with water closets were attacked with typhoid fever. It is evident, therefore, that the water closet has not been the active agent in the propagation of typhoid fever; in fact, a well-conducted sewerage system, with its accompanying system of water closets, has greatly assisted in circumscribing the area of the action of this disease. Professor Ray Lankester gives it as his opinion that the *Bacillus coli communis* from the intestines of animals may become an adjuvant factor of typhoid fever if, by any chance, it finds entry into the intestines of man. He thinks that this bacillus is an innocent saprophyte, which may alter its character and become possessed of intense pathogenic activity if it is allowed to associate with the same bacillus of an equally saprophytic nature in the intestines of man. These opinions of Lankester have been in some measure strengthened by the investigations of Drs. Lorrain-Smith and Tenant; and Sanarelli states that "when the typhoid process is set up, this bacillus acquires virulence, and takes active part in the disease, through auto-intoxication." Sewers, unfortunately, contain noxious gases, the formation of which cannot be avoided, but there is no reason why such gases should be allowed to enter houses, where they would devitalise the air, and cause a deterioration of the general system. Ventilation of sewers is highly necessary, and was understood by the Romans, but, through the dark ages which followed on the downfall of the Roman Empire, sanitation was altogether forgotten, and it is only during recent years that sewer ventilation has been revived and brought to its present state of perfection. In ventilating a sewer, care must be taken lest the current of air within the sewer may drive the noxious gases formed therein back into the dwellings through the house drains, and to remedy this traps have been devised with a water seal. No system of sewerage, however, is automatic, and therefore these ventilators and traps need very careful attention to keep them in thorough working order. The impervious, well-ventilated underground sewer is, as far as we know at present, the best means of quickly carrying away waste matters by means of water carriage, and for preventing the fouling of the soil. When mentioning other diseases it has been seen how important a part the laying of sewers plays in drying the soil, and there is no doubt that this function of sewers assists also in the diminution of the mortality from typhoid fever—

MORTALITY FROM TYPHOID FEVER, CITY OF SYDNEY, PER 10,000 OF THE POPULATION.

—	Density of Population per Acre.	Rate per 10,000 of Population.	
		Before laying Sewers.	Since laying Sewers.
City of Sydney	46·1	5·07	1·3
Glebe	37·0	2·5	1·1
Darlington	90·9	4·0	0·4
Newtown	48·5	6·6	2·5
Redfern	55·9	3·6	1·0
Waterloo	11·7	1·2	0·9
Paddington	48·3	2·7	1·5
Woollahra		2·1	0·9

Because some diseases are marked in their characteristics, and frequently become fatal in a comparatively short time, their causes are zealously traced, and safeguards are devised Many persons, through ill-health, are deprived of full vitality and capacity for useful employment and enjoyment of life, which is the result, in the majority of cases, of the neglect of sanitary laws. Because, however, the immediate effects are not made apparent, the causes are more or less ignored, investigation is often imperfect, knowledge indefinite, and popular prejudice often at fault. Such things should not be, and it is only through persistent education of the public mind that this question of mere finance will be set at rest, and that the matured recommendations of sanitarians will be carried into effect.

Authors Quoted from—

Adams
Andrewes
Bacon
Ballard
Biggs
Boussingault
Sir Wm. Broadbent
Butler
Brown
Buchanan
Bowditch
Boobyer
Childs
Catlow
Cornet
Corfield
Sir Charles Cameron
Cameron
Drysdale
Dibdin
Sir Peter Eade
Sir Jos. Fayrer
Farr
Fox
Garfield
Bostock Hill
Alfred Hill
Wendell Holmes
Haviland
Hart
Klein
Kenwood
Lankester
Sidney Martin
Murphy
Murchison
Manson
Newsholme
Notter
Nocard
Sir James Paget
Vivian Poore
Richardson
Robertson
Ransome
Olive Schreiner
Steavenson
Lorrain Smith
Springthorpe
Sanarelli
Sir Richard Thorne Thorne
Tompkins
Tennant
Whitelegge
Wilson
Sims Woodhead.

THE SANATORIUM TREATMENT OF CONSUMPTION.

By George Lane Mullins, M.A., M.D., University of Dublin, Etc.,
Assistant Physician to St. Vincent's Hospital; Physician to the Hospice for the Dying, Sydney.

In a paper to be read before the Medical Congress it is impossible to go fully into the details of the advantages to be gained by the treatment of phthisis in special sanatoria. The time at my disposal will not admit of such a discussion, so I propose to assume that you are all agreed upon the benefits to be derived from such institutions, and, therefore, I will limit myself to a description of the various methods of treatment employed in well-known sanatoria for consumptives in the Northern Hemisphere.

The subject is a large one, and is becoming more important every day

The attention of the public has been aroused, and it becomes our duty as a profession to enlighten consumptives and their friends how to stay the ravages of the most deadly disease of modern times. Few of us but have seen the hectic flush come into the cheeks of our nearest and dearest ones, the gradual wasting of the strong athlete doomed to an early grave, and the breadwinner cut off in the prime of life by a disease which we now look upon as curable. The words of Sir Samuel Wilks (*Practitioner*, June, 1898): "The only remedies I know for consumption are air and sunshine." Air, air, fresh air, rings in our ears and finds a ready response in the movement to found consumptive homes in the Australian Colonies. Germany, Switzerland, France, Austria, Norway, Russia, the United States of America, and other countries have now their sanatoria; and recently such institutions have been established in England with good results. Why, then, should Australia, with all its natural advantages, delay in developing its plans for the rescue of the consumptive from slow and often early death?

Since Brehmer founded his celebrated sanatorium at Goerbersdorf, over forty years ago, our knowledge of the dread disease has extended considerably. The bacillus tuberculosis, then unknown, is now daily searched for in the sputa of thousands of persons, and thus timely warning of the onset of this terrible disease is given us, when means may be taken to eradicate the bacillus and to cure its ravages. Forty years ago we did not study bacteriology or antiseptics; now they are our mainstay in everyday practice.

It is impossible here to follow the treatment adopted in all the important sanatoria for consumptives throughout the world, so I will content myself with bringing before you that of a few of the best-known institutions of Europe and America.

The institutions I propose to deal with specially are the Adirondacks Sanatorium, Dr. Turban's at Davos, Falkenstein, Goerbersdorf, Hohenhonnef, Reiboldsgrün, Nordrach, and St. Blasien.

ADIRONDACKS.

The Adirondacks Cottage Sanatorium was founded by Dr. E. L. Trudeau in 1884. It is situated in a pine forest in the Adirondack Mountains, in the State of New York, U.S.A., about a mile and a-half from the village of Saranac Lake. It is about twelve to thirteen hours' journey by rail from the city of New York.

According to Dr. S. E. Solly ("Medical Climatology," page 211), the climate of this region shows the temperature to be low and steady during the entire winter. Much snow falls, and there are many windless, cold, snowy days. Rain and snow storms are frequent, although they are said to be less severe than on the coast. Snow lasts from the middle of November to the middle of March or April, varying in different seasons. The soil is porous and dries quickly.

The sanatorium consists of a number of cottages, which accommodate from two to ten persons each, but the average number is four or five. Each patient has his own separate bedroom, opening into a common sitting-room, which in turn communicates with the veranda. This veranda is sheltered from the winds by means of a single glass screen. The partitions between the sleeping and sitting rooms reach but 7 feet from the floor, an arrangement which gives the patient the benefit of the entire air space of the cottage, and allows of its being heated by a single fireplace or stove situated in the central sitting-room; but the walls which separate the sleeping-rooms from each other reach to the ceilings, and are of solid construction (Trudeau: *Practitioner*, February, 1899, page 134).

Dr. Trudeau considers that the principal aim of the modern sanatorium treatment of tuberculosis (phthisis) is to improve the patient's condition and increase his resistance to the disease by placing him under the most favourable environment obtainable. The main elements of such an environment are an invigorating climate, an open-air life, rest, coupled with the careful regulation of the daily habits, and an abundant supply of nutritious food, with the exhibition of such restoratives and tonic measures as may be indicated in each case.

The line of treatment consists in rest out of doors in all weathers, the patients being well wrapped up. Constant exposure at all temperatures, and in all weathers. The patient, besides being warmly clad, is protected from the wind by glass or wooden screens placed on the exposed side only; enclosures not being allowed. This is the treatment adopted in ordinary cases, but, of course, some feverish cases must of necessity remain indoors at certain times Abundance of nourishing food is given to the patient. The food consists chiefly of eggs, meats of all kinds, milk, butter, vegetables, cereals, and fruit. Three good meals a day, with an extra meal in the evening, if desired, are given to all non-febrile patients. Milk is provided at any time the patients will take it.

Alcohol is never prescribed in early cases as a part of the treatment.

Drugs are not used except when specially called for. Cod-liver oil, hypophosphites, and arsenic are used; creosote in small doses only when there are special indications. Tuberculin has been used in a few selected cases.

Dr. Trudeau concludes, as a result of his experience gained during many years in the practical application of sanatorium methods in the treatment of pulmonary phthisis: "Tuberculosis, if detected in its earlier stages, is curable in a large proportion of cases. It is of vital importance, therefore, that the diagnosis be made early. The best results in treating incipient tuberculosis are obtainable by the open-air treatment in special sanatoria situated in good climates. The best plan for construction for such sanatoria is the cottage plan, or some of its modifications."

FALKENSTEIN.

The well-known Falkenstein sanatorium was opened in 1876, under the direction of Dr. Dettweiller. It is situated on the southern slopes of the Taunus Mountains, a few miles from the city of Frankfurt. Among forests of beech, oak, and other trees, it lies about 1,300 feet above sea-level. The climate, though cold and foggy at times, is free from wind and dust, and there is no sudden fall of temperature at sunset.

The sanatorium consists of a main building, with two villas connected by means of covered corridors. There are also several small pavilions in the grounds, some of which can be turned away from the wind.

The treatment consists in fresh air, rest, and plenty of good nourishing food.

The patients pass their day somewhat in the following manner:—On rising they are rubbed down by an attendant, after which they assemble for breakfast from 7 to 8 in summer, or 7·30 to 8·30 in winter. Then they lie down on the balcony, wrapped up in shawls and rugs, till 10 o'clock, when the second breakfast is served. Then rest again till dinner time (1 p.m.). Again rest in the open air. Supper is served at 7 p.m. After this the patients lie in the open air until about 9·30, when they retire for the night. The early and second breakfasts consist of hot or cold milk, coffee, rolls and butter. Dinner consists of thick nourishing soup and four or five courses. At 4 o'clock the patients take milk on the veranda. Supper consists of soup and two courses. At 9 p.m. milk is again served. Consumptives are encouraged to drink large quantities of milk, which comes from the dairy in the establishment. Beer, wine, and brandy (usually undiluted) are allowed.

The patients are thoroughly overhauled on their arrival at the sanatorium; they are afterwards examined once a month, but a daily inspection is made by one of the resident medical men. The temperatures are taken four times daily.

GOERBERSDORF.

"Goerbersdorf," said Dr. Weber in his Croonian lectures on hygienic and climatic treatment of chronic pulmonary phthisis, 1885, "is the cradle of the mountain health resorts and of the hardening open-air treatment in Europe, with a judicious mixture of hydro-therapeutics. Dr. Hermann Brehmer has the merit of having introduced this combination in a systematic manner, coupled with the strict supervision of the patients essential in the hygienic as well as therapeutic management."

Goerbersdorf is a village in the valley of the Steine, in Prussian Silesia. In this valley in 1859, Dr. Brehmer opened the establishment which bears his name, and which for thirty years was under his personal supervision. It lies 1,840 feet above the sea-level, in a park of 300 acres. The grounds are wooded, and some nine miles of level and graduated walks are laid out. There are also summer houses and seats in the open air for the benefit of those who can take exercise.

The buildings consist of a main structure with a number of smaller isolated villas.

All patients take large quantities of milk, those with gastric complications taking kefir. There are five meals a day, at about 7·30, 10·0, 12·30, 4·0, 7·0. Some rest; others take walking exercise in the open air between the meals. Wine is allowed, and occasionally beer, or brandy diluted. Douches or cold frictions are a part of the treatment. Febrile patients take a glass of milk or kefir every hour.

DAVOS.

Dr. Turban's Sanatorium at Davos (founded in 1887) is situated in a valley, in the mountains in the Canton des Grisons, in Switzerland, about 5,000 feet above the level of the sea. Dr. Solly describes the climate here in the winter as very cold, with a few hours of sufficient sunshine to allow of sitting out in the sun's rays; but it is usually freezing in the shade, and at night the mercury falls far below the freezing point. The night air, while cold, is dry, and invalids usually sleep with their windows open. Mr. A. W. Waters ("Some Meteorological Conditions of Davos," 1890) gives the mean annual temperature for twenty-one years as 38° Fahr.; spring, 35°; summer, 52°; winter, 23°. There is an absence of wind, fog, and dust.

The buildings consist of four villas connected with one another by means of covered passages.

Dr. von Jaruntowsky ("Sanatoria for Consumptives") describes the treatment as follows:—"The method of treatment follows the lines laid down by Brehmer and Dettweiller. Febrile patients lie down for the most part in bed close to open windows, while the mild cases spend the whole day either walking in open air or lying about on the terrace. The catering for the patients is very good, and resembles that of Falkenstein. The 'Jour Medical' of an ordinary patient would be represented somewhat as follows:—Early ablutions or cold friction or douche; between 7·30 and 8·30, first breakfast; then a walk on the hills till lunch time (10·30); after lunch a second walk till 11·30; from 11·30 to 1 o'clock, rest on terrace; at 1 o'clock, dinner; after that, rest on terrace till 4 o'clock; at 4 o'clock, afternoon tea followed by a walk till 6 o'clock; rest from 6 o'clock till 7 o'clock; supper at 7 o'clock; after supper, rest on the terrace again till 10 o'clock, when everyone retires to bed."

REIBOLDSGRÜN.

Dr. Driver's Sanatorium at Reiboldsgrün, in Saxony, is situated at an altitude of 2,300 feet above sea-level.

It consists of a number of large buildings connected by covered corridors. The treatment is on the lines advocated by Brehmer and Dettweiller.

ST. BLASIEN.

This sanatorium is situated in a well-wooded spot in the Black Forest. The general methods of treatment are similar to those of the other German sanatoria.

HOHENHONNEF.

The Hohenhonnef Sanatorium is the largest in Germany. It is situated near Honnef, about 774 feet above sea-level, and 500 feet above the Rhine. The climate is mild and sunny; and the grounds, pine-wooded chiefly, cover nearly 100 acres.

The institution is a five-storied building, with two wings somewhat after the style of that at Falkenstein. There is a large fresh-air gallery which is protected from the wind by glass screens, and which can be cooled in summer by streams of water on the roof. The methods of treatment and the dietary are somewhat similar to those at Falkenstein.

NORDRACH.

Nordrach is situated some 1,500 feet above sea-level, in the Black Forest, about thirty miles from Strasburg. It is surrounded by forests of pine trees, and comprises about eighty acres of ground. The sanatorium was established in 1889 by Dr. Otto Walther.

The climate is described (*British Medical Journal*, 30th April, 1898) as "variable, with severe frosts, snow, gales of wind occasionally, mists and dull weather in winter, and almost tropical showers and thunderstorms in summer, but there is a large proportion of bright, sunny days throughout the year, and the air is unusually still, of course exceedingly pure and fresh, and decidedly humid."

The grounds are laid out in walks both on the level and graded, with seats at intervals, but no covered walks as in many of the other sanatoria, and few shelter huts. The buildings are detached, and are of two floors.

The treatment adopted is similar to that at other institutions, with the exception that the patient, be his malady acute or chronic, febrile or non-febrile, is encouraged to pass his life in the open air.

Dr. Walther's principles of treatment are as follow:—

1. An absolutely open-air life for every class of case, in all weathers and seasons, by day and by night.
2. A regular course of adequate feeding (often over-feeding from a patient's point of view), with rich and varied diet, including much meat and milk, and more fatty and carbohydrate food (and little alcohol) given in large meals with long intervals.
3. A judicious combination of exercise on the hillsides, carefully regulated and carried out, so as always to fall short of dyspnœa and fatigue, and so never to interfere with the healing of the lungs, along with the maximum amount of mental and physical rest, forming altogether a gentle training for the lungs and heart.
4. Most important: The co-ordination of all these details, so as to induce regular habits of life in each case by the personal supervision of the physician, and, if necessary, the tactful but firm enforcement of the same.—*British Medical Journal*, 30th April, 1898.

According to Dr. R. M. Smyth (*British Medical Journal*, 30th April, 1898), one of the resident physicians, three meals are given in the day. Breakfast, at 8·30 a.m., consists of coffee, bread and butter, and cold meat, such as ham, tongue, sausage, &c.; dinner, at 1 p.m., consists of two hot courses of meat, or fish and meat, served by the doctor to each patient, according to his individual wants, with an abundance of potatoes and green vegetables and sauces, of which butter is a main ingredient. The third course may be pastry, farinaceous pudding, fruit, or ice-cream, with coffee. Supper, at 7 p.m., is usually one hot course, as at dinner, and one cold, as at breakfast, with tea. At each meal the patient has ½ litre of milk, as soon as possible reduced to ¼ litre, or stopped altogether, according to his capacity and need for putting on flesh. The two latter meals present no option in the matter of diet; all are taken under the doctor's supervision, and servants may not take away the plates until everything has been eaten or the doctor has given permission. Fever cases in bed have the same diet, with, if anything, slightly larger portions. Patients always rest in their couches from 12 mid-day to 1 p.m., and from 6 p.m. to 7 p.m. before meals. On this plan everyone gains weight, often 2, 3, and even 4 lb. a week, for the first few weeks; afterwards gaining by smaller increments. It sometimes happens that patients are sick during or shortly after a meal; if the former happens they come back and finish, usually finding no difficulty with the remainder. The rest before meals and the long intervals between aid complete assimilation very much; and dyspepsia is on the whole by no means common, disappearing as nutrition and strength improve.

THE LEADING FEATURES OF SANATORIA.

From the foregoing descriptions of some of the world-famed sanatoria for consumptives, we find that they possess many features in common.

They are mostly situated in valleys, among hills, and in districts where the native inhabitants are relatively immune to tuberculosis. Away from large towns or frequented public roads, they are free from impurities, organic or inorganic, in the atmosphere. The climate is fresh and bracing, and the air comparatively dry. The patients are for the most part well protected against cold or wind, while there is sufficient fine weather to allow of almost continuous exposure out of doors. The soil is well drained and dry. The buildings are well ventilated, with open balconies and sheltered verandas, and the institutions are under the immediate supervision of medical men.

The area of the grounds varies from 14 acres at Falkenstein to 300 at Goerbersdorf. Reiboldsgrün has 190 acres; Hohenhonnef, 100; Nordrach, 80; Adirondacks, 45; Davos (Turban's), 17. A large area of ground is a decided advantage. Shaded, graduated, and level walks, open-air shelters, &c., are a necessity.

In some of these institutions prolonged rest, in others active exercise, are the main features. We cannot, I fear, say that all consumptives would be benefited by either the one or the other exclusively.

The diet is liberal in all. Milk is the most important article of food. In some homes the patient must take food at short and regular intervals throughout the day; at others fewer but larger meals are given.

Drugs are used sparingly in ordinary treatment. Cod liver oil, hypophosphites, and creosote are prescribed when specially indicated.

Alcohol is given freely in some, and is forbidden in others. As a general rule, it is allowed in some form or another.

RESULTS.

There is no doubt that the sanatorium treatment of consumption does, on the whole, show excellent results.

At Dr. Brehmer's Sanatorium, at Goerbersdorf, 5,440 patients were treated during the period of 1876-86. Of these no detailed reports are available in 408, leaving 5,032, which may be tabulated according to Dr. Brehmer's notes as follows:—

Stage of Disease.	Number.	Cured.	Nearly Cured.	Total.
I.	1,390 = 27·62%	387 = 27· 8%	430 = 31·0%	817 = 58·8 %
II.	2,225 = 44·21%	152 = 6·83%	325 = 14·6%	477 = 21·43%
III.	1,517 = 28·17%	12 = 0·84%	33 = 2·3%	45 = 3·14%
	5,032	551 = 11%	788 = 15·6%	1,339 = 26·6%

In 1890 special inquiries were made, in order to ascertain how long the cure lasted in the case of those discharged from the institution as cured or nearly cured. The results showed that in five cases the cure had lasted for twenty to twenty-nine years, in 52 cases from twelve to twenty-one years, in 38 cases from seven to twelve years.

At the Falkenstein Sanatorium 132 out of 1,022 patients were discharged as cured = 13·2 per cent.; 110 as nearly cured = 11·0 per cent.; giving a total of 242 = 24·2 per cent.

In Dr. Haufe's Sanatorium, at St. Blasien, corresponding inquiries into the fate of 324 patients, who had been treated in the sanatorium during the years 1878 to 1889, showed that, of 288 about whom information could be obtained, 72 were living and in good health from three to twelve years later, 201 had suffered more or less from pulmonary catarrh from two to twelve years without, however, having to suspend their ordinary business; in twelve cases the health got worse, and five patients had died.

The statistics from Reiboldsgrün (Driver's) gave the following results of 2,000 cases:—Cured, 13·66 per cent.; materially improved, 28·02 per cent. unimproved, 25·20 per cent.; and 4·52 per cent. died.*

At Hohenhonnef the absolute cures are said to be 14·5 per cent., and relative cures 28·9 per cent. At Davos (Turban's) the results show 40 per cent. of cured and 40 per cent. improved. At Nordrach the cures amount to 30 per cent., and the improved cases 65 per cent.†

The results at the Adirondacks Sanatorium are given by Dr. Trudeau‡ in the following table. They are for 203 patients, who remained an average of nine months during the period 1897-98:—

Condition of Patients when Admitted.	Apparently Cured.	Disease Arrested.	Improved.	Unimproved or Failed.	Died.
75 Incipient cases	55	16	2	2	0
84 Advanced cases	15	38	19	11	1
44 Far advanced cases ...	0	7	19	13	5
Total (203 Cases)	70	61	40	26	6

Dr. Trudeau defines "Incipient" as cases in which both the physical and rational signs point to but slight local and constitutional involvement. "Advanced" cases in which the localised disease process is either extensive or in an advanced stage, or where, with a comparatively slight amount of pulmonary involvement, the rational signs point to grave constitutional impairment or to some complication. "Far advanced cases": Those in which both the rational and physical signs warrant the term. "Apparently cured": Cases in which the rational signs of phthisis and the bacilli in the expectoration have been absent for at least three months, or who have no expectoration at all, any abnormal physical signs remaining being interpreted as indicative of a healed lesion. "Arrested": Cases in which cough, expectoration, and bacilli are still present, but in which all constitutional disturbance has disappeared for several months, the physical signs being interpreted as indicative of a retrogressive or arrested process.

CONCLUSION.

"It is the duty of mankind to overcome tuberculosis, just as scurvy has been overcome," said Virchow, fifty years ago; and there is much food for reflection in these words. Is not the open-air treatment worthy of serious trial in Australia? This treatment is not new, for nearly eighteen centuries ago Celsus advised consumptives to "take a cow, go into the mountains, live on the milk, and be in the open air."

Can we not in many of our colonies find suitable sites for the erection of homes for consumptives? Great tracts of land, accessible to the large cities and towns, are available, where the air is fresh, dry, and free from pathogenic organisms, where exercise can be taken or rest secured in the open air, and where the soil is dry and well drained.

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* For the above figures I am indebted to Dr. von Jaruntowsky's work, "Sanatoria for Consumptives."

† Knopf: "Prophylaxis and Treatment of Pulmonary Consumption," Philadelphia, 1899.

‡ *Practitioner*, February, 1899.

I cannot take up the time of this meeting in dealing any further with the methods of treatment which I advocate; but if I have by means of this paper aroused interest in the movement going on in the various Australasian Colonies for the establishment of homes and hospitals for phthisical patients, I shall feel amply rewarded for the time and trouble which I have expended on the subject.

DISCUSSION.

Dr. Herbert Horrocks,
Perth.

Mr. Chairman and Gentlemen,—Let me first congratulate Dr. Mullins on his excellent opening address on the "Sanatoria Treatment of Consumption." In a concise and able manner, he has brought before this meeting the extremely interesting and important subject of "What to do with our consumptives"; and we, the representatives of the Australasian medical world, would do well to devote our earnest and serious attention to this matter, since among the civilised nations of the Northern Hemisphere the "fiery cross" has already passed through the length and breadth of their lands, and "war" has been declared against that terrible and insidious foe, the bacillus tuberculosis. If we glance for a few moments at the history of the treatment of the dreaded disease consumption during the past century, we are able to group it into several epochs.

Firstly.—That of apathy and neglect, dating from the beginning of the century to about the year 1856, during which time there was no special hospital treatment for this type of disease, notwithstanding that the annual mortality was fully one-sixth of the total annual mortality from all causes.

Secondly.—The epoch during which the special hospitals, such as the Brompton Hospital, London, and similar hospitals, were established in various parts of the motherland for the separate treatment of this disease under more suitable conditions than could be afforded in the ordinary hospitals, where, indeed, very often these patients were refused admittance. The establishment of the hospitals at Ventnor and at Bournemouth was a still further advance, since fresh air and a suitable locality were then recognised as important factors in the treatment of consumption. I had the excellent opportunity, during my tenure of office as house physician and resident medical officer at Brompton Hospital, of examining and treating many thousands of consumptive patients, and the opinion I formed whilst still attached to the hospital in 1893 was, that although I believed that institution to be the best of its kind in the treatment of pulmonary tuberculosis, yet I felt that we were severely handicapped in combating the insidious tubercle, not only on account of the locality of the hospital (in a well-populated suburb of London, where fogs are by no means unknown), but more especially by the conditions under which the patients were treated, owing to the restricted area of ground in which they could take open-air exercise.

Thirdly.—The sanatorial treatment of consumption, which gradually, and at first but slowly, made headway and established itself in public favour, chiefly owing to the increased knowledge derived from the climatic treatment of pulmonary tuberculosis by our *confrères* both in England and on the Continent. As a result, Goerbersdorf, Falkenstein, Hohenhonnef, and, lastly, Nordrach were established by medical men especially interested and endowed in the treatment of consumption. Now, just as I believe the Brompton Hospital to be the best institution of its kind, looked at from the point of view of a hospital, so I judge the Nordrach Sanatorium to be the most suitable form of "open-air sanatorium," and we would do well to glance at the statistics in these institutions. In both, patients suffering from phthisis are received "of all stages," and yet it will be admitted, I think, that, owing to the comparative inaccessibility of Nordrach, fewer advanced cases of consumption must make their way thither than is the case with Brompton. Another avenue through which error may creep in, when comparing these institutions, has reference to the different method employed in the compilation of their statistics. For instance, you will notice that in all the German sanatoria the cases are grouped under the two heads "cured" and "nearly cured." Now, although I admit that some of them may have become "cured," yet our knowledge of this disease is such that we should doubt the existence of a "cure" until at least the absence of several years from the establishment of an "apparent cure." My own opinion is that no patient can be adjudged to be "cured" of consumption until there has been a complete arrest of the symptoms of the disease for at least three years. At Brompton Hospital, on the other hand, the statistics are compiled in a different manner—viz., those who "improved" and those who "did not improve"; the chief sign of improvement—and a somewhat fallacious one, I admit—being the gain in weight of the patient during his stay in the hospital. Notwithstanding these "errors in comparison," we shall still find, I think, that there is overwhelming evidence in favour of the "open-air treatment in suitable sanatoria"

——	Cases.	Arrest or Cure.	Much Improvement or Distinct Relief.	Stationary.
First Brompton Report	535	5·6 per cent.	22·8 per cent.	36·0 per cent.
Second Brompton Report ...	6,001	4·1 ,,	15·0 ,,	49·5 ,,

Looking at the results shown in this table, we must all feel the comparative hopelessness of treating consumption in hospitals, however well managed, since in only one-fourth or one-fifth of the cases did any improvement or "cure" occur. If we now glance, however, at the statistics obtained from Nordrach's Sanatorium, we shall find that the "cured" are stated to amount to 30 per cent., and the "improved" to 65 per cent., or, in other words, 95 patients in every 100 are better in health and strength after residence in this sanatorium; and the same story is told by the other sanatoria, though to a less extent, at Goerbersdorf, Hohenhonnef, and Falkenstein. My old friend and teacher, Dr. C. T. Williams, in a lecture published in the *British Medical Journal*, 21st May, 1898, on "The Open-air Treatment of Pulmonary Tuberculosis as practised in German Sanatoria," is of the opinion that, notwithstanding the inclemency of the climate in the United Kingdom, there is no reason why sanatoria for upper class patients should not be established there (and since this opinion was expressed there have been a good many such sanatoria instituted, and their number is rapidly increasing). He, however, avers that the rigid discipline of the German sanatorium will be unnecessary in the case of English and American patients, and looks more favourably on the open-air treatment being carried out "in country houses," or even "suburban ones," by the establishment of a patient's couch on a covered veranda, balcony, or tent, and in this manner obtaining the simpler details of the Liegenhalle. Our Australian homes are to a much greater extent adapted to this "home open-air treatment," owing to the almost universality of verandas and the less congested condition of the suburbs of our larger cities and towns.

Why should not we all attempt to carry out this treatment in the homes of our patients, and then compare the results at the next Medical Congress? Speaking now more particularly of the climate of Western Australia, I am firmly of the opinion that the general excellence of the open-air conditions of life in the greater part of this colony must become appreciated before many years have elapsed, and its great suitability in the treatment of pulmonary tuberculosis by the "open-air method" will be thoroughly recognised and appreciated. In conclusion, we all must be fully convinced as to the necessity of treating the poor consumptives, and those whose means will not admit, or those whose surroundings in life are so adverse, in well-regulated open-air sanatoria, "away from the dust and din of city skies"; and the essentials of this treatment are as clearly indicated by Dr. Mullins—(*a*) Fresh dustless air; (*b*) Over-feeding; (*c*) Rest, combined with supervised exercise; (*d*) Absolute obedience to the directions of skilled medical attendants.

What is the most feasible plan in Australia for achieving this most desirable result?

My short experience of Australia has convinced me that public and private charitable subscriptions will be quite insufficient to provide for and maintain a sufficient number of sanatoria in the different colonies; and I, therefore, beg to suggest to the notice of the medical brethren present here to-day the advisability of following the example of Germany in this economical movement.

Thus as an outcome of an insurance law passed in 1889, compelling every German with an income of £150 or less to insure himself against sickness and old age, the number of persons actually insured last year amounted to 11,200,000. The insurance companies found that quite one-half of the applications for relief came from people suffering from phthisis, and they further found that the most economical plan to meet their demands was by providing sanatoria for them. Therefore, gentlemen, let me urge upon you the importance of coming to some definite and practicable result to your discussion on this subject; and in suggesting this plan that these sanatoria should be built and maintained either by our great Australian insurance companies or by the State when "State insurance" becomes *un fait accompli*, I feel convinced that we have found the only practicable and tangible solution of the difficulty of "What to do with our consumptives."

INDEX.

PAGE.
Acute Yellow Atrophy of the Liver, Notes on—A. Jarvie Hood, M.D. (Sydney) ... 79
Adjournment of General Meeting ... 29, 60
Allantoic Cyst, A Case of—R. D. Pinnock, M.D. (Ballarat) ... 275
Amœbic Dysentery and Amœbic Abscess in Australia—J. L. T. Isbister, M.B. (Sydney) ... 82
Appendicitis, General Discussion on ... 210
Army Medical Corps—Recommendations from Public Health Section ... 61
Army Medical Services—W. L'Estrange Eames, M.B. (Newcastle)... 471
Aural Vertigo—J. Lockhart Gibson, M.D. (Brisbane) ... 341

Bacteriological Demonstrations ... 63
Ball given by President ... 54
Barnard, C. E., M.D. (Hobart): Notes on Cases of Preventable Blindness ... 365
Barnett, L. E., F.R.C.S. (Dunedin): Some Observations on Renal Surgery ... 25
Bi-focal Lenses—W. Kent Hughes, M.B. (Melbourne)... 301
Brady, A. J., L.R.C.S.I. (Sydney): Macewen's Burrs... 333
Brain Fever of the Drama, The—T. Hope Lewis, M.R.C.S. (Auckland) ... 92
Bubonic Plague: One of the Earliest Recorded Epidemics (1141 B.C.)—Frank Tidswell, M.B. (Sydney), and J. Adam Dick, M.D. (Sydney) ... 130
Byrne, W. S., M.D. (Brisbane): Rupture in Early Ectopic Gestation ... 419

Cancer of the Cervix Uteri from a Preventive Point of View—Constance Stone, M.D. (Melbourne)... 423
Cancer of the Stomach, The Prevalence and Ætiology of—C. L. W. Hunt, M.B. (Warwick) ... 269
Carcinoma, A Case of Omental—J. R. Purdy, M.B. (The Hutt, N.Z.) ... 94
Carcinoma, Recurrent, of the Breast treated with Thyroid Extract—Jas. P. Ryan, L.R.C.S.I. (Melbourne) ... 254
Chapple, W. A., M.D. (Wellington): The Fertility of the Unfit ... 474
Charitable Institutions, Abuse of—Recommendation from Public Health Section... 61
Church Services ... 21
Cicatricial Contraction of the Orbit and Restoration of the Fornix Conjunctivæ for the Retention of a Prothesis—T. K. Hamilton, M.D. (Adelaide) ... 348
Clendinnen, F. J., M.D. (Melbourne): Notes on Skiagraphy ... 227
Clinical Value of the Serum Reaction in Typhoid, together with some Notes on the Use of Dead Cultures, and on Anti-Typhoid Inoculations—W. H. Goode, M.D. (Sydney), and J. L. T. Isbister, M.B. (Sydney) ... 138
Clubbe, C. P. B., M.R.C.S. (Sydney): The Surgical Treatment of Perityphlitis... 201
Committees of Congress ... 4
Complicating Infections—A. C. F. Halford, M.D. (Clermont) ... 73
Compulsory Notification and Isolation of Infectious Diseases—Recommendation from Public Health Section ... 61
Congress—General Meeting of ... 27
  Adjourned General Meeting of ... 60
  Special Meeting of... 51
  Next Session of ... 62
  President-Elect of ... 62
Conical Cornea—H. Lindo Ferguson, M.D. (Dunedin) ... 347
Conservative Gynæcology—Joseph Foreman, M.R.C.S. (Sydney) ... 383
Consumption, The Sanatorium Treatment of—G. Lane Mullins, M.D. (Sydney) ... 513
,, Discussion on ... 520
Contagious Diseases Act ... 29, 60
Conversazione (Scottish), given by Rev. T. Nisbet ... 59
Cystitis, Some Forms of—A. A. Lendon, M.D. (Adelaide) ... 223

PAGE.
Daily Proceedings of Congress ... 27
Davenport, A. F., M.B. (St. Kilda): Notes on the Origin of Sex ... 123
,, ,, ,, Case of Complete Hermaphroditism ... 423
Dental Section, Discussion *re* ... 28
Dick, J. Adam, M.D. (Sydney), and Frank Tidswell, M.D. (Sydney): Bubonic Plague; One of the Earliest Recorded Epidemics (1141 B.C.) ... 130
Digital Dilatation of Os Uteri in Labour—J. R. Purdy, M.B. (The Hutt, N.Z.) ... 385
Diphtheria Mortality of the Three Principal Australian Colonies for the past Fifteen Years with Special Reference to the Influence of Antitoxin on the Death Rate—A. Jefferis Turner, M.D. (Brisbane) ... 458
,, Some Points in the Treatment of—J. Macdonald Gill, M.D. ... 69
,, The Treatment of—A. Jefferis Turner, M.D. (Brisbane) ... 114
Discussion, General, on the Treatment of Typhoid Fever ... 143
,, Appendicitis ... 210
,, Inguinal Hernia ... 249
,, Eyesight in School Children under Colonial Conditions ... 296
,, Muscular Insufficiency ... 311
,, Sight-testing in the Railway and Marine Services ... 312
,, Prone Position in Operating for Post-nasal Growths ... 330
,, Surgical Treatment of Uterine Fibro-myomata ... 402
,, The Saving of Blood in Gynæcological Operations ... 410
,, Double Uterus ... 414
,, Ventro-fixation of the Uterus ... 417
,, Ectopic Gestation ... 421
,, Isolation of Infectious Diseases by Local Authorities ... 457
,, The Use and Abuse of Hospitals in Queensland ... 469
Double Uterus, Two Cases of—R. D. Pinnock, M.D. (Ballarat) ... 412
Dunwich Asylum, Visit to ... 64

Eagle Farm Meatworks, Visit to ... 64
Eames, W. L'Estrange, M.B. (Newcastle): Army Medical Services ... 471
,, ,, ,, Reform in Quarantine ... 490
Ectopic Gestation, Rupture in Early—W. S. Byrne, M.D. (Brisbane) ... 419
,, ,, Discussion on ... 421
Eden, D. R. (Brisbane): A Case of Odontome ... 287
Empyema, Some Notes on—G. T. Howard, M.D. (Melbourne) ... 136
Excretion of Urinary Water and Urea from the Human Body by diminished Kidney Weight—J. B. Nash, M.D. (Wallsend) ... 97
Executive Committee ... 4
,, ,, Report of ... 27
Extensive Malignant Disease, A Case of, with Unusual Prolongation of Life—C. E. Todd, M.D. (Adelaide) ... 264
Eye, Ear, Nose, and Throat, Section of ... 290
Eyesight in School Children under Colonial Conditions—H. Lindo Ferguson, M.D. (Dunedin) ... 290

Fenwick, P. Clennell, M.B. (Christchurch): Four Cases of Vasectomy ... 266
Ferguson, H. Lindo, M.D. (Dunedin): Eyesight in School Children under Colonial Conditions ... 290
,, ,, ,, ,, Conical Cornea... 347
Fertility of the Unfit, The—W. A. Chapple, M.D. (Wellington) ... 474
Flashman, J. F., M.D. (Sydney): Fractured Ribs in the Insane ... 483
Flies and their Allies—Public Lecture by Professor Anderson Stuart ... 56
Foreman, Jos., M.R.C.S. (Sydney): Conservative Gynæcology ... 383
Fractured Ribs in the Insane—J. F. Flashman, M.D. (Sydney) ... 483
Friendly Societies, Relation of Profession to ... 51

Garden Party, given by Dr. Hardie ... 50
,, ,, ,, His Excellency and Lady Lamington ... 56
General Meetings of Congress ... 27, 60
Gibson, J. Lockhart, M.D. (Brisbane): Aural Vertigo ... 341
,, ,, ,, Two Cases of Sub-hyaloid Hœmorrhage ... 359
,, ,, Case of Monocular Diplopia ... 372

PAGE.
Goode, W. H., M.D. (Sydney), and J. L. T. Isbister, M.B. (Sydney): The Clinical Value of the Serum Reaction in Typhoid, together with some Notes on the Use of Dead Cultures and on Anti-Typhoid Inoculations 138
Governor's Address 30
,, Garden Party 56

Halford, A.C.F., M.D. (Clermont): Complicating Infections 73
,, ,, The Use and Abuse of Hospitals in Queensland ... 465
Hamilton, T. K., M.D. (Adelaide): Cicatricial Contraction of the Orbit and Restoration of the Fornix Conjunctivæ for the Retention of a Prothesis... 348
,, ,, ,, Secondary Cataract 353
Hardie, Dr., Garden Party given by 50
Hermaphroditism, Case of Complete—A. F. Davenport, M.B. (St. Kilda) 423
Hinder, H. V. Critchley, M.B. (Sydney): Supra-pubic and Perineal Cystotomy ... 217
,, ,, ,, The Operative Treatment of Inguinal Hernia 244
Hirschfeld, Eugen, M.D. (Brisbane): The Influence of Hæmorrhage on the Course of Pulmonary Consumption... 109
Hogg, G. Henzé, M.D. (Launceston): Reflex Nasal Neuroses 361
Hood, A. Jarvie, M.B. (Sydney): Notes on Acute Yellow Atrophy of the Liver ... 79
Horrocks, H., M.D. (Perth): Discussion on Sanatorium Treatment of Consumption ... 520
Hospitals, Abuse of—Recommendations from Public Health Section 61
,, Isolation—Recommendations from Public Health Section 61
,, in Queensland, The Use and Abuse of—A. C. F. Halford, M.D. (Clermont)... 465
,, ,, Discussion on 469
Hughes, W. Kent, M.B. (Melbourne): Anatomy and Treatment of Congenital Talipes Equino-varus 280
,, ,, ,, Bifocal Lenses 301
,, ,, ,, Prone Position in Operating for Post-nasal Growths 329
Hunt, C. L. Wolfgang, M.B. (Warwick): The Prevalence and Ætiology of Cancer of the Stomach... 269
Hydatid Disease in Australasia during the Decennial Period 1888-97, A Review of—Robert Scott, M.D. (Ballarat) 156
,, Treatment of, by a Modification of the Closed Method, with Illustrative Cases—John Ramsay, M.B. (Launceston) 182
,, Cysts situated Low in the Pelvis, The Treatment of—W. Moore, M.D. (Melbourne) 190
,, Disease, Discussion on 193

Inaugural Ceremony 30
Infectious Diseases, Compulsory Notification and Isolation of 61
,, ,, Isolation of, by Local Authorities—Gregory Sprott, M.D. (Hobart) 446
,, ,, ,, ,, ,, Discussion on 457
Influence of Hæmorrhage on the Course of Pulmonary Consumption, The—Eugen Hirschfeld, M.D. (Brisbane) 109
Inguinal Hernia in the Young, The Ætiology and Treatment of—R. Hamilton Russell, F.R.C.S. (Melbourne) 233
,, ,, The Operative Treatment of—H. V. Critchley Hinder, M.B. (Sydney) 244
,, ,, Discussion on 249
Isbister, J. L. T., M.B. (Sydney): Amœbic Dysentery and Amœbic Abscess in Australia 82
Isbister, J. L. T., M.B. (Sydney), and W. H. Goode, M.D. (Sydney): The Clinical Value of the Serum Reaction in Typhoid, together with some Notes on the Use of Dead Cultures and on Anti-Typhoid Inoculations 138

Kendall, H. W. Martindale, M.R.C.S. (Wellington): The Ocular Symptoms and Signs of Locomotor Ataxia 367
Kendall, T. Mailler, L.R.C.P.E. (Sydney): Sewerage and Health 496
Kirkland, T. S., M.D. (Sydney): Quinine Amaurosis 334

Lecture, Public, by Professor Anderson Stuart 56
Lendon, Alfred Austin, M.D. (Adelaide): Some Forms of Cystitis... 223
Leper Lazarette, Visit to 64
Lewis, T. Hope, M.R.C.S. (Auckland): The Brain Fever of the Drama 92

PAGE.

Local Secretaries of Congress ... 3
Locomotor Ataxia, The Ocular Symptoms and Signs of—H. W. Martindale Kendall, M.R.C.S. (Wellington) ... 367
Luther, E. L., M.D. (Maryborough): Post-partum Hæmorrhage; its Treatment, Anticipatory and Actual ... 427

Macewen's Burrs—A. J. Brady, L.R.C.S.I. (Sydney) ... 333
McCormick, A., M.D. (Sydney): Presidential Address in Section of Surgery ... 150
Medicine, Section of ... 65
Members of Congress, List of ... 5
,, ,, List of Honorary ... 13
Midwifery and Gynæcology, Section of ... 374
Midwifery Practice in New Zealand, Eight Years'—J. R. Purdy, M.B. (The Hutt, N.Z.) 430
Discussion on ... 434
Monocular Diplopia, Case of—J. Lockhart Gibson, M.D. (Brisbane) ... 372
Moore, W., M.D. (Melbourne): The Treatment of Hydatid Cysts situated low in the Pelvis 190
,, ,, Some Points in the Treatment of Severely Crushed Limbs ... 195
Mullins, George Lane, M.D. (Sydney): The Sanatorium Treatment of Consumption ... 513
Muscular Insufficiency, Some Remarks on—F. Antill Pockley, M.B. (Sydney) ... 303
,, ,, Discussion on ... 311

Nash, J. B., M.D. (Wallsend): Excretion of Urinary Water and Urea by diminished Kidney Weight ... 97
Nyulasy, F. S., M.B. (Melbourne): Pseudo-Diptheritic Endometritis Vaginalis ... 380

Odontome, A Case of—D. R. Eden (Brisbane) ... 287
Œdema in Infants, not of Renal Origin—W. A. Wood, M.D. ... 78
Office-bearers of Congress ... 2
Omental Carcinoma, A Case of—J. R. Purdy, M.B. (The Hutt, N.Z.) ... 94
Origin of Sex, Notes on the—A. F. Davenport, M.B. (St. Kilda) ... 123
Orr, A. W., M.D. (Brisbane): Strabismus ... 345
O'Sullivan, M. U., L.R.C.P.E.: Presidential Address in Section of Midwifery and Gynæcology ... 374

Patrons of Congress, List of ... 1
Papers Contributed to Congress ... 15
Picnic given by Queensland Branch B.M.A. ... 64
Pinnock, R. D., M.D. (Ballarat): A Plea for Legislation to Empower Medical Practitioners to make *Post-mortem* Examinations in Cases of Doubtful Diagnosis ... 495
,, ,, ,, ,, A Case of Allantoic Cyst ... 275
,, ,, ,, ,, A Successful Case of Direct Injection of Strychnine into the Wall of the Left Ventricle of the Heart of a Patient apparently moribund during Anæsthesia 277
,, ,, ,, ,, Two Cases of Double Uterus ... 412
Perityphilitis, The Surgical Treatment of—C. P. B. Clubbe, M.R.C.S. (Sydney) ... 201
Plea for Legislation to Empower Medical Practitioners to make *Post-mortem* Examinations in Cases of Doubtful Diagnosis—R. D. Pinnock, M.D. (Ballarat) ... 495
Post-partum Hæmorrhage; its Treatment, Anticipatory and Actual—E. Luther, M.D. (Maryborough) ... 427
President-Elect of next Session of Congress ... 62
Presidential Address—John Thomson, M.B. ... 33
Presidential Addresses—
  Section of Medicine—R. Scot-Skirving, M.B. (Sydney) ... 65
  ,, Surgery—A. McCormick, M.D. (Sydney) ... 150
  ,, Eye, Ear, Nose, and Throat—H. Lindo Ferguson, M.D. (Dunedin) ... 290
  ,, Midwifery and Gynæcology—M. U. O'Sullivan, L.R.C.P.E. (Melbourne) 374
  ,, Public Health—J. Ashburton Thompson, M.D. (Sydney) ... 435
Presidential Welcome ... 27
Pockley, F. Antill, M.B. (Sydney): Notes on a Case of Segmental Hemi-Achromatopsia 300
,, ,, ,, ,, Some Remarks on Muscular Insufficiency ... 303
Post-nasal Growths, Prone Position in Operating for—W. Kent Hughes, M.B. (Melbourne) ... 329
,, ,, Discussion on ... 330

PAGE.
Premier's Reception on the "Lucinda" ... 58
Premier's Welcome ... 32
President's Ball ... 54
Preventible Blindness, Notes on Cases of—C. E. Barnard, M.D. (Hobart) ... 365
Pseudo-Diphtheritic Endometritis Vaginalis—F. S. Nyulasy, M.B. (Melbourne) ... 380
Ptomaine Poisoning, An Epidemic of—S. J. Richards, M.B. (Mount Morgan) ... 104
Public Health, Section of ... 435
Public Lecture by Professor Anderson Stuart ... 56
Pulmonary Consumption, The Influence of Hæmorrhage on the Course of—Eugen Hirschfeld, M.D. (Brisbane) ... 109
Purdy, J. R., M.B. (The Hutt, New Zealand): A Case of Omental Carcinoma ... 94
,, ,, ,, ,, Digital Dilatation of Os Uteri in Labour 385
,, ,, ,, ,, Eight Years' Midwifery Practice in New Zealand ... 430

Quarantine, Motion *re* ... 62
Quarantine, Reform in—W. L'Estrange Eames, M.B. (Newcastle) ... 490
Queensland Art Society, Reception by ... 50
Queensland Branch, B.M.A., Picnic given by ... 64
Quinine Amaurosis—T. S. Kirkland, M.D. (Sydney) ... 334

Ramsay, John, M.B. (Melbourne): Treatment of Hydatid by a Modification of the Closed Method, with Illustrative Cases ... 182
Reception by Mayor ... 29
,, Queensland Art Society ... 50
,, His Excellency and Lady Lamington ... 56
,, Premier ... 58
Reflex Nasal Neuroses—G. Heuzé Hogg, M.D. (Launceston) ... 361
Reports and Recommendations from—
  Public Health Section ... 60
  Eye, Ear, Nose, and Throat Section ... 62
Renal Surgery, Some Observations on—L. E. Barnett, F.R.C.S. (Dunedin) ... 256
Retinoscopy, On—M. J. Symons, M.D. (Adelaide) ... 316
Retinitis Circinata, A Case of—Jas. P. Ryan, L.R.C.S.I. (Melbourne) ... 327
Richards, S. J., M.B. (Mount Morgan): An Epidemic of Ptomaine Poisoning; 113 Cases ... 104
Ross, E. Fairfax, M.D. (Sydney): A Short Report on Serum-therapy in Fever after Delivery ... 429
Russell, R. Hamilton, F.R.C.S. (Melbourne): The Ætiology and Treatment of Inguinal Hernia in the Young ... 233
Ryan, Jas. P., L.R.C.S.I. (Melbourne): Recurrent Carcinoma of the Breast treated with Thyroid Extract ... 254
,, ,, ,, A Case of Retinitis Circinata ... 327

Sanatorium Treatment of Consumption—G. Lane Mullins, M.D. (Sydney) ... 513
  Discussion on ... 520
Saving of Blood in Gynæcological Operations, The—A. Watson, M.D. (Adelaide) ... 406
  Discussion on ... 410
Scot-Skirving, R. (Sydney): Presidential Address in Section of Medicine ... 65
Scott, Robert, M.D. (Ballarat): A Review of Hydatid Disease in Australasia during the Decennial Period, 1888-97 ... 156
,, ,, ,, Notes of a Case of Traumatic Separation of the Lower Epiphysis of the Tibia ... 277
Segmental Hemi-Achromatopsia, Notes on a Case of—F. Antill Pockley, M.B. (Sydney) 300
Secondary Cataract—T. K. Hamilton, M.D. (Adelaide) ... 353
Sections and Sectional Officers ... 13
Serum-therapy in Fever after Delivery, A Short Report on—E. Fairfax Ross, M.D. (Sydney) ... 429
Severely Crushed Limbs, Some Points on the Treatment of—W. Moore, M.D. (Melbourne) 195
Sewerage and Health—T. Mailler Kendall, L.R.C.P.S. (Sydney) ... 496
Sight-testing in the Railway and Marine Services, Discussion on ... 312
Skiagraphy, Notes on—F. J. Clendinnen, M.D. (Melbourne) ... 227
Special Meeting of Congress ... 51
Sprott, Gregory, M.D. (Hobart): Isolation of Infectious Diseases by Local Authorities 446

PAGE.

Stone, Constance, M.D. (Melbourne): Cancer of the Cervix Uteri from a preventive point of view ... 423
Strabismus—A. W. Orr, M.D. (Brisbane) ... 345
Strychnine, A Successful Case of Direct Injection of, into the Wall of the Left Ventricle of the Heart of a Patient apparently moribund during Anæsthesia—R. D. Pinnock, M.D. (Ballarat) ... 277
Stuart, Professor Anderson, Public Lecture by ... 56
Sub-hyaloid Hæmorrhage, Two Cases of—J. Lockhart Gibson, M.D. (Brisbane) ... 359
Supra-pubic and Perineal Cystotomy—H. V. Critchley Hinder, M.B. (Sydney) ... 217
Surgery, Section of ... 150
Symons, M. J., M.D. (Adelaide): On Retinoscopy ... 316

Talipes Equino-Varus, Anatomy and Treatment of Congenital—W. Kent Hughes, M.B. (Melbourne) ... 280
Thanks, Votes of ... 63
Thompson, J. Ashburton, M.D. (Sydney): Presidential Address in Section of Public Health ... 435
Thomson, John: Presidential Address ... 33
Thring, E. T., F.R.C.S. (Sydney): The Surgical Treatment of Uterine Fibro-myomata 395
Tidswell, Frank, M.B. (Sydney), and J. Adam Dick, M.D. (Sydney): Bubonic Plague; One of the Earliest Recorded Epidemics (1141 B.C.) ... 130
Todd, C. E., M.D. (Adelaide): A Case of Extensive Malignant Disease with Unusual Prolongation of Life ... 264
Traumatic Separation of the Lower Epiphysis of the Tibia, Notes of a Case of—Robert Scott, M.D. (Ballarat) ... 277
Treasurer's Balance-sheet (Fourth Session) ... 28
Turner, A. Jefferis, M.D. (Brisbane): The Treatment of Diphtheria ... 114
,, ,, ,, ,, The Diphtheria Mortality of the Three Principal Australian Colonies for the past Fifteen Years, with Special Reference to the Influence of Antitoxin on the Death Rate ... 458
Typhoid Fever, The Clinical Value of the Serum Reaction in, together with some Notes on the Use of Dead Cultures and on Anti Typhoid Inoculations—W. H. Goode, M.D. (Sydney), and J. L. T. Isbister, M.B. (Sydney) ... 138
Typhoid Fever, General Discussion on tho Treatment of ... 143

Uterine Fibro-myomata, The Surgical Treatment of—E. T. Thring, F.R.C.S. (Sydney) 395
Discussion on ... 402
Uterine Myoma, The Treatment of—Ralph Worrall, M.D. (Sydney) ... 388

Vasectomy, Four Cases of—P. Clennell Fenwick, M.B. (Christchurch) ... 266
Venice Convention—Recommendations from Public Health Section ... 61
Ventro-fixation of the Uterus followed by Pregnancy, Four Cases of—Ralph Worrall, M.D. (Sydney) ... 415
,, ,, ,, Discussion on ... 417
Votes of Thanks ... 63

Watson, Professor A., M.D. (Adelaide): The Saving of Blood in Gynæcological Operations ... 406
W.C.T.U. Petition, urging Repeal of the C.D. Acts ... 59
Wood, W. Atkinson, M.D. (Melbourne): Œdema in Infants, not of Renal Origin ... 78
Worrall, Ralph, M.D. (Sydney): The Treatment of Uterine Myoma ... 388
,, ,, ,, Four Cases of Ventro-fixation of the Uterus followed by Pregnancy ... 415

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